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THE

LONDON MEDICAL GAZETTE,

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WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 31, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton-Street, near St George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XVI.

Diseases of the Pleura; Structure of the Pleura—Acute Pleurisy—Pathological History; Symptoms; Physical Signs—Signs from Increased Sensibility; from Friction; from Effusion; from Supplementary Action—Of the Sound Side—Modifications by Adhesions; Columnar Adhesions; Circumscribed Effusions; Explanation of Anomalous Cases.

Having considered what may be called the aërial or mucous surface of the lungs, and the manner in which it is modified by disease, we now come to the other surface, and the pathological properties of the serous membrane which covers it, and lines the cavity in which the lungs move. This membrane in the healthy state is thin, almost transparent, and possessed of considerable elasticity, inasmuch as it remains smooth and unwrinkled, whether the lung be expanded or collapsed. It consists of two layers—one distinctly serous, which is always bedewed with a serous fluid, lines the cavity of the chest, and forms the outer covering of its organs. The other is clearly fibrous in the costal pleura, and, together with that of the pericardium, seems to be a continuation of the deep-seated cervical fascia. An under layer of the pulmonary pleura has also been described, I believe for the first time, by Dr. Stokes; but whether this be, as he supposes, *fibrous*, or of the same structure as the outer serous layer, appears to me doubtful. It may be something intermediate between the two, for both serous and fibrous membranes consist of condensed cellular tissue, and vary only in condensation and softness, elasticity and rigidity, according to the office of the parts which they cover. The sensibility of the pleura is naturally very low; and there is neither feeling nor irritation caused by this motion of the pulmonary on the costal pleura in respiration; for a constant secretion of serum gives to both a smooth slippery surface, which reduces the friction to the least possible amount. Its blood-vessels are exceedingly fine; indeed, it is denied by some anatomists that the serous membrane itself has any vessels, and they say that those which are visible are in the subserous tissue which unites it to the organ. I cannot subscribe to this opinion, for I am sure that I have seen red vessels on the very surface of the pleura, when inflamed; although these vessels probably conveyed only serum in the healthy state. No doubt the larger and more visible vessels do exclusively belong to the subserous cellular tissue, and we have before noticed that these are derived from both the pulmonary and the bronchial arteries.

It is chiefly these subserous vessels that form the striated patches or points of redness that are seen in the first stage of pleuritic inflammation, and their distension can be felt through the serous membrane, which feels slightly uneven on passing the finger over it. Probably at this period there is a diminution of the serous secretion at the inflamed spot, as

we know such to be the first effect of inflammation in the mucous membranes: the effect of this would be to increase the friction between the surfaces. Very soon, however, the flow of serum is increased, and with it, if the inflammation continue, an albuminous matter, coagulable lymph, is also exuded by the serous membrane. This instance illustrates the simplest form of inflammation. We have the vessels with no compound structure to complicate or modify their action, and we find the increased development of these vessels by inflammation obviously attended by an exaggeration of those secretive functions which they fulfil in health. These functions are two-fold—that of liquid exhalation, and that of solid nutrition. The matter exhaled is serum; the material of nutrition is the albuminous or fibrinous part of the blood. In their natural proportion, these functions preserve the membrane in a healthy state; one merely lubricating its surface with a slightly albuminous fluid, the other nourishing and sustaining the solid matter of the membrane. When moderately increased by slightly augmented vascular action, these properties of the vessels cause hydrothorax and a simple hypertrophy or thickening of the subserous tissue, the most vascular part; and this result we often meet with in an opacity, without adhesions, of the pulmonary pleura or other serous membranes. But when the exhalant and nutrient properties of the vessels are exalted by acute inflammation, there is as it were an overflow of their products; the liquid effusion is rapid and copious; and the excess of the nutritive secretion now appears on the exterior of the membrane in various forms, and either by itself, or mingled with the liquid effusion, constitutes all the different products which are recognized as the result of inflammatory action. In its smallest proportion it is held in solution by the effused liquid, which, on being drawn from the body, or after death, gelatinizes by this fibrinous matter which it contains. When very abundant, this forms films of lymph on the surface of the membrane; and this lymph will generally be more abundant and disposed to speedy organization, when the inflammatory orgasm is strong, and the blood is rich in nutrient matter.

Following still the pathological history of pleurisy, we find in this same lymph the matter of adhesions; but whether or not these adhesions shall take place, will depend on the quantity of liquid effusion between the pleuræ. This effusion will, of course, to a certain degree ponderate to the lowest parts of the chest, and in those parts will keep the pleuræ separate; and if it be the upper portions of the pleura that are inflamed, they will the more readily adhere, unless the liquid effusion be very abundant. But if the pleuræ be inflamed only in their lower portions, a moderate quantity of liquid will be enough to keep them separate; and if the lymph then become organized, it forms, not an adhesion, but a false membrane coating of the lung, which may have further effects in modifying the remains or the products of the previous inflammation. Before we consider these various results of the modifying influence of time, of the degree and kind of inflammation, and of previous disease, on the further pathological history of pleuritic cases, we had better take a view of the symptoms and signs of the more simple and recent forms of pleurisy.

You know well enough the general symptoms of pleurisy, and how characteristic they sometimes are; the sharp cutting pain in the side restraining every common inspiration, and often making the act of coughing or deep breathing almost intolerable; the short breath which consequently results, and the lying on or holding the affected side; the short dry cough; the general inflammatory fever, hard quick pulse, heat of skin, flushed cheek, &c.; but I have had occasion to tell you that extensive pleurisy and its consequences may be present without any of this array of symptoms; that even in cases in which there had been scarcely a suspicion of the presence of disease in the chest, acute inflammation, and its concomitant, copious effusion, had been for many days or weeks occupying the pleura. And further, you may have the signs above described without the presence of pleurisy. Sharp pains of a nervous character not unfrequently closely imitate that of pleurisy, especially in hysterical females; and if they happen to be attended with feverish excitement, the resemblance is perfect. In fact, the greater number of the symptoms which are supposed to be distinctive of pleurisy, depend on a much exalted sensibility of the pleura, which is by no means a necessary accompaniment of its inflammation; and the symptoms of oppressed breathing, proceeding from the pressure of the effusion, may be distinct only when this effusion has accumulated very rapidly.

On the other hand, the physical signs in the greater number of cases are very unequivocal; and although they by no means speak of the degree or of the intensity of the inflammation, they seldom fail to announce its presence, and thus pretty accurately measure its most serious concomitant, the liquid effusion. I will now enumerate the signs of pleurisy in the order in which they commonly occur in the affected side, and we shall then consider the nature and value of each.

1. Diminished motion and sound of respiration, from pain.
2. Sound of friction accompanying the motions of respiration.
3. Dulness on percussion in the most dependent parts of the chest, from the effusion.
4. Diminished motion, and sound of respiration, from the same cause.
5. Ægophony.
6. Cessation of vocal vibration felt by the hand.
7. Cessation of ægophony and all sound of the voice.
8. Enlargement of the side.
9. Displacement of the heart, liver, mediastinum, intercostal spaces.
10. Increased motions and sound of respiration on the sound side.

1. I believe that I formerly mentioned that the respiratory movements are so far within the control of the will, that they may be restrained in parts affected with pain; and it is obvious that the sound of respiration will be diminished in proportion. This has been noticed by M. Andral as an early sign of pleurisy; but you can readily perceive that it is a very equivocal one, since it depends merely on the presence of pain, which, as I have just remarked, may exist quite independently of inflammation. I may mention here, that when you examine the chest of a person who is suffering from pain, or who is shivering under the influence of cold, or of the rigor of a fever, you will often be perplexed by the rumbling sounds produced by the slight convulsive action of the muscles of the chest, particularly of the back, which are sometimes enough to mask the respiratory murmur. You may distinguish them by observing that they are not stopped, but increased by the effort of holding the breath.

2. At the first onset of pleurisy there is sometimes heard a rubbing or creaking sound accompanying the movements of the chest. This may be owing to a certain degree of roughness or defective lubrication of the pulmonary and costal pleuræ at certain points, and when combined with the general symptoms may be considered a pretty exact sign, but it is very transient, and is not often heard. It may be produced, also, by interlobular emphysema, where it lasts for a much longer time. I am inclined to think, from a recollection of the cases in which I have heard this sound, that its production is favoured by the lung being partially distended or pushed against the walls of the chest during their motions; for it is especially apt to occur where the lung is displaced by a tumor, or by a pleuritic effusion confined by adhesions; and where the lung is partially distended with tuberculous or other deposits. In any of these cases you can understand how a portion of lung, being pressed against the ribs as they rise, may occasion a sound of friction; and you may imitate it in the dead body by rubbing a piece of distended lung on the interior of the chest. The friction sound of pleurisy is commonly heard about the middle parts of the chest; it generally ceases as soon as the sound of percussion announces the accumulation of liquid; but in dry pleurisy, and in the cases of partial pressure, just alluded to, it may continue for a long time.

3. In by far the greater number of cases of pleurisy there is an effusion of serum soon after the commencement of the inflammation, and the accumulation of this liquid in the chest is the cause of the signs by which pleurisy can be best distinguished. This fluid will accumulate first in the lowest parts of the chest, floating, to a certain extent, the lung upon it. Hence these parts will sound more or less dull on percussion, whilst the higher parts retain their usual resonance; and to some degree, change of posture, by changing the place of the liquid, will alter the situation of these sounds. As, however, the external vesicular structure yields more readily to pressure than the tubular parts within, the accumulating fluid soon mounts up in the form of a thin layer, between the lung and the ribs, to a considerable height in the chest. This thin layer slightly impairs the sound on percussion; and this more distinctly if the percussion be gentle and abrupt, as by filliping on a finger tightly applied, and comparing the sound, as usual, to that of corresponding parts of the opposite side. This sign, as well as those to be next described, are liable to modifications from adhesions previously existing between the pulmonary and costal pleuræ. These we shall notice afterwards.

4. This same accumulation of liquid must, as you can easily perceive, diminish the extent of the motions of respiration in proportion to its bulk, which has taken the place of the most expansible part of the lung. The sound of respiration will, for the same reason, be weakened, and its duration shortened on the affected side. These signs need no further explanation.

5. About the same time at which the dulness on percussion and diminution of the respiratory murmur reach the middle regions of the chest, the vocal resonance there presents a remarkable modification. It is heard much more distinctly than is usual in those regions, and it is superficial, as if produced in the spot, separately from the oral voice, and changed to a sort of small bleating trembling note, which so

much resembles the voice of a goat, that Laennec has well termed it *ægophony*. This modification of the voice is heard most distinctly between the third and sixth ribs, which corresponds with the situation of the middle sized bronchial tubes; but about the spine it is generally mixed with a louder and more uniform resonance, more of a common bronchophony, from the larger tubes at the root of the lung. Now how is it that a layer of liquid, which pushes the lung aside, can make the voice more audible than usual? Why does not the liquid condense the porous tissue of the lung, and thus make it a better conductor of sound? Then you will understand something of the modified character of the voice, if you consider the nature of the matter which it has to pass through, a thin layer of liquid, which, being thrown into active vibrations by it, trembles and dances in an irregular manner, now checking the sound, now transmitting it with increased force, so that the voice comes through tremulous and wiry. The high tones of the voice are those which are best transmitted in this way; for, as I mentioned to you formerly, the bass tones do not enter the small tubes, but if strong, pervade the whole tissue with a diffused fremitus. Hence you will hear ægophony best in women and children, and those who have high voices. In persons with a bass voice it is more commonly limited to the inferior angle of the scapula, or near the spine, and from its being seated in larger tubes, takes more the character of bronchophony; but even there, if the layer of liquid reach to that height, it will have something of a buzzing and bleating note. As the liquid increases, the ægophony becomes weaker, more distant, and loses much of its flutter or tremor, having now rather the sound of a diminutived deep seated voice, or a silvery echo of the original. This is because the lung is pushed so far away from the walls of the chest, and its tubes so much compressed; and as these circumstances increase, the sound ceases altogether. It is not easy to determine what quantity of effusion is enough to do this; but I am inclined to think that much sound of the voice is not transmitted when the layer of serum exceeds an inch in thickness. If the ægophony continue stationary for several days, it is a proof that the effusion is moderate, and does not increase rapidly, which is a favourable sign; but it is often very transient, and many cases of pleuritic effusion are discovered after they have passed the degree which causes ægophony. Old adhesions, will, however, modify this as well as the other physical signs. When ægophony is most distinct, it is often coupled with bronchial respiration, especially between the scapulæ, where, also, there is a good deal of common bronchophony with it. M. Reynaud, a very good auscultator and able pathologist, has lately confirmed the opinion of Laennec, that ægophony is a kind of bronchial voice modified by its transmission through a layer of liquid. He observed, in a pleuritic case, that the ægophony heard at the lower angle of the scapula when the patient was sitting, became changed to simple and louder bronchophony when the patient lay prostrate or stooped much forward; this change of posture having the effect of permitting the liquid to gravitate to the anterior part of the chest, and floating the lung into contact with the parietes. Ægophony and bronchophony are different enough when their characters are well-marked; but they often present mixed and doubtful varieties that do not admit of such easy distinction. You must be practised in listening to both, before you can easily distinguish them; but, as far as description goes, I would represent the true character of ægophony to be a certain *tremulousness* in the voice when it is superficial, and an echo-like slenderness when it is deep-seated; whilst bronchophony may present many other varieties.

6. An early and very characteristic effect of the accumulation of liquid in the pleural sac, is its intercepting the diffused vibration of the voice, which is usually felt by the hand applied to the chest. I formerly explained to you that this vibration is caused by the voice pervading the common tissue of the lung, and you can readily conceive that a layer of liquid would muffle and destroy it; and it does this even when ægophony may be heard at the same spot, the vibrations of the latter being of too fine a kind to be felt by the hand. You have here also a distinction between a liquid effusion and a solidification of the lung; for the latter transmits the vocal vibrations with unusual force from the tubes. This diagnostic sign we owe to M. Reynaud; and it is the more valuable because it is so easily obtained, even by a person who does not practise auscultation. It must not, however, be always considered as quite conclusive; for there are some exceptions to it, both positive and negative. For example, in case of partial adhesions of the lung to the chest, there may be even more vibration than usual felt at the adhering parts where the lung and its tubes are pressed into close contact with the walls of the chest; and it may happen, on the other hand, in solidification of the lung, that liquid or other obstruction in the bronchial tubes may prevent the voice from being transmitted through them.

7. As the liquid effusion increases, the ægophony and all sound of the voice ceases throughout the affected side, except within two or three inches of the spine, and in spots where the lung may have been adherent, which frequently happens at the upper part of the chest. The sound of respiration is also abolished in most parts of the chest, but never in the interscapular region, and rarely under the clavicle; it is, however, much weaker in these parts than on the sound side, and may probably be only transmitted from that side. I have met with several instances in which the respiration had been audible to the last under the clavicle, yet on examination after death the lung was found entirely compressed against the spine. A word more presently about the influence of adhesions.

8. Enlargement of the affected side is the next of the signs of pleurisy that I have named. As you may suppose, the effusion must be pretty copious to render this enlargement perceptible; but a difference between the two sides may sometimes be seen on inspection of the chest in different periods of respiration, where the quantity of liquid is not very great. The affected side is first seen to be larger at the end of expiration, when it does not diminish equally with the other side, especially in its lower portions. So if you encircle the chest with a piece of tape, fixing it at the sternum and at the spine, you will see it tighten and slacken with inspiration and expiration more obviously on the sound than on the diseased side, which remains more fixed in a state of partial distension. As the effusion increases, the difference is perceptible during the whole respiratory act, and the eye can easily detect the want of symmetry, whether the inspection be made in front, behind, or, as I formerly described to you, from above, looking downwards on the patient's shoulders. To be more exact, however, you must measure the chest, by encircling it this way horizontally with a piece of tape or ribbon, making it meet at the middle of the lower end of the sternum; then taking the tape by the point where it crosses the spinous processes of the vertebral column, thus,—you have at once the measure of two sides to compare together. By the way, you see a considerable discrepancy between the two sides of this statue; it is, nevertheless, an accurate cast of an antique marble; and this shews that the sculpture of the ancients is not always so perfect in accuracy and symmetry as it is said to be. Now in comparing the measurement of the two sides, you must bear in mind that the right side is, in the healthy state, from a quarter to half an inch larger than the left; and you must not trust to slight differences in favour of the right. The enlargement of the side is sometimes discoverable by the eye, as well as by measurement, two or three days after the first attack of the pleurisy; and it does not generally proceed afterwards in proportion to the effusion, until this becomes excessive, and has displaced the adjoining parts to a great extent.

9. Then we have a very important class of signs arising from the displacement of certain of the walls and organs bounding the effusion. Laennec had remarked that the intercostal spaces on the side of the effusion do not present their usual depressions, and are sometimes, especially in chronic cases, even prominent beyond the surface of the ribs. This is scarcely perceptible, however, in acute pleurisy, unless the subject be thin. In such cases I have seen the intercostal spaces not only prominent, but presenting also an evident fluctuation. In looking for this sign you should place the patient obliquely with regard to the light; and you may sometimes perceive it more readily by surveying him from a little distance, as from the foot of his bed, than by a closer inspection.

But we may generally learn more from the displacement of the organs adjoining the effusion, especially that of the heart and of the liver. These displacements had been noticed in empyema by surgical writers; but Laennec does not seem to have paid attention to them, and I believe that we owe to Drs. Stokes and Townsend their proper application to the diagnosis of liquid effusions in the chest. The displacement of the heart by an effusion in the left pleura is the most valuable and easily recognised of these. In this case the pulsations of the hearts are felt and heard mosst distinctly under, or to the right, of the sternum, or in the epigastrium, instead of, as usual, between the cartilages of the fourth and sixth left ribs.

Again, when the effusion is on the right side, you find the liver pushed down below the margins of the ribs, and you can trace its position both by feeling and by its dull sound on percussion. It is sometimes depressed so much as to form a sensible tumor in the abdomen; and I have known more than one case of latent pleurisy, in which this tumor was long supposed to be the chief disease, and the case was treated accordingly. The effusion in the right side, when considerable, also displaces the heart further than usual to the left, and you may sometimes feel it pulsating to the left of the mammilla, and even below the axilla. Dr. Stokes has lately published some interesting observations, with the view to prove that the displacement of

the diaphragm and intercostal muscles is much favoured by a paralysis of their muscular fibres, the result of inflammation in the serous membrane covering them.

The displacement of the mediastinum is to be discovered only by percussion. Situated as this is naturally, in the mesial plane, it divides the two cavities of the pleura at a line down the middle of the sternum, which bone sounds well on percussion from the margins of both lungs, which lie under it. But a copious effusion will push the mediastinum towards the opposite side, and, by occupying the space behind the sternum, will give this bone a dull sound on percussion; and this may extend even half an inch beyond it. This dulness is most perceptible below the juncture of the second rib, for above this the resonance is seldom destroyed, and often has an amphoric character from the great air-tubes, which are not compressed. This is still more remarkable in some cases of adhesion which I shall notice presently.

Now all these displacements may also be produced by an accumulation of air in the cavities of the chest; but you can perceive at once that percussion would give a prompt distinction between these two cases, by the tympanitic sound in case of air, and the dulness in case of liquid.

10. In all cases of physical examination, you must avail yourselves of the standard of comparison which the two sides offer, and you will often find that the sound side will give you not only the negative proofs of the absence of disease in it, which may well be compared with the positive signs of disease on the opposite side, but it will even shew you an exaggeration of the signs of healthy action, in consequence of its work being really increased. Thus you will see the healthy side move more fully and quickly than usual, and the sound of respiration will be increased there in a remarkable degree, so as to resemble the loud respiration of children; wherefore it is called *puerile*.

I might enter into many other particulars respecting these different signs, but we have not time to do so; and indeed, if we had, I would rather leave for your own observation and reflection the less essential details which present themselves in practice. These diagrams, which represent by sections of the chest the position of different quantities of effusion, as I have found them after death, and their effect on the chest and its organs, may serve to impress the physical signs more strongly on your memory. In fig. 1, you see indicated by the shading a moderate effusion on the right side, and the manner in which it displaces a lung unrestrained by adhesions; ægophony is more or less heard from *a* to *b*, and the sound of percussion is impaired, but not dull; but below *b* it is perfectly dull. Fig. 2 represents an abundant effusion on the left side, which compresses the lung against the mediastinum and spine, displaces the mediastinum and heart to the right of the sternum, gives a prominence to the intercostal spaces, and depresses the diaphragm. You would suppose that such a quantity of liquid would render the sound of percussion quite dull on the whole of that side; but it commonly happens that percussion above *a*, gives a sound which is not perfectly dull, for it receives a slight degree of resonance from the opposite lung; below that point the heart cuts off that source of resonance, and the sound is quite dull.

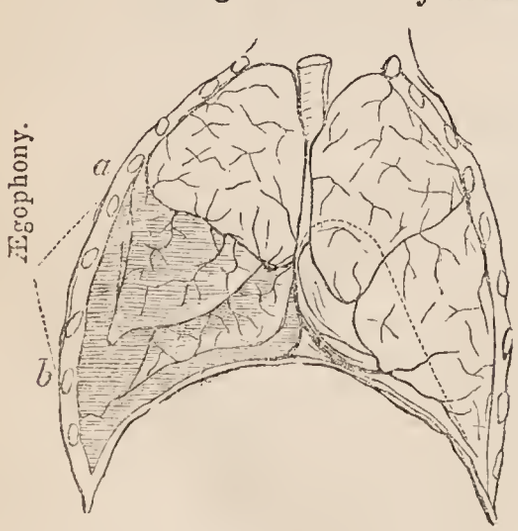

Fig. 1.

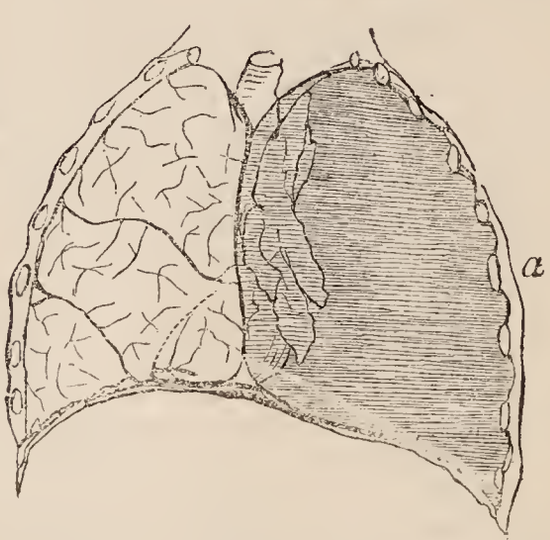

Fig. 2.

I have before stated to you that the physical signs of pleurisy are liable to be much modified by old adhesions, which bind the lung to the walls of the chest; and some of these modifications are so singular and important in relation to diagnosis, that I must advert to them. When the adhesions are loose, they only form bands or cells distended with fluid; and, keeping the lung at a moderate distance from the

walls of the chest, they may render the continuance of ægophony much longer than it would be without them. When an adhesion is so close and strong that the accumulating fluid cannot separate it, the lung is then compressed against it; or if there are several adhering points, the attachments to these are preserved by so many pillars of compressed lung. You see this represented in fig. 3, where the compressed lung still adheres to the upper part of the chest, and also to the diaphragm and mediastinum; and mark the effect: instead of a total abolition of the voice and respiration, you have at the part corresponding with the upper adhesion a loud bronchophony and bronchial respiration transmitted from the large tubes by the adhering dense column of lung; and the same thing not uncommonly occurs in other of the upper and middle regions of the chest. Fig. 4 repre-

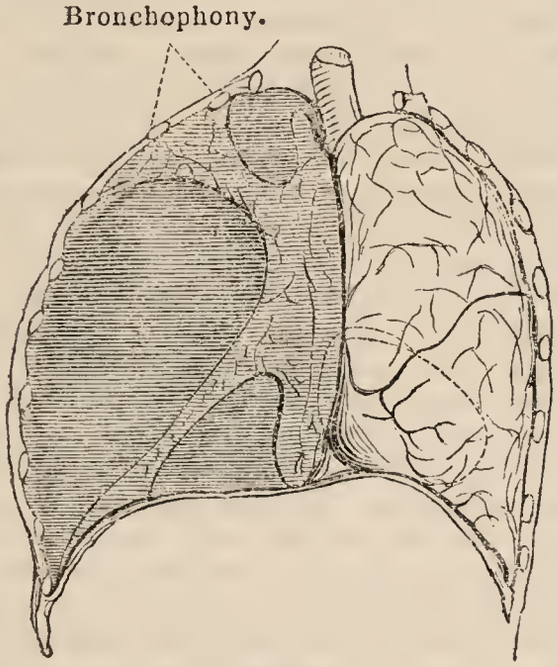

Fig. 3.

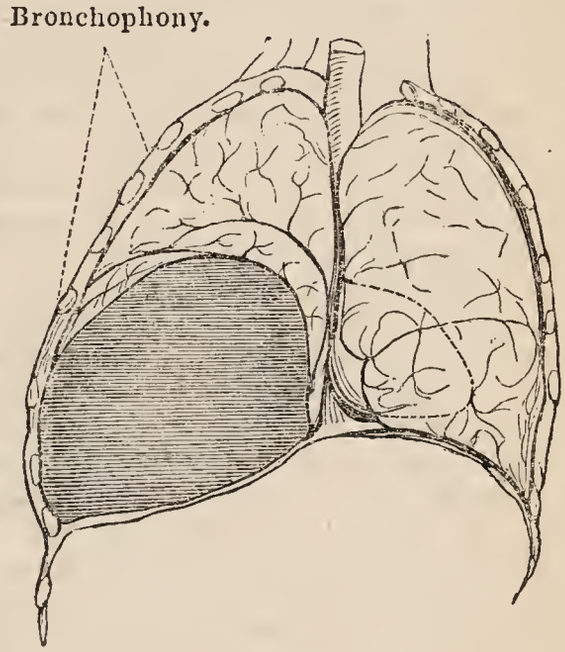

Fig. 4.

sents a pretty common form of partial effusion. The upper lobes of the lungs are the most liable to close adhesions; and when thus adhering in consequence of former disease, the lung is compressed by the effusion below against the whole upper part of the chest. In this condition it may still admit air; but as its vesicular structure is much compressed, the sound of respiration will be tubular or bronchial, and a noisy bronchophony will be transmitted by it to the whole upper region of that side. I have often heard the voice and respiration quite tracheal from this cause; and I have been more than once deceived by it into the belief that there were caverns underneath. The displacement of the heart and liver, the prominence of the intercostal spaces, and the dulness on percussion of the whole lower portion of the sternum, together with the enlargement of the affected side, will generally distinguish the true nature of these cases. Fig. 5 represents a much more

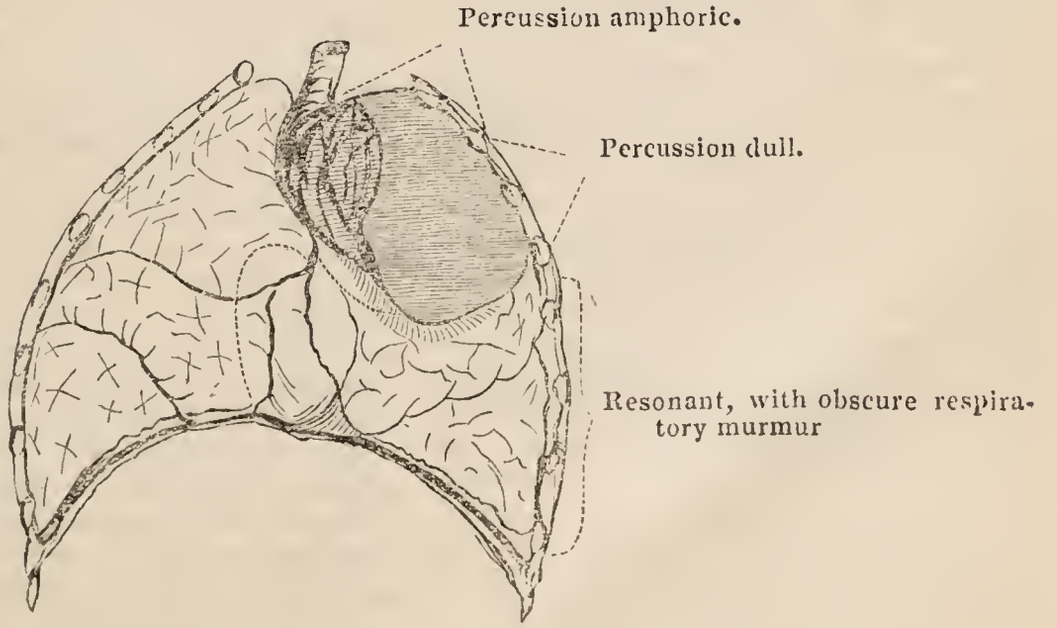

Fig. 5.

rare kind of partial pleurisy; but I have met with it in some remarkable instances, and the phenomena were so extraordinary in two of these, that I think it necessary to mention them, lest you should in any case be misled by them, as I was in the first instance. In both these cases there was moderate sound on percussion below the fourth left rib, and the respiration could be heard there obscurely; in one case it was bronchial. The heart was felt to pulsate in the epigastrium, and to the right of the sternum. From about the second to the fourth rib the sound on percussion was dull; but above this the sound was just of this bottle-like character that you hear when I fillip on my finger applied to the windpipe. It is a kind of tympanitic sound, and as in one of these cases there was also a sort of amphoric respiration heard in this spot, I concluded the case to be one of pneumothorax from perforation of the lung. This patient, whom I saw with my friend Dr. Roscoe, surprised me by soon getting well, and losing all these signs, which made me reflect more on the matter; and having since met with a similar case which proved fatal, I have satisfied myself as to the cause of the phenomenon. Dr. Hudson, of Dublin, has also recently described cases in which a loud tympanitic sound on percussion was presented in the upper part of the chest of a patient affected with pneumonia. Now you will understand how this sound is produced, if you listen to this tracheal sound which I get by filliping on my windpipe above the sternum. The windpipe also lies under the sternum, and it divides into the two great bronchi, which spread between one and two inches below the clavicles. Here, however, the porous lung lies over these tubes, and intercepts their resonance on percussion; but let this portion of lung be *perfectly* condensed by a liquid effusion, or perfectly consolidated by hepatization, and you will then get the bottle-note of the tubes, just as you do of the windpipe where no lung intervenes. The reason why this phenomenon does not occur more frequently is, that it does not often happen that the compression or solidification of the upper lobe is complete enough; but since my attention has been drawn to it, I have met with several cases of both pleurisy and pneumonia, in which it existed in a smaller degree; and I had occasion to notice in the last lecture that it sometimes occurs with dilated bronchi.

LECTURES ON BLOOD-LETTING,

Delivered from time to time,
At the General Dispensary, Aldersgate Street,

By Henry Clutterbuck, M.D.
Formerly one of the Physicians to that Institution.

Lecture I.

General Observations on Blood-letting.

We are now, gentlemen, approaching a subject of paramount interest and importance—one which merits, and I doubt not will receive, your closest attention. I mean, the use of *blood letting*, as well for the prevention, as the removal of disease. From the earliest times this remedy has been extensively employed; and at no period, perhaps, has it been held in higher estimation, or more frequently resorted to, than in the present day: one might naturally expect, therefore, that a pretty general agreement in opinion would be found among practitioners with regard to it. This, however, is far from being the case; opinions are still vague and unsettled on the subject, and, in some respects, contradictory; while the merits of the practice, as far as I am able to judge, are by no means justly or sufficiently appreciated. I know of no greater service that could be rendered to the healing art, and, consequently, to society at large, than the assigning, with tolerable certainty and precision, the various circumstances that ought to influence us in the use of this herculean remedy. The task of doing this, however, is one of no ordinary kind; and, indeed, as I believe, not to be perfectly accomplished by any individual. In offering myself to you, therefore, as a guide on this occasion, I am not unaware of the difficulty, nor of the importance of the undertaking; but it is an essential part of the duty I owe you, and I shall perform it as well as I am able. This I will venture to say, after a long and pretty extensive acquaintance with diseases and their treatment, and after as frequent a use of blood-letting as has perhaps fallen to the lot of most others;—that it is a remedy both safe and efficacious on a great number of occasions in which it is not commonly resorted to, while, on the other hand, it must be confessed that it is sometimes employed without sufficient judgment and discrimination; to the no small injury of the patient.

The importance of blood-letting, as a *medicinal* agent, in comparison with other means of cure, is shewn in various respects. It is a remedy the most frequently called for in general practice; and often, of itself,

and without the aid of other means, accomplishes all we wish. In point of efficiency, none will bear a comparison with it; while it is one for which, on numerous occasions, no adequate substitute can be found. It is prompt in its effect, so as to be adapted to many cases of great and sudden emergency. The danger of misapplying it, too, is not to be disregarded; and this is another circumstance that adds to the importance of the subject. Lastly, it is the least equivocal of remedies: its good effects, when properly administered, are, in most cases, so immediate and striking as not to be mistaken. This can, with truth, be said of few other of our curative means; which, except with regard to those of the simplest operation, seldom accomplish what they are supposed to do. In short, blood-letting is a remedy which, when judiciously employed, it is hardly possible to estimate too highly. There are, indeed, few diseases in which, at some periods, and under some circumstances, it may not be used with advantage, either as a palliative or curative means. A great number of diseases are speedily brought to a termination by the early use of this remedy, which, without it, are apt to run a protracted course; thereby inflicting much, and unnecessary, suffering on the patient. On various occasions, life itself is brought into immediate hazard by the neglect of this essential means of cure; and still oftener does it happen that, by such neglect, a foundation is laid for chronic maladies, of different descriptions, that are not less fatal in their result, though the termination is sometimes so remote that the primary affection is apt to be lost sight of: a large proportion of the fatal cases of what is called *decline*, or pulmonary consumption (a disease so destructive among all ranks in the present day, and that at the most interesting period of human life), is clearly traceable to this source—the neglect of early blood-letting; all other means being of trifling import compared with this. Dropsies, too, and *confirmed asthma*, as it is called, and a hundred other chronic maladies that render existence miserable, and ultimately destroy life, are often referable to the same neglect.

But, if I wished to excite your attention in the strongest manner to this subject, namely, the necessity of a prompt and vigorous application of the remedy in question, I would mention those inflammatory affections of the brain that are so common in early life; in boys and girls, for instance, between the ages of six and twelve more especially, though such affections are not confined exclusively to any age. From violent exertions in play, or other causes of excitement, and perhaps exposure to the mid-day sun, the patient is first found complaining of slight headache. He loses his appetite, complains of thirst, is languid and spiritless; his sleep is scanty and disturbed, and all application to his usual studies becomes irksome to him. Such cases are of frequent occurrence in seminaries for education, and attract, for a time, but little notice. If sickness and vomiting occur, as is often the case, the patient is said to be bilious, and this at once solves the difficulty—a dose of some family purgative is administered, preceded, as a matter of course, by a blue-pill, or a few grains of calomel (for these, you know, in the language of the day, are *good for the bile!*) Now, such treatment is correct, as far as it goes, for it is generally proper to exhibit evacuants at the commencement of such attacks; but how often does it not happen that, after the lapse of two or three days of fancied security, the fire, which was at first but barely kindled, blazes out with unrestrainable fury. The cheeks are flushed; the pain in the head becomes acute and throbbing; light and noise are intolerable to the patient; the fever grows intense (brain fever); utter sleeplessness follows, and this is at length succeeded by delirium, which often ends but in fatal stupor. This is a scene you will probably be called upon frequently to witness hereafter—more frequently, I fear, than you will be able to counteract the evil; for the time for acting with effect has, perhaps, gone by, and is not to be recalled. But what cannot be cured when confirmed, may often be prevented by the use of prompt and active means at the outset of the disease. This, above all others, is a case in which the maxim, *principiis obsta*, holds good. You should be extremely watchful and attentive to all cases of this description. You should do too much, rather than too little. It is better even to apprehend danger, where possibly none exists; for, by so doing, you will best consult the safety of your patient. In such cases, an early bleeding, that is, within a few hours of the attack, at the same time that it is perfectly safe, will rarely fail to put an immediate stop to the disease; whereas, by waiting two or three days, as is often done, in the hope that the disorder will subside without this remedy, and trusting, in the meantime, to saline draughts, and other equally trifling and inefficient medicines, the disease may have gone beyond the power of art to arrest its progress. You must ever bear in mind that the tendency to violent inflammatory affections of the brain is peculiarly strong in early life, and the progress of the mischief rapid; and that nothing is half so effectual in arresting their course as early

blood-letting. If, in such subjects, the slightest of the symptoms I have mentioned be observed to linger on from day to day, not yielding quickly and perfectly to quiet, abstinence, and mildly aperient medicines, you ought at once to take alarm, and have recourse to the great—I might almost say the only—remedy against the growing evil. In laying down your plan of cure in these cases, as indeed in most others, your first inquiry should be, is bleeding requisite, or even likely to become so? for in either case it should be at once resorted to. Blood-letting, more than other remedies, requires to be well-timed. It is always the more effectual the sooner it is had recourse to, insomuch, that although it may be highly proper at first, it becomes equivocal after a time, and often positively injurious.

By acting with such promptitude and decision you must expect occasionally to encounter opposition, and perhaps obloquy. You may be accused of acting with unnecessary vigour in cases that might possibly have terminated favourably by other and apparently milder means. But you can never know beforehand, at least with any thing like certainty, that this will be the result in any individual case. When such a disease as *inflammation*, and that of an acute kind, arises in organs of importance to life—as the brain, the lungs, or the heart, and which experience tells us often leads to danger—and, as the same experience proves, is best controlled by blood-letting—we are not justified in withholding the most effective, and often, indeed, the only remedy, merely because we are not sure that, in the particular instance before us, the disease may not give way to other and milder means. It is not without a feeling of regret that I now look back on some cases of this description, where, from yielding to the prejudices of others, in opposition to my own judgment, at a time, however, when I felt less confident on the subject than I now do, a means of cure has been withheld, that might, not improbably, have arrested the progress of a fatal malady. I might, with justice, extend these remarks to other forms of disease as well as the *brain-affections* of young people: but I have said enough, I trust, to ensure your attention to so important a matter.

I would here inculcate on you the absolute necessity of acquiring the requisite facility and dexterity in the use of the lancet, and which can only be attained by frequent practice. As general practitioners you are liable to called upon, daily and hourly, to administer this remedy, and you should be able to do it with confidence, and without timidity. This is by no means a superfluous suggestion on my part; for, simple and easy as the operation of bleeding appears to be, and is in reality, to the greater number of practitioners, there are still not a few, to whom, either from timidity or want of frequent use, the proper performance of it is a matter of difficulty and uncertainty. The consciousness of this naturally begets a reluctance to employ it, and it is often omitted in consequence, in cases that really require its use. An aversion, which is quite natural on the part of the patient, to the employment of a measure that has something revolting in its appearance, is now and then fostered, I fear, by the practitioner; not altogether from any doubt of its propriety and fitness, but on the less excusable ground of the difficulty experienced in the performance of the operation.

Formidable and objectionable, however, as blood-letting may appear to those who are imperfectly acquainted with it, it is, in reality, less painful and far milder in operation; and, I may add, when used with judgment and discrimination, safer also in its effects, than many other remedies that are resorted to without hesitation, as substitutes for it. Drastic purgatives, blisters, calomel, and other forms of mercury, that are in such general and almost indiscriminate use in the present day in the treatment of fevers and inflammations, are, generally speaking, infinitely inferior to blood-letting in point of efficacy, while they occasion much greater and more present and lasting suffering to the patient. Convalescence, or the restoration of strength after the termination of disease, is sooner accomplished where the cure has been effected chiefly by bleeding (always supposing, of course, that this is the appropriate remedy, and that it has been used with judgment) than where it has been brought about by those miscalled milder means. The reason of this is obvious:—in the former case the organs of nutrition are left, on the termination of the malady, in a sound and natural condition, ready and able to resume their various offices when no longer disturbed by the presence of the disease; while in the latter, by the daily-repeated use of drastic medicines, those organs (by the agency of which alone the strength can be recruited) are left in a state of weakness and disorder, from which they are often very long in recovering.

It is no small commendation of blood-letting, in comparison with other means of cure, that it tends, when judiciously employed, to supersede, and render unnecessary, a vast heap of drugs, which are often worse than useless; seldom effecting the purpose for which they are administered, and which by no means merit the implicit confidence placed in them by a credulous

public, who, if they knew the vast proportion of instances in which diseases subside spontaneously, without the aid of medicine, would be less disposed than at present to yield their confidence to ignorant pretenders to the art. The great and crying evil of the present day is what I would denominate super-medication—an overweening fondness for, and an unreasonable belief in, the power of drugs, improperly termed medicines—many of which, by their deleterious properties (for we have forced into the service, of late, the whole tribe of poisons, from arsenic to the prussic acid), interrupt the natural course of diseases, so as to make it impossible, in many cases, to distinguish between the effects of the medicine and those of the disease itself. I do not scruple to tell you that I am one of those who think that greater simplicity in prescription, and less of mystery in the practice of our art, are required both for the most successful administration of it, and for the character of its professors, than have yet found favour amongst us, but which a more enlightened state of the public mind will infallibly produce.

What I have hitherto said you are to consider as merely introductory. I wished, in the first place, to excite your attention strongly to by far the most important agent we possess for the removal of many of the most dangerous forms of disease. The subject altogether is of great extent, and requires to be placed in different points of view, each of which calls for, as it undoubtedly merits, the fullest consideration. It is only by proceeding in this way, in fact, that the advantages of the practice can be either sufficiently known or appreciated, or the cautions that are absolutely necessary for its safe administration be clearly understood. We shall have to notice its effects on the system in health as well as in disease; to describe and point out the various circumstances which influence its employment, the mode or modes in which it may be supposed to operate in the removal of disease; in other words, the theory of the subject—the different methods in use for abstracting blood from the system for medicinal purposes, with the relative advantages and disadvantages of each; and lastly, its particular application to diseases; involving, of course, the inquiry into its necessity or indispensability in particular cases, with the signs or indications that call for and justify its employment, as well as those that limit or forbid its use, and the injurious consequences that may result from its misapplication; the auxiliary aid it admits of; and the substitutes that may be found for it. It is only by proceeding in this way that the advantages of the practice can be sufficiently known or appreciated, or the cautions that are absolutely necessary for its safe administration be clearly understood. The circumstances which are to govern us in the employment of blood-letting are both numerous and various; and it would be wholly unjustifiable on my part to recommend the practice to your favour for adoption, without at the same time impressing on you the absolute necessity of carefully weighing the circumstances of each individual case that comes before you. General rules are of little service on these occasions. You may be told, for example, that bleeding is a remedy for inflammation; and so it is, in numberless instances, and that where all others are of little avail; but if it be not applied with due regard to time, degree, and various other circumstances, it will do injury rather than good.

You see, then, gentlemen, that a field of great extent is open before us, which calls for no small share of time and labour for its successful cultivation. You must be prepared to follow me in detail, and not shrink from the task, arduous though it may be. By so minute a study of the subject, and by this alone, will your labour be rewarded. With minds so imbued, you will resort to the remedy with confidence, where others feel nothing but doubt and hesitation.

A few remarks on the *history of blood-letting* will not, perhaps, prove uninteresting to you. Should it be your wish to follow out this part of the subject more in detail, I would refer you to the *Histoire de la Médecine* of Le Clerc; a work of great credit, and which will furnish you with a clue to most of the principal ancient writers. Sprengel has probably given us the most complete history of the subject; unless we choose to give that credit to Dr. Carol. Fred. Nopitsch, a physician of Nuremburg, who published, in 1833, a Chronological and Systematic Account of Blood-letting, from the earliest times. He cites no less than thirty writers before the Christian æra, and 710 from that period to 1830.

ON HEAT.

By Sir Chas. Scudamore, M.D. F.R.S *.

I purpose, on the present occasion, to consider in a medical point of view the effects of heat on the human body, and also in a brief manner to treat of its influence as a physiological and physical agent.

* Read at the College of Physicians, March 5, 1838.—Abridged from the original composition.

In the last session of the meetings of this College, the effects of *cold* were set forth by the learned president with his usual felicity and talent; and, although *haud passibus æquis*, I will endeavour to give an outline of the converse, so far as the confinement of my necessary limits will admit.

Questions of deep interest continue to engage the attention of the philosopher and the chemist, concerning the properties of this all-pervading agent. The greater part of what is useful and important has perhaps been discovered; and I may probably with truth state, that the two chief points which remain as problems for future science and inquiry, relate—1st, to the consideration whether caloric or the matter of heat is a distinct substance, or whether, like gravitation, it is only a property of matter; and, 2ndly, to the cause of subterranean heat. Is it volcanic in its origin, and partial in locality and operation; or central, universal, and differing in intensity and action, from causes too deep within the recesses of the earth for human hand and eye to discover?

In my present inquiry I shall first speak of heat as belonging to climate.

The mean temperature of the surface of the earth, and which embraces the middle latitudes, is 60°, and we may fairly assert that those climates which enjoy exemption from the extremes of heat and cold afford the most numerous and striking examples of superiority in the human race, both as regards the elevation and refinement of the mental qualities, and of the physical structure.

It was forcibly observed by the late Dr. Black, that "were the heat which at present cherishes and enlivens the globe allowed to increase beyond the bounds at present prescribed to it, besides the destruction of all animal and vegetable life, which would be the immediate and inevitable consequence, the water would lose its present form, and assume that of an elastic vapour, like air; the solid parts of the globe would be melted and confounded together, or mixed with the air and water in smoke and vapour; and nature would return to the original chaos."

That the solar powers should be so distributed and regulated that the terrestrial globe may receive its benefits, however differently apportioned; and man, its highest inhabitant, enjoy so many consequent blessings; is only one amongst the innumerable instances in which we find occasion to bow down our knee with gratitude, and look with uplifted eye, not to the great luminary of the globe, like the untaught *Parsee*, but to its all-wise author, the great Creator himself! In the wide scheme of creation, we may be well assured that "whatever is, is right," notwithstanding that in the extremes of seasons, and in the irregularities, as they appear to us, of meteorological phenomena, we may experience great inconvenience, and even occasional injury.

In our temperate climate we know but little of the deleterious influence of extreme solar heat upon the human frame; but who amongst us has not experienced, in a hot summer, the total languor and enervation which a great continuance of high summer heat produces?

Our descriptive poet of the Seasons thus makes his invocation:—

> "All-conquering heat, oh, intermit thy wrath!
> And on my throbbing temples, potent thus,
> Beam not so fierce! Incessant still you flow,
> And still another fervent flood succeeds,
> Pour'd on the head profuse. In vain I sigh,
> And restless turn, and look around for night;
> Night is far off, and hotter hours approach."

The calorific rays of the sun during a hot summer exercise a peculiar influence upon the functions of the liver, and excite that organ to an increased secretion of bile; so that, even in this country, the system becomes strongly predisposed to be acted upon by those remote causes of disease which abound in autumn, the most conspicuous of which are the miasmata generated in marshy districts, and in all situations of stagnant water, especially if with muddy bottom.

I remember the instance of a cow-keeper attending a sale of hay in a meadow about a mile out of London, close to a pond low in water, upon a hot summer afternoon, being in good health at the time, or rather feeling so, for he did not possess a sound liver. On the following morning he was, for the first time in his life, seized with a severe paroxysm of well-marked ague, of which he had a few returns.

Bilious fever, gastric and intestinal disorder, often inflammatory, cholera morbus, remittent fever, are amongst the morbid productions of the continuance of extreme summer heat; not, however, as the simple consequence of

great elevation of temperature, but of a certain constitution of the atmosphere produced by the blended influence of great solar heat, and certain terrestrial conditions, operating on human constitutions especially predisposed to disease.

It may not frequently happen in our English climate that the heat of summer is sufficient to be productive of immediate great injury to the body; but it is of occasional occurrence that extremes of temperature have within short distance of periods been remarkable. It is stated in the Phil. Trans. that in the night of June 10th, 1749, water exposed in a saucer formed a firm cake of ice, and that on some following days there were considerable frosts. On the 2nd of July following, at twenty minutes past noon, Fahrenheit's thermometer stood in the shade at 88°, and at two P.M. at 87°.

In 1748 the heat was great, and the thermometer rose in one day 22 degrees more than it had been on the preceding. On that day several horses dropped down dead under their riders, overcome by the violent heat.

In June, 1577, the heat at Plymouth was such as to raise the thermometer on the 11th, 12th, and 13th, about three P.M., in the shade, to 87°, and on the 12th it was above 88°. In regard to the public heath, the author of the communication, Dr. John Huxham, observes, "Abundance of people have suffered very severely from these successive heats; putrid, bilious, petechial, nervous fevers, are exceedingly common. Dysenteries, hæmorrhages, most profuse sweats, affect not only those in fevers, but a vast many others. The days and nights were so intolerably hot, that little or no sleep was to be got day or night. The wind we had, like the campsin, blew hot though strong."

Luke Howard, in his elaborate meteorological work, gives the following account of the heat of July 1834:—"The heat of the weather, in France, appears to be extreme; persons of both sexes, labouring in the fields, have dropped down dead from the heat. Birds also have fallen dead, from the same cause, both in France and in Spain. In the latter country, vegetation has been ruined to an alarming extent.

The *coup de soleil* is of unfrequent occurrence in this country. It is not mentioned as a disease in Cullen's Nosology; but exposure of the uncovered head to a vertical sun is stated as one of the causes of phrenitis.

We are not, however, to consider that the effects of insolation bear exclusively on the head. At one of the evening meetings of this College, last year, a paper was read from the pen of Mr. Russel, of the 73d regiment, who relates three cases of *coup de soleil*, in which the morbid appearances found after death were not in the brain, but in the lungs; which were found congested even to blackness, through their whole extent. The men were seized on a march, on a hot day. In reflecting on the probable consequences of such exposure, we must take into consideration the particular predisposition of the individual; and also whether he be in exercise, or stationary, during the exposure to the solar rays. I conceive, that in the first case (especially if the exercise should be severe and continued), the evidence of diseased action would be found in the lungs; but in the latter example, especially without protection of the head, in the brain.

It is to tropical countries that we must turn for all the great and striking examples of the injurious influence of solar heat. The different capability of different individuals to endure tropical heat is very remarkable; and the freedom from serious inconvenience which many Europeans enjoy, under direct and continued exposure to the calorific rays of the sun in a hot climate, is a surprising fact; while we can readily understand the perfect facility with which the native Indian exposes himself, the head naked, to the most intense solar heat, through the day. Together with the force of habit from his birth, he is aided in this security by the remarkable thickness of his scalp. The African proper, is, I believe, alone distinguished by the peculiar woolly hair.

"The burning province of Cumana," observes M. Humboldt, "the coast of Cora, and the plains of Caraccas, prove that excessive heat alone is not unfavourable to human life."

Henry Ellis, Esq., Governor of Georgia in 1758, describing the heat in that country, states that the thermometer, in the shade, rose in July twice to 102°, several times to 100°, and for many days successively to 98°; and did not in the night sink below 89°. He says, "I think it highly probable that the inhabitants of this town breathe

a hotter air than any other people on the face of the earth." And again, "I must acquaint you, however, that the heats we are subject to here are more intense than in any other part of the province; the town of Savannah being situated on a sandy eminence, and sheltered all round with high woods. But it is very sufficient that the people actually breathe so hot an air as I describe, and no less remarkable that this very spot, from its height and dryness, is reckoned equally healthy with any other in the province."

"Experience," says Dr. Lind, "fully confirms this truth; that in elevated and temperate situations, where the soil is dry, gravelly, and clear from wood, shrubs, and stagnating water, Europeans enjoy good health in the hottest climates, in all seasons of the year. This asylum for health is to be met with in almost every quarter of the globe. In Sumatra, Fort Marlborough affords a retreat tolerably safe. The unhealthy town of Calcutta has in its neighbourhood the healthy situations of Barasatte and Garatte.

Dr. Chisolm, in his remarks on the climate within the tropics, observes, that if the endemic causes of disease could be avoided, the pure solar heat alone would not be found a cause of injury, except in the accidental production of that species of phrenitis known within the tropics by the name of *coup de soleil*; but," he adds, "the conditions necessary to the production of this disease are different from those in which solar heat is usually applied to the human body. The subject must be exposed to the hottest rays of the sun, during the hottest time of the day, when the thermometer is generally 130°; he must be stationary; his head must in general be uncovered, and the atmosphere must be still."

It is an unquestionable and most important fact, that the human constitution is capable of great endurance of continued solar heat without injury, in situations favourable as to soil, and free from stagnant water or sources of foul exhalation; provided also that great temperance in living is observed.

Mr. Moreing, who was employed on a survey in the Red Sea, by Sir John Malcolm, in 1830, has favoured me with the following account:—

"During the latter part of June and beginning of July, 1830, we were exposed, for some weeks, to an average temperature of 93° in the shade; yet, during the whole of this period, we had not a single man on the sick-list, out of a crew of 130. The surrounding country was a desert, without a particle of vegetation for miles, and the shore sandy and rocky, without sea-wind. I have found also, that in Upper Egypt, with the thermometer 105° in the shade, I possessed as much bodily and mental energy as in England; while, in the moist climate of India, with the thermometer at 70°, I have been fainting on a sofa. Our vessels of war on the African station, it is well known, are only sickly when near shore. At sea, the crew are exposed to the same degree of heat."

Yet it must at the same time be confessed, that the European constitution is always put to a severe trial in being transferred to a hot climate. Mr. Marshall, on the influence of a tropical climate on the natives of Britain, after pointing out the immense value of temperate habits, admits that the strictest temperance, and the greatest precautions in regard to health, are often totally inadequate to guard against the diseases of equinoctial regions. He says, that when a body of troops is transferred from Great Britain to a tropical climate (India, for example), the men undergo a diminution of muscular power; the sick-list increases from four or five to ten or twelve per cent.; and the ratio of mortality rises from one and a half to six per cent. per annum. British soldiers will surmount considerable fatigue for a short time, in equatorial regions, but not by any means with a similar impunity from disease as in this country. Excessive exertions in warm climates never fail to add greatly to the sick-list. The ratio of mortality of the children of British residents in inter-tropical climates, is usually considered high; and they require to be removed, at an early age, to a temperate climate. It is often seen that they become blighted and stunted in their growth, without the appearance of the pathognomic symptoms of any specific malady."

Domestic animals imported into India from Europe are predisposed to disease—dogs particularly; they soon droop and become excessively thin; many die before they are six months in India, and only a small ratio survive a few years. They rarely propagate their species.

Imported cows and sheep do not thrive. Horses from high latitudes are neither so effieient nor so healthy as in their own country. Animals which are common to a temperate and tropical climate are, perhaps, generally smaller in equatorial regions than in high latitudes; such, for example, as the sheep, the fox, the hare, and some varieties of the deer kind.

The superior power which man enjoys over all the rest of the ereated world to maintain his standard heat in extremes of high and low temperature, qualifies him to explore every known region of the earth. Inferior animals degenerate, or die, by removal to a very opposite climate, though beautifully fitted to their destined localities; as, the lizard and the chameleon remain cool under the equator, while the whale and the porpoise retain a degree of heat above that of the human body, though surrounded with the waters of the coldest northern seas, and amidst mountains of iee, in the neighbourhood of the Pole.

The capability of the human body to resist with impunity the influence of very high artificial temperature, has been proved by many experimenters, and by none more distinguished than our countrymen, Sir Joseph Banks, Dr. Fordyce, and their friends.

Dr. Blagden relates that he went into a room so heated, that in the hottest part the thermometer rose to 260°. In addition to his common elothes he had a pair of thick worsted stockings drawn over his shoes; he also wore gloves. He remained eight minutes in this apartment. He says, "The air felt very hot, but still by no means to such a degree as to give pain;" and he and the other gentlemen thought they could have borne a much greater heat. He observes, "For seven minutes my breathing continued pretty good; but after that I began to feel an oppression in my lungs, attended with a sense of anxiety, which gradually increasing for the space of a minute, I thought it most prudent to put an end to the experiment." His pulse, on returning to the cool air, was 144. He conjectured, that should a heat of this kind ever be pushed so far as to prove fatal, it would be found to have killed by an accumulation of blood in the lungs, or by some other immediate effect of an accelerated circulation.

In all the experiments, which are too numerous to be quoted, the gentlemen were not inconvenienced by the transition from the heated apartments into the cold air. These results eorrespond with the fact with which we are familiar, that when we are thoroughly warmed, we can leave a hot apartment and go immediately into a very cold atmosphere without injury, provided that we are not at the time in a state of nervous exhaustion from any cause, or perspiring freely from the general surface; but if the body be much fatigued, as from long-eontinued daneing, and especially with a very relaxed skin, the risk of injury is great.

These and similar experiments serve to shew that the animal powers ean resist heat as they can resist cold; or, in other words, that the standard degree of the animal heat can be maintained with wonderfully slight deviation, both in the highest and the lowest temperatures.

That the human animal heat, however, is raised by a warm climate, is proved by Dr. John Davy, who found the temperature of the inhabitants of the Island of Ceylon higher than ours by one or two degrees of Fahrenheit; and that he had observed a similar change in the same individuals before their departure, and after their arrival.

The greatest elevations of the human temperature under the influence of external heat have been observed by Delaroche and Berger in their own persons; and they mueh exceed what I have obtained.

At my request, Dr. Green, of Marlborough-street, made ten examinations of patients receiving his sulphurous gas baths, with a eorrect animal heat thermometer, the bulb of which was placed under the tongue; and the following was the result:—The average degree of the animal heat was 97·5°; the elevation at the expiration of five minutes was to 99°; at the end of fifteen minutes to 100°. Then free perspiration took place, and the animal heat again diminished. Such was the mean of the experiments. The lowest temperature of the bath was 135°; the highest, 165°.

The highly deleterious influence of hot winds in tropical elimates, both on animal and vegetable life, is well known. Those who have been in the hottest parts of India compare the blast of the external air to that from a furnace. But not speaking of the very

hottest parts, in the hottest season, I have found invariably that those who have lived in India complain of greater inconvenience from the oppressive heat of a crowded room at a London *soirée* in warm weather, than from the atmosphere of Calcutta; an effect referrible to the deteriorated air from the numbers assembled, especially if there be want of proper ventilation.

The Kamsin of the great Arabian Desert sometimes overtakes a whole caravan, and sheds its pestiferous influence on man and beast, to almost total destruction.

In Africa, Lander says, "the Harmattan has the effect of drying up the skin of the natives in a very extraordinary manner. After an exposure to it the skin peels off in white scales from their whole body, which assumes an appearance as if it were covered over with a white dust.

With relation to atmospherical temperature, the pestilential period varies in different countries, according to geographical position, and the consequent particular seasons. Dr. Russel observes of the plague in the Levant, "the winter puts an end to it in Constantinople; the summer destroys it in Egypt."

Sir Gilbert Blane, in "his Elements of Medical Logic, states that it is incontrovertibly established by the experience of ages, that the existence of plague cannot consist with a heat of the atmosphere above 80°, nor a little below 60°." He adds, "the pestilential yellow fever, in like manner, has its own range of atmospheric temperature, but on a higher scale than the plague, for it cannot subsist long if the temperature falls below 80°."

I think it questionable whether Sir Gilbert Blane may not have stated this condition of temperature too positively with regard to yellow fever; and it appears to me certainly so with respect to plague. In the year 1664, when the plague raged in London, the deaths were, in December, in the course of one week, upwards of 12,000; and in the following year they were, in June, 590; in July, 4129; in November, 3449; and December, under 1000.

Mr. Wilkinson, who resided many years at Alexandria, informs me that the average temperature at the beginning of the plague, in March and April, was about 78° at mid-day, and from 92° to 97° when the plague ceased, about the 20th of June. The disease is not known as an epidemic in Upper Egypt, where the heat is so much greater than in Lower Egypt, and the soil so remarkably dry. Any rain there is a surprising phenomenon.

On the point in question I am favoured with the opinion of Mr. Urquhart, whose testimony is highly valuable from his long stay at Constantinople, and from his having made a very diligent study of all the important circumstances relating to the statistics and topography of the plague. He says, "I have seen the plague in the midst of snow, and with a temperature above 100°. The universal opinion at Constantinople is in its contagious character. They also believe that its ravages are arrested by a low or a high temperature; and it generally so happens; but when the plague begins late in the year, it continues through the winter, as during last winter, when it commenced two months later than usual. I have observed it to run a course of six months, or thereabouts, in the same place. There are places where it never ceases; but there, the one-half of the town will have a different exposure from the other, the plague alternating from the one portion to the other with the alternation of the winds, which generally blow six months from the south, and six from the north. There are other localities where the plague never ceases."

The connexion of a southerly constitution of the air with pestilence has been remarked by writers from the earliest records to the present time. The new and unwelcome visitor to Europe, the Asiatic cholera, did not shew any particular preference to the months of the year, or appear to have its virulence modified by atmospherical temperature; but certainly it prevailed most in damp and low situations.

According to the statistical accounts of M. Moreau de Jonnes, the deaths from cholera in Paris, and the departments, were, in April 1832, when the disease first broke out, 16,607. It continued, without intermission, shifting its situation till December. In June, the deaths were 19,440; in December, 602; but its relative fatality to the number attacked was not diminished in the colder months.

M. Brièrre de Boisment, in his analysis of the epidemic cholera in Poland,

observes, "that every atmospheric change was followed by an accumulation of cholera patients in the hospital; while, if the weather remained stationary for some days, the disease ceased to make material progress. Great heat, a storm, or sudden cold from a state of heat, was immediately followed by an increase of admissions."

From all the examinations I have made of the various details published, I am led to the conclusion, that this dreadful pestilence ran its course in the particular district which it invaded, in three months, as a mean statement.

Curious accounts have been given by Dr. Henry, of Manchester, and by Mr. Wallace, of Dublin, of an apparatus for disinfecting, by means of dry heat, all articles supposed to be impregnated with contagious matter. It was considered that the infectious matter of cow-pock was rendered inert by a temperature not below 145°; and that the more active contagions are probably destructible at temperatures not exceeding 212°.

I shall conclude my subject with some observations on the influence of heat of climate on the physical characters of the human race.

The early development of the organization distinguishing puberty in warm climates, is well known; and life itself, for the most part, runs its several stages in a shorter time between the tropics than in temperate climes.

As we go south, the period at which the catamenia of females commence and cease become earlier with every degree of latitude; until, on arriving in India, we find that in the plains, females become mothers at from ten to twelve years of age, and lose the catamenia at from twenty-five to thirty. In the south of Europe, in Italy and Spain, the same circumstances are observable in a modified degree.

There are occasional instances of remarkable longevity in the natives of hot climates. It appears, from the statistical reports of the American government, that in the Southern States the number of persons above 100 years of age bear a proportion of twelve to one in favour of the blacks over the whites; and I find that from those given of the West Indies, the instances of equal longevity amongst the coloured population are very numerous, and in a proportion over the white population equally remarkable. We learn from travellers, that in Africa longevity amongst the negroes is far inferior to that which so remarkably occurs in the West Indies.

From the whole inquiry, the conclusion, I think, very clearly follows, that the health and longevity of the human race are in an infinite degree more connected with the various circumstances of food, clothing, occupation, habits of living, place of residence, free ventilation in dwellings, &c. than with climate merely, whether hot or cold.

That solar heat causes blackness of the skin is an ancient opinion, and for the difference of colour, and other varieties of the human race, some modern writers of eminence have resorted to climate as an almost sufficient explanation.

"The heat of the climate," says Buffon, "is the chief cause of blackness among the human species." I could quote other writers who express a similar opinion still more strongly.

That certain diversities are produced by climate, and especially in the complexion of the skin, is a truth that must be at once acknowledged; yet a little inquiry enables us to discover that we must look for deeper causes to explain the great varieties of the human race, than climate alone.

"It cannot be doubted," says Humboldt, "that the greater part of the natives of America belong to a race of men, who, isolated ever since the infancy of the world from the rest of mankind, exhibit in the nature and diversity of language, in their features and the conformation of the skull, an incontestible proof of an early and complete separation."

I am favoured by a very intelligent friend, who has been a great traveller in India, with the following particulars:

"The Malays, Javanese, Borneans, and the inhabitants of some of the other islands in the Eastern Archipelago, on and within a few degrees of the equator, N. and S., are all copper-coloured, and much lighter in complexion than the natives generally of Upper India, Lahore, and Afganistan, between the 30th and 35th degree of N. latitude. A large portion of the Himalayan mountaineers, at an elevation of from five to eight thousand feet above the level of the sea, where the winter is as severe as in Europe, are fully as dark or darker than the Malays living near the equator,

as just now stated, where the solar rays are so powerfully felt. The latter expose their bodies almost entirely to the influence of the sun, whereas the mountaineers constantly wear white woollen clothing: yet their bodies are of quite as dark a hue as their faces.

In the various parts of Hindostan and the neighbouring countries, there is an extraordinary variety in the colour of the natives, where the power of the solar rays must be the same. But the phenomenon in question is still more remarkably exemplified in Africa, a comparison being drawn between its inhabitants and those of Asia in the same latitude. For example, look at the people of Africa, the natives of Darfour, Kordofan, Bourneau, and in N. latitude from 15° to 25°, they are generally, I believe, with the exception of the Egyptians and Arabians, quite black, with woolly hair; whilst the Persians, the natives of the Deccan, in India, the Birmese, Siamese, and Chinese, are all either brown or copper-coloured."

The different customs of the natives of hot countries in different parts, in regard to clothing, is worthy of further remark.

Mr. Moreing informs me, that in the hot region of the Nubian coast, between the Straits of Bakel-Mandah, and the 25th degree of N. latitude, and generally the western shores of the Red Sea, the natives are copper-coloured, and without the negro features, exposing themselves to the hottest rays of the sun with uncovered heads; while, on the opposite coast of Arabia, where the thermometer seldom indicates so high a temperature, they most carefully cover themselves. Their complexion is a light copper colour. Mr. Shaw, the missionary, relates to me, that in South Africa he had observed marked distinction of complexion amongst the various tribes; that the Kaffres, Zeulas, and Buchwanas, on the eastern side of the Peninsula, were black, with woolly hair, whilst the Wamaquas, Bushmen, and Hottentots, were copper-coloured, with lank straight hair.

In now bringing this paper to a termination, I must offer much apology for having trespassed at such length on the patience of my audience; although, from the nature and extent of the subject, I feel that I have rather borrowed from my materials than exhausted them.

TAPPING IN EMPYEMA.

To the Editor of the Medical Gazette.

Sir,

Conceiving that the following case presents a few peculiarities worthy of notice, I shall be obliged by its insertion in your widely circulating journal.

I am, sir,
Your obedient servant,
T. Ogier Ward, M.D. Oxon.

Birmingham, March 10, 1838.

Sept. 14th, 1837.—Joseph Hadduck, æt. 2. In April last (four months ago) he was seized with cough and other symptoms of inflammation of the chest, for which he was treated by a druggist. He recovered in some degree, but never lost his cough, and continued to emaciate, which his mother attributed to the irritation of teething; and growing anxious about him, she came to consult me.

Present state:—Great emaciation, but little fever; no night sweats; pulse 160; tongue clean; appetite good; hacking cough, without expectoration; bowels regular. Sleeps only on left side; left side of chest distended, and dull on percussion, with evident fluctuation; no respiratory murmur over that side, except at the upper part and roots of the lung, where it is bronchial. Heart is felt to the right of the sternum. Respiration clear and puerile over right lung. I ordered mercurial friction to the side, and a mixture containing the iodide of potassium.

18th.—As no improvement had taken place, but the emaciation seemed increased, I requested my friend Mr. Crompton to puncture the chest, which he did, and drew off through a canula above a pint of creamy inodorous healthy pus, without the admission of more than a very minute quantity of air into the chest. During the operation the child coughed frequently, and at each effort the pus was projected in a powerful jet through the canula. After the operation the left side of the chest was as much smaller as it had previously been larger than the right, that is, about three-fourths of an inch; but the heart retained its unnatural position, and the child continued to lie on the left side. No bad symptom fol-

lowed the operation; on the contrary, there was marked improvement. Nevertheless, the chest filled again, and was again emptied on the 25th. On this occasion the pus was the same in consistence and quantity as before; but it had a greenish tinge, and the external air entered the chest freely, though some of it was withdrawn again by an elastic gum bottle. Percussion of the left side immediately became very clear, but no dashing of fluid was heard upon shaking the patient.

26th.—The child looks much better, and is gaining flesh decidedly. He is lively, and his appetite is keen. The pulse has fallen forty beats since the operation, being now 120 instead of 160. Cough slight; left side much smaller than right, and left shoulder lower, the spine being curved laterally. The heart is still to right of sternum; percussion clear; amphoric sound of dashing of fluid on succussion; respiration only audible at back and roots of lung. A day or two after the second operation, the weather being warm, the child was observed to sweat much while he slept; but he continued to improve in flesh and strength. The chest again became filled with fluid and air, that gurgled audibly upon moving him even slightly; and on October 7 he was tapped for the third time, and a similar quantity of pus removed in my absence.

Oct. 8th.—The pus having been kept for my inspection, consisted of about a fourth of clear serum floating above the thicker portion, which was void of the slightest mark of putrefaction. The heart has returned a little towards its natural position, and respiration is more extensive. The fluid is appearing again, and is detectible by percussion and succussion.

12th.—Fluid increased in quantity, but from its dash is evidently thinner than before; cough still less, and sweating has ceased; respiration is clear at back of chest.

17th.—Much better; gains flesh fast. Fluid is less, and, judging from the sound, is of thicker consistence. Heart almost in natural position; respiration more extensive, with a little mucous rattle; no cough nor sweating; pulse 100 From this time the fluid became gradually less, and more dense, till it ceased to be detectible by percussion and succussion. The heart returned to its proper place, the spine became straight, and the ribs expanded; the only symptoms left at the end of October being a little obscurity of respiration, and occasional intermittence of the pulse. The child is now (March 10) in perfect health.

The medical treatment, after the first operation, consisted merely in regulating the bowels by occasional doses of hydr. c. cretâ, and in allaying the cough and arresting the perspirations by a linctus containing quinine and acid. sulph. dil. The skin of the chest was drawn aside at each operation, in order to form a valvular opening.

The early age of the patient, the duration of the disease before it was detected, the absence of hectic fever, the nature of the pus at the first operations, and the change in its consistence after the admission of air into the chest, and its final absorption after the third tapping; the fall in the pulse immediately after the second operation, the prolonged dexiocardia, and the perfect restoration of the child to health, without any deformity of the chest remaining,—these are all interesting points for consideration, but are rather of a speculative character than of much practical importance. But there are two physical signs presented by this case, viz. the fluctuation and sound on succussion, that were of considerable use to me during the progress of the cure, both in regard to the diagnosis and prognosis of the disease.

I need not enlarge upon the utility of fluctuation as a diagnostic (I might almost say a pathognomonic) sign of effusion, although I am not aware of its having as yet been applied to the diagnosis of chest disease. In this case, owing to the thinness and elasticity of the parietes of the chest, the fluctuation of the fluid was very distinct on percussion, and in fact formed a useful guide for the introduction of the trocar, since for want of knowledge of the exact height attained by the effusion, there was some risk of wounding the lung. Another circumstance that was of great use in determining the situation of the puncture in the second operation, was the fluctuation of the fluid caused by the heart's impulse, that was very evident over the intercostal spaces as high as the effusion existed, but there ceased. This sign I once saw exhibited

in a remarkable degree in a case of gangrene of the lung, with destruction of the intercostal muscles, and hydro-pneumo-thorax.

As this is the only case in which I have been able to detect the exact level of the fluid by this means, it would be presumptuous to offer more than an opinion that it may be of service in ascertaining the more moderate effusions of acute pleurisy and pericarditis; and I conceive its utility would be restricted to indicating the *presence* only of a fluid, notwithstanding that M. Piorry speaks of a gelatinous elasticity communicated to the finger by percussion over an hydatid cyst. Combined, however, with the next sign that I am going to mention, it affords an accurate notion of the *consistence* of the fluid also.

This sign, the sound on succussion, has not to my knowledge been noticed by any author, except perhaps by Hippocrates, as indicative of the nature and consistence of the fluid effused into the chest. In the present instance, after the air had been admitted, upon shaking the child, a dash of liquid against the walls, and its fluctuation, were perceptible by the hand, as well as audible to the ear at a distance; and as the fluid became thicker or thinner, augmented or diminished in quantity, so did the dash become smart or heavy, and the sound acute or dull; so that, after the last operation, finding that as fluid decreased in the smartness of its impulse, the dulness of its fluctuating sound was increased, I concluded that it was becoming absorbed, and that no further operation would be required. It is difficult to describe these varieties of sound and impulse; but any person may convince himself of the possibility of drawing such distinctions, by shaking oil or treacle, and water or spirit, in separate elastic bottles, when not only the state of plenitude of the vessel will be readily ascertained, as Laennec has already pointed out in his article on pneumo-thorax, but also the density of the contained fluid; indeed, the difference of sound between water and spirit is very apparent.

A DESCRIPTION OF AN ANOMALOUS CONDITION OF THE HIP-JOINT; ALSO, A CASE OF PARTIAL LUXATION OF THE HUMERUS UPWARDS,

CONSEQUENT ON SENILE DISEASE OF THE SHOULDER-JOINT *.

By Hamilton Labatt, A.B., M.R.C.S.I.
Demonstrator of Anatomy at the Royal College of Surgeons, Ireland.

(*For the Medical Gazette.*)

I wish to call the attention of the Society to two very interesting cases which lately attracted my notice in the neighbouring dissecting-rooms. The first represents

A singularly Distorted and Half-anchylosed Condition of the Hip-joint, produced by considerable osseous growth from the component bones.

The preparation now on the table was taken from an adult male, apparently about 40 years old, with muscular system fully developed, and no appearance of emaciation about the joint. My observation was first called to a large oblong tumor in the left inguinal region, of bony consistence, about the size of a turkey's egg externally, covering the anterior inferior spine of the ilium, and extending downwards and inwards under Poupart's ligament, which was pushed a little forwards. The integument covering the tumor was perfectly healthy, and moveable on the subjacent structures. The corresponding limb was slightly everted; hip-joint in an almost fixed state of semi-flexion, consequently its motions (by none of which was the tumor affected) were extremely limited; abduction and adduction very slight, as also flexion and extension; rotation impossible. The great trochanter not as prominent as usual; no very decided shortening or elongation of the limb.

The above is a brief outline of the case before any dissection was entered on. As to its precise nature I had considerable doubts. The absence of alteration in the length of the limb indi-

* Read before the Surgical Society of Ireland on the 10th of March.

cated that there had been no dislocation; and there being no emaciation of the surrounding soft parts, made it probable that the symptoms did not result from old disease of the joint. The questions to be solved were—whence the eversion of the limb? whence the interference with the motions of the joint? and lastly, whence the fixed state of semiflexion?

Dissection. — The superficial coverings, glands, and cellular membrane, had been removed, and Poupart's ligament, with the abdominal muscles, dissected away, when the fleshy fibres of the iliac muscle appeared pushed forward by, and expanded over the tumor. Its tendon, tense and flattened, at first sight resembling the common accessory ligament, passed downwards and inwards to the lesser trochanter, which it pulled forwards, thus in part accounting for the limb being "everted." The tumor itself, evidently of a bony structure, was pyriform, its long axis from above downwards, and from without inwards. Its greater extremity was firmly connected to the pelvis, above the acetabulum, and involved the anterior inferior spine of the ilium, with the origin of the rectus muscle, infringing posteriorly on the iliac fossa. The other extremity was lost below on the neck of the thigh-bone, to which it appeared to be united anterior and superior to the lesser trochanter. On its internal margin, situate about $2\frac{5}{8}$ inches from the symphysis pubis, a deep groove, unprovided with cartilage or synovial membrane, was presented for the tendon of the great psoas muscle, which was perfectly normal in other respects. The external margin was about an inch from the anterior superior spine of the ilium, and, passing downwards and inwards, became continuous with the inferior margin of the tumor, which overhung the neck of the femur anteriorly, and appeared to push the great trochanter backwards, thus entirely accounting for the limb being everted. On dissecting away the iliac muscle, the periosteum of the iliac fossa, thick and cartilaginous, passed over the anterior convex surface of the tumor, which was rough and scabrous, and formed a firm connexion with that membrane.

I now proceeded to take an accurate memorandum of the exact position of the thigh-bone; and taking the tip of the great trochanter as the centre of three measurements, I found the distance to the anterior superior spine of the ilium to be $4\frac{3}{4}$ inches; to the anterior part of the tuber ischii, $1\frac{7}{8}$ inch; and lastly, to the tip of the coccyx, $5\frac{3}{4}$ inches. The muscles attached to the great trochanter were relaxed, but natural in other respects.

The capsular ligament was normal in its attachments and structure posteriorly and externally, but anteriorly and internally it was lost above, on the drooping margin of the tumor. On making a perfect circular division of it, the head of the thigh-bone was still held in the acetabulum by a firm osseo-cartilaginous structure, an inch in thickness, uniting, as it were, by symphysis, the anterior and internal part of the neck of the thigh-bone to the inner surface of the overhanging morbid growth. With the assistance of a knife, and very considerable force, the union was torn through, and the head of the bone disengaged from the socket, displaying *the investing cartilages, synovial membrane, and round ligament, in a perfectly healthy condition.* This, together with the fact of the surrounding soft parts having undergone such trifling alteration, has led me to conclude that the exostosis must have been very rapid in its development.

Since the above was written the periosteum and other soft parts have been removed from the bone by maceration, enabling us to observe other peculiarities in this remarkable case. On examining the bone forming the iliac fossa, you will remark that for some distance posterior to the tumor it possesses a very unusual degree of fragility and brittleness, as if the vessels in this situation had been either deficient in their supply of animal matter, or that a superabundant proportion of earthy material had been given out; indicating in a very beautiful manner the circumscribed morbid action of the vessels in this situation. It was also interesting to observe, that during the drying of the preparation, a constant sero-sanguincous exudation took place from the surface of the bone here: *could this be attributed to a weakened and diseased condition of the coats of the arteries supplying the bone in this region?*

The last point for observation was the state of the neck of the thigh-bone. Considerable bony deposit on the anterior part, where it appears to be short-

ened in a slight degree; superiorly, from the head to the great trochanter, the contraction or shortening is very striking; and perhaps this may be connected with a peculiar twist which the bone presents here. The neck appears to be more perpendicular than natural, and to be thrown on a plane posterior to its usual limits. I am not prepared to assign any cause for such a condition. We know that this portion of the bone is sometimes thrown on a horizontal line from the effects of interstitial absorption; but I cannot account for its direction in the preparation before us.

For the second preparation on the table I am indebted to Mr. Maclean, one of the gentlemen dissecting in the adjoining school; and, in the first place, I must ask the indulgence of the Society, being obliged to omit the appearances presented by the joint before any of the superficial parts had been injured, my attention, unfortunately, not having been called to the case until some progress had been made in the dissection.

In the shoulder-joint before us, we have *an excellent specimen of that remarkable disease to which old people are peculiarly liable, as also an example of partial dislocation upwards, predisposed to by the primary disease of the parts.*

The subject was female, apparently about 60 years old, with muscular system fairly developed. The common integuments had been removed, and the deltoid muscle cut across and thrown back, when the attention of the dissector was attracted by the head of the humerus, which was exposed, and firmly supported against the under surface of the acromium process by the lips of a vertical rent in the capsular ligament, otherwise healthy, firmly girding the anatomical neck of the bone. The insertions of the supra and infra spinate muscles, as also of the teres minor and subscapular, were perfect, and there did not appear to be any wasting of those muscles.

At the superior extremity of the vertical rent in the capsule, you may observe a strong band of fibrous structure passing from the exterior of the capsule to be connected with an osseo-cartilaginous body, about the size of a kidney-bean, lying above the glenoid cavity of the scapula, and enclosed in a pouch of the synovial membrane. The head of the humerus is spread out, flattened, and considerably elongated in its transverse direction. The investing cartilage and synovial membrane absorbed. For a considerable extent anteriorly it is rough and scabrous, but superiorly, towards its summit, for the extent of about two inches transversely, and half an inch antero-posteriorly (where it was exposed to a corresponding surface on the under surfaces of the acromium and adjoining extremity of the clavicle,) it presents a smooth, white, and polished appearance, like ivory. Posterior to this again the head of the humerus is rough, with the exception of a small smooth portion which moved on a similar one at the superior part of the glenoid cavity of the scapula.

The capsular ligament being divided internally and inferiorly, considerable ossific deposit presents itself in this direction at the junction of the head and anatomical neck of the humerus; and, projecting into the joint, but imbedded in, and evidently connected with, the tendinous structure of the subscapular muscle at its insertion, is an osseous body, rough on its surface, and nearly as large as the one already described. The tendon of the biceps is excessively thin and wasted: on tracing it up the groove, it terminates by a firm insertion into the superior part of its external edge, sending off internally a fibrous production, which almost immediately becomes continuous with the capsule, running along the inner lip of the rent towards the apex of the glenoid cavity, but to which it was not clearly traceable.

The acromio-clavicular articulation was remarkably relaxed, admitting of extensive rotatory motion. On dividing the superior ligaments, it was much thickened and hypertrophied, and inclosed three or four minute cartilaginous nuclei, *one of which pressed in on the joint.* Synovial membrane nearly absorbed; investing cartilage thin and softened.

The preparation which I have just attempted to describe is interesting, as not only presenting us with an admirable specimen of the disease under consideration, but also as exhibiting to us the extensive destruction of parts which it may entail; and although it is true that this senile disease of joints does not affect the general constitution in

the serious manner which certain diseases of joints to which young people are liable do, still it is evident that it is as fully capable of ultimately spoiling every mechanical provision connected with the joints.

As to the cause of the disease, I am not aware that it is yet quite decided. Mr. Adams, in his very excellent article on the abnormal condition of the elbow-joint, in the Cyclopædia of Anatomy and Physiology, attributes it to the effects of chronic rheumatism; and the observation of Beclard, who states that he has observed the same pathological condition of joints more frequently in northern than southern climates, would certainly tend to strengthen the opinion. Moreover, in the article on chronic rheumatism in the *Dictionnaire de Médecine et de Chirurgie Pratiques*, the author, in describing a variety of structural diseases produced in joints by that disease, especially mentions a state of parts coinciding precisely with the preparation on the table, and alludes particularly to the occurrence of spontaneous luxation as a consequence. "Thus," as he says, "a new disease is added to that which existed before."

I do not purpose delaying the Society as long on this subject as, perhaps, its importance might warrant; but I shall conclude, after referring to two remarkable productions in the disease—I do so particularly, as the preparation before us gives considerable information as to their mode of formation.

1st. *With respect to the Cartilaginous and Bony Productions.*—It would appear that these may proceed from any of the fibrous structures, whether ligament or tendon, in the neighbourhood of the joint. In our case, for instance, you observe one of them appended by a strong band of fibrous membrane, passing from the exterior of the capsule; and the other was *imbedded* in the tendinous structure of the subscapular muscle at its insertion, and projected into the joint: and it is remarkable to observe how the former, which was exposed to friction in the interior of the joint, has been rendered quite smooth, whereas the latter, not so circumstanced, remains rough on its surface. Mr. Adams, in alluding to those bodies, states that "they seemed to be productions of the synovial membrane." It is certainly not improbable that this may be so in some instances; for I have found them on more than one occasion in analogous membranes, the peritoneum, for example, where they amounted to the size of a pea or less. Still our case proves that such an opinion is limited by at least one exception. In fact, the fibrous structures appear to have been particularly engaged in the instance before us, and at a very early period, if we may judge by the state of the acromio-clavicular articulation, where, although the disease appeared to be only commencing, the ligaments had already assumed a thick and hypertrophied condition, enclosing in their structure small *cartilaginous nuclei, which, from the circumstance of one of them pressing on the joint, I assume as presenting to us the first stage of the formation of the loose cartilaginous or bony productions.*

Lastly, I would refer the Society to the *ivory deposit* on the head of the humerus. Mr. Adams ingeniously lays it down as the result of friction; and this is quite in accordance with our preparation, as we only see it on those parts of the head of the bone which have been in actual contact with the processes of the scapula *.

The preparations connected with this paper shall be placed in the College Museum, where they may be examined by any one desirous of further investigation.

MEDICAL GAZETTE.

Saturday, March 31, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE DEBATE ON FLOGGING.

A DEBATE upon this subject took place in the House of Commons last Monday, on the motion of Captain Boldero for a select committee to inquire into the military punishments and rewards now in force in the British army. The gallant Captain objected, with great justice, to the commission which was appointed some years ago to examine

* The opposite shoulder-joint presented precisely the same diseased condition.

into the propriety of abolishing corporal punishment in the army, that there was not a single person appointed upon it who had ever advocated the abolition of flogging, either in that or the other House of Parliament. He also objected that not one medical witness had been examined before it. Our readers will find, on turning to a former article*, that just *one* surgeon was examined by the Commissioners; but as he came merely to apologize for the death of a soldier named Ramsey, who died of tetanus, after having received 134 lashes, the Captain's objection is substantially correct.

Now, two stronger objections could hardly be made; the first being, that of a set of men appointed to inquire into the doubtful necessity of a cruel system, not one had shown a decided wish for its abolition, while many were pledged to its continuance; the second being, that no witness was called of the class best qualified to set forth the horrors of corporal punishment at present inflicted, and to discuss the relative merits of the proposed substitutes†.

Captain Boldero's motion was, of course, rejected; but we hope he will persevere in it every year till increasing minorities become a majority, and the cause of reason and humanity is triumphant. Truth, said the ancients, is the daughter of Time, and the mother of Virtue; or, in the homely language of the Commissioners—"Nothing can be more certain than that in this country, and with the ample means afforded to every man in it for the free discussion of any subject in Parliament, in courts of law, in public meetings, and through the press, no practice can be long maintained which is really contrary to the well-considered judgment and settled feelings of the country."

A good deal of the debate, as often happens in the House, and elsewhere, consisted of the *argumentum ad hominem:* "how came you, Sir De Lacy Evans, when in Spain, to flog men, to let women be flogged, and to give the Provost-Marshal a quantum of power unauthorized by the Articles of War?"

General Evans came out of this brisk fire, not quite unsinged indeed, but yet not much injured. His floggings were usually very moderate, he says—mere schoolboy punishments; so that some of the officers, staunch *milleplagarians*, or thousand-lashes-men, expressed their discontent at the niggardliness with which torture was dealt out, and felt quite uneasy under the non-infliction. It is impossible to content every one. Bull-baiting was once thought a manly sport, and flogging is probably looked upon by many as a warrior-like relaxation. Baiters and milleplagarians must alike deplore the effeminacy of the age. There were one or two cases, however, alleged against General Evans, by Sir H. Hardinge, in which the punishment was not of this mitigated cast. A man received 300 lashes in the convent of Corban, and he declared to Sir H. Hardinge that the naval cat was used on the occasion; three hundred lashes with which are equal to a thousand with the ordinary cat. When we find the desire of vengeance becoming so rampant as this, and hear of men slowly cut to pieces under the name of punishment, we are naturally led to inquire what was the object for which the Legion fought—what was the end they hoped, if successful, to attain? The expulsion of Don Carlos, and the placing the crown of Spain on the head of

* Med. Gaz. vol. xx. p. 597.

† Mr. Hawkins, in the valuable lectures reported in our last number, exhibited a tumor formed in the scar left by a flogging, and removed by Sir B. Brodie from a man who had been sentenced eleven years before, to receive 1000 lashes; and another which was removed from a man's back by Mr. Jeffreys, and which, also, followed a flogging received twenty-seven years previously.

the young Queen, were means, not ends. The real object which inspired the finer spirits of this army, was the triumph of liberty. They had hoped to deliver Spain from the horrors of despotism—above all, from the Inquisition; and to such men the infliction of torture must be equally hateful whether under military, judicial, or ecclesiastical pretences. As to the punishment of women, Sir De Lacy Evans replied, that it was never inflicted by his authority.

We must confess that the gallant General came off rather lamely on the point of the extended powers of the Provost-Marshal. According to the British articles of war (by which the Legion, as British subjects, were to be governed) the Provost-Marshal has no authority to act without the orders of the officer commanding in the field, unless he himself sees the offence committed; while General Evans allowed the Provost-Marshal to receive his orders from any general or staff-officer.

To this the limping reply of the gallant General was, that the document stipulating that the Legion was to be treated according to the British articles of war, was signed by the Spanish Ambassador, but not by him; and that he had only put his name to it, as attesting it to be a true copy.

Now whatever might be thought of this quillet, if the gallant General were on his trial in the Court of Queen's Bench, we are quite sure it will avail him nothing before that Court of Equity—the public; as it is clear that it was understood that the Legion were under the protection of the British articles of war, and that Sir De Lacy Evans had accepted the command under this limitation. Of course the real excuse is to be found, if at all, in the difficulty of governing his strangely compounded Legion by ordinary means, and partly, perhaps, to his own inexperience;—

> Res dura, et regni novitas me talia cogunt Moliri.

On this, as on other occasions, the great and insuperable difficulty with those who are for the total abolition of flogging, has been to find a substitute. General Napier, as we observed in our last article upon this subject (Med. Gaz. vol. xxi. p. 854), prefers even picketting; a preference in which we are unable to agree with him. Several other substitutes remain to be considered. "*Death*," says General Napier, "is a punishment peculiarly suited to the military code*." If it is not freely inflicted, he adds, in time of war, the army becomes a mob, and victory is lost; "while the miserable inhabitants of the country would be submitted to a description of cruelty such as few men like to think of, much less to describe." Even in peace, he thinks that if mutinous conduct were permitted, the monstrous nature of an army would become more monstrous, and that it would devour the country. Hence, if flogging is abolished, mutiny must be more frequently punished with death.

Now, it is quite clear that *simplex mors*, or death without torture, is better than death by the lash. Much, therefore, might have been said in favour of shooting soldiers in the days when men were doomed to die by the cat-o'-nine tails; at present the choice is more dubious; and the current of opinion runs so strongly against capital punishments, that we will pass on to the next substitute.

There is a fashion in castigation, as well as in other things, and *solitary imprisonment* is just now vaunted as a panacea for all our moral sores. The true way of forcing on the Millennium, say many of our reformers, is to shut up the whole of our furacious population in separate cells; from the hoary forger of 70,

* Remarks on Military Law, and the Punishment of Flogging, p. 175.

to the budding pickpocket of seven, the remedy is infallible. It is true, that, like other infallible remedies, this commonly fails; and the prisoner condemned to a long period of solitary confinement comes out an idiot or a madman. But what matter? Our projectors, like the academicians of Laputa, are never in the wrong; and, whatever happens, can always demonstrate that they are right—upon paper! Hence, it is not surprising to hear long periods of solitary confinement recommended, in military as well as in civil life, as the true method of reforming criminals, and the best substitute for all debasing punishments. But it is rather curious to find it advocated on the score of humanity, in the following strain:—

"——— had he not been field-officer of the day in the garrison of Dublin, and seen men solitarily confined to the extent of human endurance—pale faces thrust out at holes, to show the visiting officer that the culprit was alive—and was he to be told, or being told, to admit, that men of education and talent could not maintain order without the assistance of the lash * ?"

Most people will allow that it would be better to give a man thirty or forty lashes, and have done with it, than to wire-draw resentment in this manner, and waste away the victim by inches. And this, too, on pretence of humanity and kindness! We suppose this is what is meant by killing a person with kindness.

Transportation is an obvious substitute for flogging, and has the advantage, as General Napier remarks, of freeing the army from ruffians. As a punishment, it has always seemed to us one of doubtful merit; for in the transportation either of civilians or soldiers we naturally fall into the following dilemma:—If the sentence were really executed in its theoretical rigour, the convict would be to all intents and purposes a slave; and it would seem in most cases very hard that a man transported for life should be working in chains in 1838, because he picked a pocket in 1814; or still more so, that a soldier should be the thrall for life of a farmer in Australasia, because, twenty years ago, he struck his serjeant.

We are well aware, that the other horn of the dilemma is the common one: the Australasian convict is better off in most points than the poor of England. His diet is, of course, better than that either of criminals, or of those in this country who have been convicted of the offence of indigence; and with tolerable conduct he may hope for a pardon. We speak of the goodness of his diet as a matter of course, as it would be absurd to suppose that, in a new country, where provisions are cheap, a master would have hungry discontented labourers on his estate, when he might have them satiated and satisfied. Hence the allowances to convicts are on a very liberal scale. Thus Sir F. Forbes, Chief Justice of New South Wales, in his evidence before the Select Committee on Transportation, says that he gives the agricultural convicts in his employment the same as the gentlemen upon the neighbouring farms do, namely, ten or even more pounds of flour, eight pounds of beef or mutton per week; frequently tea and sugar, and when not tea and sugar, two quarts of milk per day, two ounces of tobacco, with soap, salt, and annual clothing.

These facts are pretty well known in England, and transportation, in consequence, is not looked upon with much dread, though there is no doubt that, in the penal gangs, the existence of the convicts is full of hardships.

Rather a curious substitute for the commoner military punishments has been proposed; namely, *blistering:*

* Colonel Thompson, in the debate of April 7, 1837, on military flogging.

it is strongly recommended by General Napier, who says that it succeeded in two cases. The first was in the 14th regiment. In 1794 he heard Major-General Doyle say, that whenever a soldier of his regiment got drunk, he applied a blister to the man's back. This counter-irritant put a complete stop to drunkenness in the regiment, and consequently to flogging, for ebriety and flogging stand in the relation of cause and effect. The second instance occurred in Guernsey. An officer, who had heard what General Doyle had done, determined to repeat the experiment, and with the concurrence of the surgeon (now a celebrated accoucheur in London) had recourse to the emplastrum cantharidis. The first time that a man was drunk, the colonel and the surgeon went to the guard-house, felt the culprit-patient's pulse, and declared him to be in a fever. "Nothing could be more true; he was therefore put into a blanket, and four soldiers bore him through the barracks, his comrades all laughing at the care taken of him. On reaching the hospital the patient was put to bed and blistered between the shoulders, fed on bread and water for a week, and then turned out—'cured.'" When he made his appearance on parade the colonel congratulated him on his recovery, and sent him back to his company, where he was sufficiently laughed at. Other drinkers were cured in the same elaborate manner; and though the joke was an excellent one to the bystanders, the patients felt the point too keenly to relish it: there was the smart of the blister, with bread and water for diet, and no pity from their comrades. After a fortnight not a man got drunk; and not a soldier of that regiment was flogged after the drunkards had been put upon a course of blisters, or rather, the blisters had been put upon the drunkards. It must be confessed that the story savours something of the marvellous. It might be asked, why has the blistering system not been generally adopted in the service? Perhaps other colonels are too angry to joke; or perhaps they fear that a long string of jocose punishments might weaken their authority.

Another and more agreeable substitute consists in rewards—in stimulating the soldier's exertions rather by hope than by fear, and making his situation sufficiently desirable to render dismissal a severe chastisement. Some improvement has taken place on this score, but more remains to be done. At present Justice has her sword drawn to punish the soldier's crimes; but her scales seem rather destined to weigh his errors than his merits.

MESMERISM ON THE LOWER ANIMALS*.

We can find very little account of the effects really produced by Mesmerism on animals: it is generally *stated* that horses and dogs have given indications of magnetic influence; but it is clear that though the evidence on this part of the subject would be less exceptionable than that in which women, fallible and deceiving, are operated on, it is greatly deficient: we will therefore supply some.

All the phenomena that have been produced have followed on the exciting of fear, or some analogous feeling, in the animal, or on the soothing influence of the same monotonous impressions as we have shown to act on man—as gentle friction, &c. There are some facts, well known, indicating the operation of the former influence in a remarkable degree: among them is the fascinating power, as it was called, of serpents over small birds. The more powerful snakes are described as fixing their steady penetrating eyes on some creature at a short distance, as if marking it for their prey. As soon as their glance is perceived, the bird evinces the greatest agitation and alarm, flying from one branch of the tree to another, and screaming aloud, yet not daring to attempt an escape,

* It was intended, if our space had served, that this should have been embodied in the article on Magnetism last week.

till, overcome with fright, it falls, and becomes an easy prey. Serpents are in their turn under the influence of music, during which they are said to become at last so torpid that they may be handled without any fear.

The effect of looking steadily for some time at a horse, when his eye is fixed upon you, is well known. The most vicious may be tamed by holding them with a tight bit firmly under the chin, and keeping the eyes intently fixed on them. This was the plan employed by a celebrated Irish horse-tamer, who was commonly called the Whisperer. He used to enter the stable alone, in which a horse, he cared not how fierce, was stalled, and fix his eye resolutely upon him; when he turned his head round, he would walk quietly up to him, and by his halter draw his muzzle sharply round. Whatever resistance the horse made, he held him tight, and never took his eye off him. In a short time the contest was sure to be ended; and when the grooms were called in, the horse was found so quiet and tranquil that any one might approach him, and not unfrequently lying in his stall trembling and shivering with fright. Indeed, so great was the influence which the *Whisperer* had by his glance, that horses whom he had found with tempers that had baffled the management of every one else, were rendered spiritless cowards for the remainder of their lives.

The same influence is exerted over dogs. We have known a man who would approach the fiercest, even on his own territory, on hands and knees, looking steadily at him. No sooner did the dog perceive his fixed glance, than his courage was staggered; as the man, with unmoved coolness, came nearer, he would retreat; and we know that no dog had ever courage to make the least resistance even when taken hold of, but that the great majority used to run yelping off. In this metropolis, a similar thing may be seen daily. When an idle errand-boy sees a dog quietly lying in the street, he knows no better amusement than to point at him, and look him impudently in the face. At first the cur is only a little discomposed, but soon, like a magnetized lady, he grows very restless; then he begins to bark, erects his bristles, and looks fierce; but as the boy keeps his eyes quietly fixed on him, or, as an additional excitement (and this quite *secundum artem magneticam*), moves his fingers in little circles, the poor creature's courage rapidly oozes out, and he runs off, with his tail tightly flexed, as much degraded as if he had been well thrashed. Ladies' lap-dogs, like their mistresses, are often exceedingly nervous, and therefore, like them, very subject to magnetic influence, though we hope with a different result; for if they be steadily looked at for some minutes, they (we mean the lap-dogs) are not unlikely to be seized with incontinence of urine.

Other quadrupeds, and even birds, may be similarly influenced. If one person holds a chicken, with his beak down on the ground, while another draws a piece of red or white chalk, on which the bird has fixed his eye, slowly along the ground in a straight line forwards, he will keep his eye rivetted on the line immoveably, as if in a trance. So, too, if a fowl be laid on its side, and there held for a minute or two, and any light body be laid across its head or neck, it will remain perfectly quiet till again raised up, or otherwise disturbed.

Now we submit that it would be quite absurd to attribute all these effects to animal magnetism: they clearly arise from the feeling of fear which the sight of some unusual object, or of a resolute unmoved glance, excites in the animal; they are, we doubt not, a very small portion of those which every one in the habit of watching the manners of domesticated creatures is well acquainted with. But they are sufficient to prove that the effects (if any) which magnetizers have produced on animals, have been constantly produced by others who neither knew nor cared for the agents which they employed, though they were in reality the same as the magnetizers, self-blinded by their imaginations, have always used.

PHYSICAL SOCIETY—GUY'S HOSPITAL.

March 10, 1838.

Mr. Chapman, of Tooting, in the Chair.

Mr. Brereton read a thesis upon

Cerebral Physiology, founded on Cerebral Pathology;

the object of which was—

1st. To attempt an explanation of the manner in which inflammation of the me-

ninges produces such symptoms as usually attend it, and to controvert some of the prevailing opinions of the day respecting the origin of these symptoms.

2d. To show, from extensive pathological inquiry, that the cineritious portion of the true brain is that in which the intellectual faculties reside, and to suggest a plan for future inquiry on this subject, having reference to the mapping out of the brain.

3d. To show that the medullary portion of the true brain is a mere conductor of impressions to the cineritious; and of volition to the spinal marrow, the true source of mobility.

4th. To controvert the opinion of Foville and others, regarding the functions of several parts of the brain, such as the thalamus; and to suggest other explanations upon the point.

5th. To offer some pathological evidence in favour of a more extended range of functions derivable from the commissures than those usually assigned to them.

6th. To offer a few observations on the senses.

7th. To review Gall's opinion of the office of the cerebellum; to contrast it with that of Flourens and Foville, and to bring forward some facts in favour of the latter.

8th. To take a hasty review of the excito-motory system, as advocated by Dr. Marshall Hall; and to relate a few experiments upon human beings, made by himself, which appeared to militate against Dr. Hall's views.

Mr. Bransby Cooper commenced the discussion by requesting Dr. Marshall Hall's and Mr. Grainger's attention to the view which he had taken of the Doctor's discovery of the excito-motory system, and of Mr. G.'s dissections confirming them. He was anxious to be sure that he correctly understood these gentlemen.—Mr. Cooper having explained,

Mr. Grainger rose, and said, that the view taken by Mr. Cooper was perfectly correct. He would, however, enter into the explanation required of him, as the subject had been but recently examined, and many of the older practitioners might feel a difficulty in comprehending the exact state of the question. Anatomists, up to the present day, had been in the habit of describing the spinal nerves to arise on each side by a double origin. The posterior or sentient root and the anterior presiding over motion. To these Dr. M. Hall has added another pair of roots, which he has termed respectively the incident and the reflex; the former proceeding with the sentient root, and serving to convey impressions (independent of sensation) to the latter, which accompanies the motor root, and constitutes the conductor of involuntary motion.

Sir Charles Bell's discoveries had shewn that every nervous filament possessed but a single faculty, and wherever a nerve appeared to have two functions, it was necessarily made up of two sets of fibres. Dr. Hall's experiments had demonstrated, that when the brain was completely separated from the spinal cord, voluntary motion and sensation in the body were completely arrested; but upon the application of some irritation to the surface, the muscles could still be made to contract, independent both of sensation and of volition. Hence the Doctor reasoned that some chain of connexion existed between the nerves of motion and perception in the spinal cord, altogether irrespective of the brain, and such as have been already alluded to. Mr. Grainger had endeavoured to verify this theory by actual dissection, and had satisfactorily demonstrated the fact, that such connexion did indeed exist; for he found each root, as it arose from the spinal marrow—the anterior from the motor tract, and the posterior from the sensitive—to have also another origin deeper in the cord, from the central grey matter; whilst the original roots proceeded no further than the white medullary structure of the cord; the posterior of these origins, from the grey matter being the incident nerve, and the anterior the reflex nerve, of Dr. Hall. Thus, like the discoveries of Bell, had the more recent theory of Hall been verified by dissection.

The idea, indeed, was not altogether original on his part, for it had been already mentioned by Gall, Bellingeri, and Mayo, that the nerves of the spine arose not from the medullary alone, but also from the cineritious part of its structure. This had not, however, been fully admitted by anatomists; and although many investigations on the subject had been made, he was not aware that the fact had been previously satisfactorily demonstrated. He was happy to add, that his dissections had been now confirmed by those of Mr. Solly.

If these notions were correct, the quantity of brain and white part of spinal cord ought to bear a proportion to the development of sensation and volition; and that of the cineritious part should be in proportion to the development of motion and perception. And so indeed it was, this being fully verified by comparative anatomy, which shewed, that in man the brain and white part of the cord preponderated largely, whilst in animals endowed with strong motive powers the cineritious part of the spinal marrow prevailed.

Mr. Grainger illustrated these facts by

reference to examples from fishes; as the Lophius piscatorius, and from the lower classes of the animal kingdom; as the Annelidæ, the common earth-worm, insects, &c., by which it entirely appeared that the persistence of muscular movement bore a decided ratio to the development of the spinal and ganglionic systems. Mr. G. concluded his remarks by expressing his conviction, that dissections in anatomy had hitherto been too coarse, and the recent labours of Kiernan, Müller, and others, gave encouragement to hope that many parts of anatomical structure were susceptible of far minuter demonstration than as yet had been applied to them.

Dr. Marshall Hall rose in explanation of his views. In the cerebro-spinal system there were, indeed, two systems—the cerebral, containing the nerves of volition and sensation, and the spinal, which operated independently of either to produce motion. If the brain of an animal be removed, though motion may occur, yet such motion cannot be the result of volition, nor can any motion whatever be produced without the application of some external irritation. Thus, if a snake be decapitated, and then left till it be perfectly quiescent, though it may be made to move by external stimuli, yet if left to itself no motion will be observed again, and it will remain in whatever form it may have been placed. The movements which take place upon the application of external stimuli in the coma of apoplexy, opium, &c. &c., also in the case of an animal struck down by a blow on its head, indicated that there existed some other source of contractile power besides that of the brain, and independent both of volition and sensation. Reverting now to the paper, Dr. Hall said, that experiments enough were on record to shew, however much you may lacerate or tear the cerebrum, no convulsive movements will arise. It was a matter of great interest, therefore, to inquire why convulsions occurred in certain inflammatory and other conditions of that organ. With regard to the cerebellum, Serres, in speaking of an apoplexy there, and believing it to preside over the sexual passions, had prognosticated the disease from the existence of priapism. Doctor Hall did not believe this to be the function of the cerebellum: he thought the priapism might be explained upon the supposition that the clot pressed upon and irritated the adjacent medulla oblongata, the source of muscular movement—as in the guinea-pig, irritation of that part immediately produced a seminal emission. To apply this physiology of the excito-motory nerves, the Doctor adduced the case of the crowing inspiration of infants, which always depended upon one of three causes—dentition, constipation, crudities in the stomach. Removing the cause of irritation invariably relieved the crowing; in this affection the brain being only secondarily affected, and becoming diseased finally, because every convulsion renewed congestion there, and this ultimately gave rise to disease.

Mr. Brereton agreed with Dr. Hall in some points, but differed with him in others. He was not satisfied that sensation was not essential to the production of motion, for although the brain be removed, it had not yet been proved that sensation did not also reside in the spinal cord. In several cases of paraplegia, and various forms of palsy, in the hospital, he had lately endeavoured to excite motion by tickling, pinching, stabbing with a penknife the affected limb, &c. &c., but he had altogether failed in producing the result stated by Dr. Hall. An acephalous fœtus had been known to cry; this Mr. Brereton did not comprehend, for crying presupposes a sensation of something unpleasant, a certain exercise of the will. He agreed in the opinion that the cerebellum did not preside over the sexual appetites. With regard to the crowing of children, Dr. Hall had mentioned but three causes. Dr. Hugh Ley, however, has described a fourth, viz. tumors in the neck pressing upon the par vagum.

Dr. Hall replied, he thought there must be some error of observation in the experiments made by Mr. Brereton upon the paralytic patients, for it was quite proved that convulsion of a paralysed limb could be produced in the way he had stated. There was now a patient in the "Dreadnought," under Dr. Budd, completely paralysed; tickling the soles threw the limbs into strong movements, and passing the catheter occasioned priapism. With regard to the acephalous fœtus, there is but one case of crying recorded; it is by Ollivier, and the word used is "*crier*." This does not necessarily signify crying; it may mean a hiccup, or any noise in the air-passages. He differed altogether with Dr. Hugh Ley about the crowing in children, and did not believe that pressure on the pneumogastric nerves could occasion crowing; any pressure there caused stupefaction, as in excessive drunkenness, coma, dyspnœa, &c.; and in a case of the kind at the Salisbury Infirmary, Mr. Sampson had saved life by a well-judged operation of tracheotomy.

Mr. Aston Key suggested whether, in the cases of paralysis where Mr. Brereton had failed in exciting motion, the injury or disease of the cord had been low down,

at the end nearly of all the cineritious matter: might not this be sufficient to account for the failure?

Mr. B. Cooper said it was the same thing physiologically, whether the injury was in the spinal cord or in the cauda equina, which depended upon and arose from the spinal cord.

Mr. Brereton.—One patient had fracture of dorsal vertebra. A catheter was always in the bladder, but there was no priapism, and no movement was caused by pinching or blistering. Another had injury just below the head: here, too, he had failed in his experiments. On looking over the old museum-books of Guy's, Mr. B. had found there was once a patient, under Mr. Morgan, with injury at the sixth cervical vertebra. There was at first (as is usual) paralysis of the parts below the seat of injury. After a time, however, (some days,) the arms also gradually lost all power. He would ask Dr. Hall whether he knew of other cases where disease in this way seemed to affect parts above the seat of injury; and whether any analogous result could be produced in experiment by the agency of the excito-motory nerves.

Mr. Golding Bird stated, that he had electrified many paralysed patients in the hospital, and, in every instance, contortions of the limbs were produced by the electricity.

Dr. Hall had no information upon the question last put by Mr. Brereton: it was extremely interesting, and he had often thought of it; but he had never seen, or before heard of, any case bearing upon it. Mr. Bird's fact about electric fluid proved nothing; for electricity, or galvanism, acts directly on the muscular fibre, and not through the agency of the nerves.

Dr. Bright expressed his gratification at the very complete manner in which the excito-motory theory had been verified by dissection. The Doctor related a case which he had seen with Mr. A. Key, where perfect paralysis of the lower extremities existed, and in which violent convulsive movements could be produced by tickling the foot. He believed some explanation might be found for the failures experienced by Mr. Brereton: he did not think they were to be looked upon as militating against the excito-motory theory.

Mr. Grainger said that he believed Mr. Brereton's failures might be accounted for by the fact that the excito-motory influence was differently diffused (as regarded intensity) in different parts of the limbs; nature, who gave nothing in vain, had not afforded the excito-motory influence to parts that did not stand in need of it. Thus, though many parts of the leg were incapable of rendering involuntary contraction from irritation, yet the sole of the foot would at once yield the phenomenon. Mr. Verrall had made experiments upon a case under his care, showing this very completely. Mr. Grainger concluded by adducing illustrations of this point from comparative anatomy.

On the motion of Mr. Key, the discussion of this subject was adjourned to the next meeting.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Surgeon, Mr. Andrews.
Assistant-Surgeon, Mr. Adams.

Mar.	Sex.	Age.	Case.
6.	M.	27	Contused ankle.
	F.	2½	Burn.
7.	M.	37	Wounded knee and contused thigh.
	M.	14	Comminuted fracture of the humerus.
	M.	20	Fractured femur.
	F.	12	Injured thigh.
	F.	23	Scald.
	F.	11	Contused knee.
9.	M.	30	Fractured fibula.
	M.	45	Fractured ribs.
	M.	35	Wounded head.
	F.	20	Lacerated forearm, wound of radial artery.
	F.	8	Comminuted fracture of femur.
10.	M.	23	Fractured pelvis, lacerated bladder (death).
	M.	30	Dislocated (*a*) humerus and contusions.
12.	M.	57	Fracture of the anatomical neck of the humerus.
	M.	35	Injured leg.
	F.	30	Contusions.
	M.	45	Fractured ribs.
	M.	7	Necrosis of femur, (not an accident, but admitted as an extra case).
	M.	23	Compound comminuted fracture of tibia.
	M.	46	Fractured cranium, rupture of spinous artery, trephine (*b*.)
	M.	36	Wounded hand.

In-patients	23
Out-patients	42
Total	65

(*a*.) During this week five cases of dislocated shoulder were admitted—four into the axilla, and one forward on to the edge of the glenoid cavity. Of these, the four first were reduced by the house-pupils,

Messrs. Lewis and Vincent, by extension, the heel being placed in the axilla. The other slipped in by the action of the muscles of the shoulder, when the arm was raised and moved during examination.

(*b.*) This man was brought to the hospital in a state of perfect insensibility. He had fallen twelve feet into a ship's hold, and is supposed to have pitched upon his head. He was completely stunned by the fall. The following was his state on admisson:—Consciousness gone, not answering when loudly called to; pulse weak; skin cold; pupils a little dilated and acting but feebly. On the left side of the vertex there was a distinct mark of contusion with a cup like depression, into which you might insert the end of the thumb. This was regarded as the consequence of effusion of blood under the scalp; and the symptoms indicating violent concussion of the brain. The man was left until the evening, nine hours after admission, being visited in the interval by the house-pupils. I saw him at this time, and found him worse. The pulse had risen, but was still slow and labouring. temperature restored; still insensible; pupils non-accordant; irides almost motionless; right side of face flattened, from paralysis of portio dura; right arm evidently paralytic; breathing inclining to stertor; considerable swelling over the left temporal region, and the original depression had disappeared. A consultation having been called, it was determined at once to make an incision through the scalp at the part where the depression was first observed. A fissure was here distinctly seen, which, on further examination, was found to be the termination of a fracture apparently coming up from the base of the skull. As this must of necessity have traversed a groove containing a considerable branch of the spinous artery, the crown of the largest sized trephine was applied in such a manner as to include the fractured portion. A large coagulum was seen on raising the bone, and the pulsation of the brain became apparent. A part of the clot was gently scraped with the handle of the scalpel from the dura mater. A slight oozing of blood took place. The patient seemed more quiet after the removal of the bone, but manifested no consciousness. The irides acted now with much more freedom, and the pulse rose. The wound was slightly dressed. On the following day he was in no respect better, nor was there afterwards the least amendment. He was, at intervals, somewat convulsed. The medicines administered were, calomel gr. x. on the morning after the operation, with 5 grains at intervals of four hours; and a stimulating enema was thrown up in the evening. He died 48 hours after the operation.

Post-mortem.—A fracture, traversing the petrous portion of the left parietal, and making its way upwards across the squamous plate and parietal bone, had passed through a large groove of the spinous artery. Effusion of blood on the dura mater to the extent of at least half an ounce. Laceration of the substance of the brain at the basis of the right hemisphere. Contusions of the brain in the different parts, and marks, also, of effusion of blood occurring in dotted portions in the parts forming the walls of the lateral ventricles.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, March 22, 1838.

John Henry Brummell, of Brighton.
George Lestourgeon, of Cambridge.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Mar.* 27, 1838.

Age and Debility	29	Hooping Cough	3
Apoplexy	3	Inflammation	14
Asthma	19	Bowels & Stomach	1
Childbirth	3	Brain	2
Consumption	31	Lungs and Pleura	6
Convulsions	13	Liver, diseased	1
Croup	1	Measles	2
Dentition or Teething	2	Mortification	3
Dropsy	5	Paralysis	1
Dropsy in the Brain	4	Small-pox	10
Dropsy in the Chest	1	Sore Throat and Quinsey	1
Epilepsy	1	Thrush	1
Fever	10	Tumor	1
Fever, Scarlet	3	Unknown Causes	25
Fever, Typhus	7		
Gout	1	Casualties	6
Heart, diseased	6		

Decrease of Burials, as compared with the preceding week . . . 47

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

March.	THERMOMETER.		BAROMETER.	
Thursday . 8	from 31	to 47	29·99	to 30·14
Friday . . 9	23	49	30·21	30·09
Saturday . 10	26	46	29·94	29·85
Sunday . . 11	28	47	29·73	29 83
Monday . . 12	23	47	30·00	30·06
Tuesday . . 13	25	50	30·05	30 04
Wednesday 14	35	58	30·00	29.90

Winds variable. N.W. prevailing.

Except the 9th, 11th, and 12th, generally cloudy. A little rain on the 13th and 14th.

CHARLES HENRY ADAMS.

NOTICE.—The binder is requested to observe, that the ten pages *extra limites*, as well as the Index, stitched up with this number, belong to the preceding volume.

WILSON & SON, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 7, 1838.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine,

By Southwood Smith, M.D.

Physician to the London Fever Hospital.

Lecture X.

Apoplexy from Derangement of the Circulation of the Blood in the Brain—So transient as to leave no Indication of its Existence after Death—Simple Apoplexy—Apoplexy with Vascular Turgescence; with slight or with extreme Congestion; with Disease of the Choroid Plexus; with Serous Effusion; with Extravasation of Blood—Successive Changes of the Effused Blood, and of the Cerebral Substance.

Apoplexy from derangement of the circulation of the blood in the brain.—The enormous quantity of blood with which the brain is supplied, every particle of which enters it by means of the pia mater, on the extended surface of which are spread out myriads of the finest capillary veins and arteries, flows through these delicate vessels easily, regularly, with a determinate velocity and force. It is essential to the sound structure and the healthy function of this organ that it should constantly receive a certain supply of arterial blood, and that it should be able to transmit from it with facility a corresponding quantity of venous blood. If the arterial blood sent to it in a given time exceed a certain quantity, or if the venous blood returned from it in a given time do not amount to a certain quantity, specific disorders of function and diseases of structure take place. The morbid changes induced by derangements of the cerebral circulation are now to a considerable extent clearly and certainly ascertained; and with what has been ascertained it is essential to the due performance of your duties as medical witnesses that you should be thoroughly acquainted.

We know, for example, that numerous circumstances are capable of disturbing the normal circulation of the blood through the capillary blood-vessels of the brain. On the one hand, a preternatural quantity of arterial blood may flow through the vessels with preternatural velocity and force; on the other hand, from some impediment to the due return of the blood, there may be retardation of the venous current, and a consequent accumulation of blood in the venous capillaries. Such changes in the cerebral circulation are denoted first by the different changes of colour in the cerebral substance which have been already explained; by the rose red, or the deep crimson colour, communicated when the derangement is in the arterial capillaries; and by the dusky, grey, or purple colour, imparted when the derangement is in the venous capillaries: secondly, by the manifest inordinate distension, sometimes of the large branches, but more often of the minute divisions of the blood-vessels; by the apparently increased number of blood-vessels, in consequence of branches which in the normal state are so minute that they are capable of admitting only the thinner colourless portion of the blood, and which are therefore invisible, now enlarging sufficiently to admit the red particles, those particles rendering the containing vessels manifest to the eye; by the numerous large drops of blood, forming so many bloody points, which become visible when an incision is made into the cerebral substance, and which always indicate a preternatural accumulation of blood in the arterial blood-vessels: and, thirdly, by the actual effusion of serum or of blood in different parts of the brain.

Among the evils consequent upon these morbid conditions of the blood-vessels of the brain, one of the most important, one the most frequently and suddenly fatal, is the disease termed apoplexy, which we are now to consider in its relation to forensic medicine. The essential anatomical character of apoplexy is a preternatural accumulation of blood in the cerebral blood-vessels; its essential physiological character is the perfect abolition of sensation and voluntary motion as the direct consequence of such preternatural accumulation of blood in the cerebral blood-vessels.

So transient as to leave no indication of its existence after death.—Now it is established by indubitable evidence that a disturbance of the cerebral circulation to such a degree as to constitute true apoplexy, capable of proving suddenly fatal, may be of so transient a nature as to leave not the slightest appreciable trace of its existence after death. If this be an ascertained fact, it is one of the highest importance in relation to forensic medicine.

One of the most instructive examples of such a sudden and transient accumulation of blood in the cerebral blood-vessels is afforded by those cases in which there is an obvious mechanical impediment to the return of the blood from the brain threatening instant death; and in which, on the removal of the mechanical impediment, the healthy functions of the brain are immediately restored. A boy had drawn his neckcloth remarkably tight; he was whipping his top, stooping down and rising alternately; after a short time he fell down suddenly apoplectic. The neckcloth was unloosed; blood was freely drawn from the jugular vein, and he speedily recovered. The accumulation of blood in the brain in this case caused by a mechanical impediment to its return, had the obstruction continued, would, perhaps, in a few minutes, have increased to such a degree as to induce fatal apoplexy.

Cases are on record which demonstrate that an accumulation of the like transient nature, yet capable of proving rapidly fatal, may take place from internal causes. A person may be apparently in perfect health. While engaged in his ordinary occupation, he may fall down, suddenly deprived of sense and motion: after lying for some time in a state of coma, from which nothing can rouse him, he expires: on the most careful examination of the brain, not the slightest deviation can be detected from its healthy structure. An event like this may give rise to a violent suspicion of poisoning; and unless you are well prepared for the true nature of the case—a case which, without previous consideration, you might think not likely to happen—you may be in doubt about it, you may fall into error respecting it, you may defeat the ends of justice, and bring lasting disgrace on your own professional character.

Dr. Abercrombie has collected a number of cases which place this point beyond all dispute, and which demonstrate that death may take place with the ordinary symptoms of apoplexy, while on the most careful examination of the brain after the fatal event, not the slightest morbid appearance can be detected in any part of the cerebral substance, or in any of its investing membranes. Similar instances are recorded by other authorities. A woman, aged about thirty, of a full habit, some years before her death had been affected with symptoms in the head, accompanied by impaired speech and partial loss of recollection. Some effects of this attack had continued for a considerable time, especially in her speech, but by degrees she had perfectly recovered, and enjoyed excellent health for a long period preceding the attack now to be described. She was stooping over a washing-tub, when she was seized with a violent fit of sneezing; she almost immediately became insensible, and would have fallen down had she not been observed and supported by some persons standing by her, who carried her to bed in a state of perfect apoplexy. All the usual remedies were employed in the most active manner without the least effect in alleviating any of the symptoms; she lay with all the symptoms of the most perfect apoplexy, and died on the following day. On inspection, no vestige of disease could be discovered in the brain or in any other organ.

A man, after complaining of headache and giddiness, fell down in a state of insensibility, with some convulsion. He then lay in a state of profound apoplexy for forty-five hours, when he died. On the most careful examination of the brain not the slightest morbid appearance could be discovered.

A young lady, after appearing for one day heavy and disposed to sleep, fell into a state of perfect coma, which was interrupted only by occasional attacks of general convulsion. Without any change in the symptoms she died on the third day. After the most minute examination no morbid appearance could be discovered in the brain.

Dr. Duncan records a similar case which occurred to him in the Edinburgh Infirmary. A man, aged 54, of a plethoric habit, and short-necked, was admitted into the clinical ward in a state of nearly perfect coma, speechless, and with palsy of the right side, to such an extent, that even the intercostal muscles of that side did not act. The leg and arm of the left

side were occasionally affected with convulsive motions. Breathing stertorous; deglutition much impaired; pulse 74. The affection was of three days' standing, and had come on with vertigo, loss of vision, violent headache, and vomiting. All the usual remedies were employed in the most active manner without benefit. He died on the fourth day, without any change in the symptoms. A most minute and careful examination was made of the brain, without discovering any appearance of disease, except that the choroid plexus seemed rather darker than usual, and the basilar artery was diseased at one spot. By the side of the artery there was a spot of the cerebral substance, no larger than a barley-corn, which appeared somewhat softened; but even this Dr. Duncan considered as extremely doubtful.

Simple apoplexy.—These cases, to which it would be easy to add others, afford decisive evidence that there is a modification of apoplexy which is capable of proving fatal without leaving any appreciable trace of its existence after death. It is this form of the disease which is termed simple apoplexy. It was at one time thought to be a common affection; but though you cannot doubt its existence, and must be always prepared to meet with it, yet its occurrence is certainly rare. M. Rostan states that in the course of his pathological researches he has examined, with a special reference to this affection, no less than 4000 heads, and never met with an example of it. Accurate observers in our own country, and those of large experience, speak, some of having met with a single case of it, and others with one or two. Suppose a person to die suddenly of this disease; suppose the circumstances connected with the death to have given rise to a suspicion of poisoning; you examine the body; you find no evidence of poison; you are satisfied, from the absence of all the characteristic signs, that no poison has been administered; yet you can detect no trace of disease in the brain. What, then, is the cause of death? You affirm positively that the death has not been produced by poison; but you are required to assign the true cause of death. Simple apoplexy—is that the true cause of death? Inquire into the history of the disease. Nothing of that history is known; at least you can obtain no information to be relied on. Next, examine carefully the state of the viscera of the chest and abdomen; if, which may possibly be the case, you find all these organs also entirely free from disease, in this case, with no account of the symptoms that preceded death, and with no morbid appearance either in the brain or in the thoracic or abdominal viscera, you can only arrive at the conclusion, that simple apoplexy is the probable cause of death. You can affirm nothing positive, and you should show why you cannot; but you may further state what you conceive to be the probable nature of the case; and assign your reason that, although simple apoplexy, thus proving fatal without leaving any trace of its existence in any organ whatever, be not a frequent event, yet that it may and does occasionlly occur.

But, on the other hand, the history of the disease is known. The attack was preceded by giddiness, loss of memory, headache, or some other of the premonitory signs of apoplexy; while the attack itself consisted of coma, with or without convulsions. You may now safely conclude that the cause of death is simple apoplexy. That conclusion will be placed beyond all doubt, if, on examining the thoracic and abdominal viscera, you find these organs to be the seats of certain diseases; for it is fortunate for the subject of forensic medicine, that while simple apoplexy is a rare affection, it is still more rarely met with as an idiopathic disease; it is much more frequently the consequence of some malady seated in a distant organ. Catarrh, bronchitis, asthma, anasarca, serous effusion into the thorax and abdomen, irritative fever, however induced, often precede it, and after exciting symptoms more or less acute, and varying according to the nature of the disease, the apoplectic seizure suddenly supervenes. For example, a lady, aged 45, for some months past had been in a state of ill health. One evening she was observed to talk hurriedly and incoherently; the next morning she was in a state of stupor, from which at first she could be partially roused; soon after mid-day it increased to perfect coma. From that time she lay in a state of profound apoplexy, with stertorous breathing and much moaning; the face rather pale; the pulse 72, and of good strength; and she died on the following morning. No disease could be detected in the head, after the most careful examination; but in the thorax and abdomen there was some effusion. In the heart there was considerable hardness about the root of the tricuspid valve. No morbid appearance could be discovered in the other viscera.

We thus learn from experience that such an effusion into the thorax and abdomen, and such a disease of the valves of the heart, slight as these affections may at first view appear, are sufficient to prove fatal, by producing the state of the brain designated by the term simple apoplexy.

But perhaps the most common affection

seated in a remote organ, and associated with simple apoplexy, is the irritative fever arising from the presence of worms in the intestines. A young man was seized suddenly with an attack of headache; to this succeeded vomiting and loss of speech; he fell into a state of deep coma, in which he died. On examining the body, no trace of disease could be detected in the brain; no indication of disease could be found in any other organ, but a prodigious mass of worms was found in the small intestines.

From the whole of what has now been stated, then, you can hereafter be in no danger of overlooking or mistaking this, to our subject, very interesting modification of apoplexy. When you meet with a case of sudden death, without any indication of disease of the brain, or in any other organ, and without any evidence of the presence of poison, you may infer that the probable cause of death is an attack of simple apoplexy. You may infer that this is the real cause of death, with a high degree of probability, if you ascertain that the symptoms preceding death were those which characterize the apoplectic state; and you may affirm that it is certainly the cause of death if, together with the presence of the apoplectic symptoms, you discover in the thoracic or abdominal organs any of the marks of inflammation, or any of the obvious sources of irritation which have been described. The evidence will now be complete that it is a case of simple apoplexy—a case of death of the brain, either as an idiopathic disease, or from irritation in some distant organ.

Apoplexy with vascular turgescence.—If apoplexy be capable of producing almost instantaneous death, without leaving the slightest trace of its existence after the fatal event, you will not wonder that it should occasionally prove mortal, with only slight indications of its presence. Any real indication of the presence of apoplexy, however slight, is of great importance in reference to forensic medicine; and how very slight the indication may be, which nevertheless may be quite sufficient to authorize a positive conclusion as to the existence of the disease, it is essential, to prevent your falling into practical errors of the gravest consequence, that you should be fully satisfied. With an apparently sound condition of every other organ of the body, the only appreciable disease in the brain, after a fatal attack of apoplexy, may be very slight vascular turgescence. Perhaps the condition of the vessels alter at the moment of death, or immediately after the fatal event; but however you may account for the fact, of the fact itself it is of the greatest importance that you should be well assured. Satisfactory evidence of it is afforded by the following case, to which it would not be difficult to add others:—

A gentleman, aged 24, had been observed for some days to be dull and drowsy, and he frequently complained of his head. Not having appeared at his usual time one morning, his friends went into his room, and found him lying across his bed half-dressed, in a state of perfect apoplexy. The attack was evidently recent, and it was supposed that he had been seized while he had stooped over his basin in washing. His face was rather livid; his breathing stertorous; his pulse slow, and of good strength. All the usual remedies were applied with assidnity, but through the day there was no change in the symptoms. In the course of the night he recovered considerably, so as to know those about him; but in a short time after he relapsed into coma, and died early on the following day, little more than twenty-four hours after the attack. There was a slight turgescence of the vessels on the surface of the brain; no other appearance of disease could be detected after the most careful examination. All the other viscera were in a healthy state.

Now suppose the circumstances connected with this case to have given rise to a suspicion of poisoning. Suppose these suspicions to have attached with no inconsiderable force to a particular individual; circumstances growing out of the relation between the deceased and the accused, and belonging to the class which we have already fully described under the head of moral evidence, or connected with certain facts known to have taken place a short time before the death of the deceased—such as that the accused was about the person of the deceased, that he prepared his last meal, that he gave him, with his own hands, the last portion of food of which he partook, or administered the last medicine he drank; suppose the premonitory symptoms, that the deceased, "some days before the attack, had been dull and drowsy," and that he had suffered considerably "from headache," had been overlooked; suppose even that the condition in which the deceased was found, "a state of perfect apoplexy," to have been unknown, or involved in doubt—as might easily have happened had a longer time elapsed before his friends obtained access to him, and as would certainly have happened had death actually taken place before he was discovered; suppose, finally, the only facts of the case actually known to have been, that the deceased was observed to be in his usual health at a given hour; that at that time he was last seen

in company with the accused, and that twenty-four hours afterwards he was found dead; suppose, under these circumstances, on a careful examination of the body, you find no evidence of the presence of poison, and no indication of disease in any organ, excepting a slight turgescence of the vessels on the surface of the brain; now, however strong the suspicions against the accused may be, it will be in your power completely to remove them. From this morbid appearance of the brain, slight as it is, you may infer with certainty that it is a case not of poison, but of natural death from apoplexy, with slight turgescence. And this being the only correct conclusion at which you can arrive, it will be your duty to state it, without hesitation or indecision, as a positive fact; because one of the most important duties of your office as medical witnesses, is to silence idle rumor, and to rescue the falsely-accused from ungrounded suspicion.

With slight or with extreme congestion.—From the slight turgescence described in the preceding case, there may be all degrees of vascularity represented in the drawings which I now show you—that is, every degree, from a slight fulness of the vessels to the most extreme congestion. This turgescence, or congestion, may affect either the membranes or the surface or the substance of the brain. In proportion to the degree of turgescence, or the intensity of the congestion, the evidence of the existence of apoplexy may become more manifest, but it does not thereby become more real. I repeat, the more intense the congestion, the more *obvious* the condition of the brain connected with the apoplectic seizure; but though the congestion be slight, it may afford evidence of the apoplectic state just as real. Not long ago, indeed, it was supposed that the effusion of serum, or the extravasation of blood, was essential to a fatal attack of apoplexy; but cases long since observed by Morgagni, and successively accumulating by the record of careful observers since his time, have at length satisfied all who have attended to the subject, that this opinion is wholly inaccurate. It is expressly stated by Baillie, that "the milder forms of apoplexy depend upon a distension of some of the vessels of the brain, from undue accumulation of blood in them. I have known (he continues) one instance of fatal apoplexy, where many of the vessels were found, upon examination after death, to be much distended with blood, but no blood had been extravasated in any part of the brain." The phraseology of this passage, that the milder forms of apoplexy depend on the undue accumulation of blood in the vessels, and that one instance had been observed of fatal apoplexy, in which there was no extravasation of blood, sufficiently indicate what the prevalent opinion was at that time. Modern observers have placed the fact beyond all doubt, that fatal attacks of apoplexy are by no means of unfrequent occurrence, in which the only morbid appearance after death is vascular turgescence, or congestion.

Sometimes this congestion of the cerebral vessels, inducing fatal apoplexy, arises primarily from a disordered circulation in the brain itself; but oftentimes it is a secondary affection, produced by obstructed circulation in some distant organ—as the mucous membrane of the bronchi, the substance of the lungs, the heart, the liver, and so on. Many persons die apoplectic, from a fatal congestion of the brain, indirectly induced by bronchitis, asthma, peripneumonia, hooping-cough, and various affections of the heart.

Whenever, then, you are called to examine a case of sudden death, and find congestion of the brain, inquire whether the signs of vascular turgescence were present during life—headache, drowsiness, giddiness, inaptitude for mental exertion, diminution of physical power, failure of memory, loss of sensation, or depraved sensation; loss of voluntary power, or even temporary numbness in any muscle or set of muscles. If these were the symptoms during life, and the brain after death present a high degree of congestion, all the other organs of the body being sound, of course you will have no hesitation in assigning the cause of death to be primary apoplexy from congestion. If, on the contrary, such were not the symptoms during life, but instead of these there were present symptoms characteristic of disease of the lungs, heart, liver, intestines, and so on; and if, on examining these organs, you find them to be the seats of such diseases as are ordinarily denoted by such signs, you will have no hesitation in assigning the cause of death to be secondary apoplexy from congestion—that is, apoplexy from congestion induced by obstructed circulation in some distant organ.

A high degree of congestion of the brain is often induced when death is caused by external violence, by suspension, by suffocation, and by certain poisons. In the cerebral congestion itself, there is no appreciable difference whether it arise from natural disease or be induced by external violence; you must look, for the diagnosis, to other sources; all of which will be fully considered hereafter.

Though the most intense degree of congestion take place as the result of inflammation, as when the injection of the arterial capillaries communicates to the

membranes, or the substance of the brain, a rose-red or a deep crimson colour, yet a fatal degree of congestion may result from the very opposite state—a state of debility, which is favourable to irregular accumulations of blood. In this way chronic diseases, and the manifold causes which produce an enfeebled state of the system, predispose to, and after a time often suddenly induce, congestive apoplexy; while precisely the same result follows from causes which act to a certain degree mechanically—as diseases of the heart and lungs.

Whatever determines a preternatural quantity of blood to the brain, or renders its transmission through the cerebral vessels unusually difficult and slow, may cause an effusion of serous fluid, or of blood. The apoplectic state may be associated with either event: the first constitutes serous, and the second sanguineous apoplexy.

Serous apoplexy.—The apoplectic state is constantly found connected with the effusion of serum between the membranes of the brain, on its external surface, into its ventricles, or at its base; affording a cause of natural death more obvious than either of the preceding conditions of the brain. Sometimes the quantity of effusion is small, not exceeding a few drachms; at other times it amounts to several ounces, or even pounds. It is remarkable that the quantity of fluid effused bears no proportion to the severity of the apoplectic attack. Sometimes the apoplexy is intense when the quantity of fluid is small; at other times the apoplexy is slight when the quantity of fluid is large; and at other times there are no apoplectic symptoms whatever, when the effusion is most extensive. The inference is, that the effusion is an accidental circumstance, and not the cause of the apoplexy; that the cause of the apoplexy is a certain morbid condition of the brain, which is essentially the same whether the brain be without any morbid appearance whatever, or whether it present the appearance of turgescence, vascularity, congestion, or effusion. The condition of the brain which gives rise to apoplexy, may, as I have said, be more obvious when it is attended with manifest turgescence, vascularity, congestion, and effusion; but it is of the last importance, in relation to forensic medicine, that you should be satisfied that these appearances do not constitute the disease, and are not essential to it, but that it may exist just as really, and operate just as fatally, when, on the most careful inspection of the brain, you can detect no morbid appearance whatever.

Apoplexy with disease of the choroid plexus.—There is, however, one morbid appearance distinctly appreciable, which you may often observe, and the true indication of which it is important that you should fully understand. It has been clearly pointed out by Dr. Bright, who has also stated its possible importance in relation to forensic medicine. The choroid plexus is not unfrequently the subject of a change of structure which you might easily overlook, or to which you might not attach due importance, without a previous knowledge of its real connexion with the state of apoplexy. In the choroid plexus, on both sides, is often found a number of small vesicles; sometimes, instead of vesicles, minute granulated bodies, and at other times yellow rounded masses of the size of a small bean, consisting of hard matter. Whatever be the immediate origin of these bodies, their formation seems to be intimately connected with such disarrangements of the circulation as give rise to apoplexy. They are found connected with apoplexy from turgescence, vascularity, congestion, effusion, and even extravasation; and occasionally they constitute the only morbid appearances that can be detected. They are so often found in connexion with these obviously morbid conditions of the brain, that when they occur alone they may be taken as evidence of the existence of some fatal change in the cerebral circulation.

In a case which came under judicial consideration, an individual narrowly escaped suffering punishment for murder, when it was ascertained by dissection that the death in question had been caused by apoplexy, with slight effusion. Tumors of this kind, of the size of marbles, were detected in the choroid plexus. Save these tumors, and very slight effusion, no morbid appearances were discoverable.

Another person was tried on the suspicion of murder. On examining the brain of the deceased, in the choroid plexus were found several of these tumors. This was deemed satisfactory evidence that death had been produced by a natural cause.

Apoplexy with extravasation of blood.—But the most unequivocal, and at the same time the most common form of apoplexy, is that which is attended with an extravasation of blood in the brain. The blood effused may be found between the bones of the cranium and the dura mater; between the dura mater and the arachnoid; beneath the pia mater; in any portion of the cerebral substance, in the ventricles, or at the base of the brain. But according to Dr. Bright, the situation in which it takes place more frequently than in any other, is a little to the outside of the corpus striatum in either hemisphere, just at the point where many large vessels may be traced coming off from the branches of

the fossa sylvii and pervading the brain. These vessels are often seen peculiarly turgid in cerebral congestion without rupture, forming numerous distinct spots in the horizontal section of the brain.

The source of hæmorrhage may be the rupture of a single vessel of moderate size, or of a large trunk; and the lacerated vessel may be distinctly visible on the membranes, on the surface, in the ventricles, or deeply imbedded in the substance of the brain; but this is by no means common. It is seldom possible to trace the effusion to particular vsssels. Commonly, indeed, the blood appears to flow from numerous points at once, probably the capillary terminations of a diseased trunk. Occasionally, it would appear that the effusion takes place by exhalation without rupture. The diseases of the blood-vessels which predispose to rupture, and consequently to apoplexy from extravasation, have been already fully detailed.

The quantity of blood effused varies from that of a minute point to a coagulum as large as a pea, a walnut, or an egg; and from the latter to a clot extending over a large portion of the surface of the brain, occupying the greater part of its substance; completely distending its ventricles, covering its base, and even filling the canal of the spinal cord.

On escaping from its vessels, the blood forces its way through the cerebral substance, breaks down its fibres, and forms for itself a sort of hollow or cell, in which it coagulates. The fluid, and still more the coagulated blood, acts upon the cerebral substance with which it is in contact as a foreign body, softens and disorganizes it, and often there are observed numerous small bloody portions of brain immediately surrounding the apoplectic clot.

Successive changes of the effused blood and of the cerebral substance.—After some time the effused blood and the cerebral substance which immediately surrounds it, and which is injured by it, undergo certain changes, with which it is important in a judicial point of view that you should be familiar.

According to Dr. Craigie, when the clot of blood begins to change, it assumes a brown, or brownish black colour, and is separated into fragments floating in a wine-lee-fluid. The further dissolution of these forms a homogeneous chocolate-coloured matter, which is eventually removed more or less perfectly; while the part which the extravasation converted into a hollow is filled with serous fluid, and softened or pulpy cerebral matter. In some cases of complete recovery, this is gradually converted, by a slow process of adhesive inflammation, into a membranous substance, harder than the surrounding brain, which, however, is generally softer than sound cerebral matter. In this manner are formed the cavernous sacs described by Wepfer and Morgagni, and the cavities described by Baillie, Wilson, Abernethy, and others.

The change in the structure and consistence of the brain surrounding the clot forms one variety of softening (*ramollissement*), or pulpy disorganization.

According to Dr. Bright, sometimes there is no effort at reparation or restoration; but the surrounding parts gradually soften down, while in other cases the mingled mass of blood and comminuted brain remains for many months, forming a soft mass, there being no sufficient power in the constitution to produce reparation or absorption. But in other cases all the injured portions of the brain appear to separate; a smooth surface is formed, with more or less vascularity, derived partly from the natural vessels of the brain, and partly from newly-formed vessels. Thus gradually a kind of cyst is generated, as a lining to the cavity produced by the clot. Then a process of contraction and absorption goes on, till, according to the greater or less powers of the constitution, and the admixture of cerebral matter with the blood effused, either a small quantity of watery fluid remains, which diminishing, the walls of the cavity coalesce, or a more solid yellowish-white substance interposes, forming a permanent cicatrix in the brain.

The period of time required for the completion of these changes varies greatly. In a case which proved fatal on the sixth day, these changes had commenced, and made some progress. In another, fatal on the tenth day, the clot had undergone considerable change in its colour, and, with the broken cerebral matter, was separating from the more sound brain; but in another, even in twelve days no such favourable change had begun. In a case where death occurred after twenty-three days, considerable change had taken place. In seven weeks a distinct lining membrane had formed, of an opaque white colour, and so solid as to allow of being detached from the surrounding brain. After about a year a cicatrix was formed, a little soft in the centre, proceeding to the obliteration of the cavity. In another case, after an equal period, the soft and disorganized mass showed no tendency to undergo a favourable change.

You see the great importance, in a judicial point of view, of attending carefully to the successive changes which the effused blood undergoes. Apoplexy from extravasation is not invariably fatal. The different constituents of the clot may be successively absorbed, and the cell only remain. A person having recovered from an apoplectic attack, with an apoplectic

cell still in his brain, may die of poison. You examine the brain. You find an apoplectic cell. If satisfied that you have now discovered the cause of death, you search no further; you may overlook the real cause of death, and the poisoner may escape. From the discovery of an apoplectic cell, you can conclude with certainty that death has been produced by apoplexy with extravasation then only when the cell contains recent blood; or when the cerebral substance around it is in a state of softening; or when it exhibits some unequivocal signs of recent inflammation.

CONTRIBUTION

TO THE

HISTORY OF THE NERVOUS SYSTEM.

Read before the Medical Society of University College, March 30th, 1838.

By John Durance' George,

One of the Presidents.

(For the Medical Gazette.)

1. This contribution to the history of the nervous system owes its origin to a remark made by Professor Sharpey in lecturing upon the reflex or excito-motory function, "that, of all previous physiologists, Prochaska had approached most nearly in his views to those at present entertained"—a remark which caused some surprise, when I recollected that in Dr. Hall's account of the "Opinions of former Physiologists *" no such author is mentioned, or even alluded to. I was therefore obliged to seek information as to Prochaska's views from his own writings; and, in pursuing my investigations further, soon found that Dr. Hall's account was very imperfect; and as the subject is at present exciting much interest, I have thrown the results of my reading into the present form, more, however, with the hope of supplying some few of the deficiencies, than the idea of rendering the history complete.

2. The remarks about to be made are arranged under two principal divisions, viz.—I. The phenomena observed; and II. The explanations given of them, together with their generalization.

I.

3. For the sake of convenience, a further division has been made of the phenomena—into experimental, natural, and morbid.

4. As to the *experimental* phenomena, many which are explained by and illustrate the reflex function of the nervous system have been long known and speculated upon; they are, in fact, as Dr. Hall justly observes, "amongst the oldest of physiological experiments; though, indeed, the justice of the continuation of the remark is not quite so obvious, "nevertheless they are totally without application to physiology *." Contractions or motions persisting in separated limbs, or portions of the body, or in the body itself, after the removal of important viscera, have always excited much attention. Thus, Aristotle † mentions that insects live after being cut in two, and tortoises lived and moved after removal of their hearts; and, indeed, these phenomena were so well known as to be alluded to in the works of Ovid, and others of the poets; whilst in more modern times they were noticed by Cardan ‡ (1550), Scaliger §, Kraasius, Bacon ||, and Moses Charras ¶ (1668).

5. Up to this time, however, we find no very particular reference to the continuation of these motions after the removal of the brain or other parts of the nervous system; but in 1671, Robert Boyle ** mentions that tortoises lived for some time after decapitation; that vipers moved quickly on being pricked, though two or three days after deprivation of their skin, head, heart, &c.; and female moths not only admitted the male, but also laid eggs, after decapitation. Glisson ††, in 1677, devotes a chapter to Irritability, in which he speaks of the "tumultuous motions sometimes remaining in decapitated animals" as referrible to the cases in which no suspicion of sensation can be entertained. Redi ‡‡, in 1684, found that a tortoise, whose brain he extirpated in November, lived till the following May, continuing to move and walk about to the day of its death. Another decapitated tortoise moved its fore and hind legs with force as often as it was pricked, and was convulsed: it did not, however, walk about like the former, though it lived thirty-three days. Woodward §§, in 1726, relates many experiments of this kind: he noted active motions in the body of a snake, after taking off the head, and in each section of the body when *it* was divided; chickens and pigeons lived and moved after scooping out as much brain as could be got at conveniently, and "mashing" the

* Vide Memoirs on the Nervous System, p. 53.

* Lancet, vol. 1, for 1837-8, p. 652.

† In lib. de juventute, senectute, vita, morte ac respiratione.

‡ De Subt. Norimb. (1550).

§ Excercit. ad Cardan.

|| Histor. Vitæ et Mortis.

¶ Hist. Nat. des Animaux, &c.

** Usefulness of Physiological Experiments.

†† De Ventriculo et Intestinis, p. 170.

‡‡ Observationes circa Animal. Vivent. (1708), pp. 209, 210.

§§ Essay toward the Natural History of the Earth, with a Continuation and Supplement. Lond. 1726.

remainder; frogs "leaped and creeped" body, and pushings forward, as if he intended to take a leap;" whilst the power of stinging possessed by the separated tails of wasps is also noted. Abraham Kaau Boerhaave *, nephew of the celebrated physician (1746), says that a young cock, whose head he cut off with a sharp razor, as he was running with great eagerness to his food, went on, in a straight line, 23 Rhinland feet, when he was stopped by some obstacle. Whytt † (1751) noted the convulsions produced in the hind legs of a frog, by irritating the spinal cord after division of the body, and especially that these motions were much diminished by the effects of opium ‡. (He also repeated many of the experiments which have been already mentioned, but which I shall not again detail, as in noting the observations made by each individual I only point out such as had not previously been made known). Sir Gilbert Blane § (1788) made an experiment on a kitten, which proved similar phenomena in warm-blooded animals. Legallois || (1812) varied the experiments upon the frog, by making sections of the spinal cord, and noticing that the parts corresponding to each division, when stimulated, moved in a manner that proved them to be quite independent one of another; which Mr. Mayo ¶ (1823) proved more clearly, by making a section at once in the neck and in the back, and thus producing "three unconnected nervous centres," in the parts connected with each of which motion was produced on the application of a stimulus. Mr. Mayo also produced motion in the iris of a pigeon (from whose head the cerebrum, cerebellum, and medulla oblongata were removed), by dividing the optic nerve, and irritating that portion of it adherent to the tubercles. To these experiments Dr. Marshall Hall ** (1837) has added a great number that are highly interesting and important—viz., in the *head* of a decapitated turtle—closure of the eye on touching the lid; convulsive contractions of the muscles of the larynx, on passing a probe up the trachea into that part; opening of the mouth and descent of the submaxillary fringes on touching the nostril; closure of the larynx on touching a part immediately within the teeth of the upper jaw, or the nostril; all which phenomena ceased on removing the brain: in the *body*, firmer contractions of the sphincter on irritation with a pointed instrument, and many of the experiments before mentioned, viz. motions in the limbs, &c; all which, together with muscular tone, ceased to be produced on withdrawal of the spinal cord. He has also noticed the diminution of these phenomena by opium and hydrocyanic acid, and increase of them by strychnine, with the cessation of the artificial tetanus thus brought on, immediately on the destruction of the spinal cord; excitement of the sphincter in a horse after it had been stunned with a pole-axe; motion in both the anterior and posterior fins of a turtle on irritating the middle of the undivided spinal cord, or an intercostal nerve; and motion in the anterior fins on irritating the upper part of a divided spinal cord; motions of inspiration on irritating the lower end of the medulla oblongata in the turtle, and also in the rabbit; movements of the muscles supplied by an interganglionic nerve in a lobster, and those muscles alone, when the nerve was irritated; but motions both anterior and posterior to the part stimulated, when irritation was applied to a ganglionic nerve, or a part of the combined nervous column itself; and an act of inspiration, followed by one of deglutition, and then a general struggle; produced by pinching the pneumogastric nerve in a living donkey, whilst the sympathetic was pinched without any effect whatever. Lastly, Professor Müller * says, that no movements were produced in the anterior parts of a frog when the posterior roots of the spinal nerves were irritated.

6. The *natural* phenomena, such as sneezing from irritation in the nostril, vomiting from tickling of the fauces, &c. as they must have been observed from the earliest times, need no historical mention.

7. The same may be said of the *morbid* phenomena, such as the convulsions of epilepsy, chorea, &c.; and, for the sake of convenience, the generalization of these facts will be deferred till we come to consider the explanations given of them.

8. The phenomena observed in acephalous fœtuses, such as their sucking, swallowing food, crying, and drawing up of the legs when the feet were tickled, and other examples of motion, as in the cases mentioned by Sir G. Blane, Sir Charles Bell, and MM. Lawrence, Lallemand, and Ollivier, and quoted by Dr. Hall †, cannot well be placed in either of the three classes enumerated, and are therefore mentioned by themselves.

II.

9. I now turn to the explanations given of these phenomena, which, in the earlier periods, were, as might be expected, rather

* Impetum Faciens, Leyden, 1745. No. 331.
† Essay on the Vital and Involuntary Motions of Animals. Republished 1768.
‡ Op. cit., p. 197.
§ Select Dissertations.
|| Exper. sur le Principe de la Vie, 1812.
¶ Anat. Comment. No. II., p. 17.
** Memoirs on the Nervous System, 1837.

* Vide Memoirs of the Nervous System, p. 68.
† Memoirs on the Nervous System, pp. 16 et sequent.

after their brains were removed; and one, after decapitation, exhibited "pluckings up of the hinder legs, raisings of the whole faulty; for the experimental phenomena, or such of them as were then known, were referred to sensation and volition, whilst the others were supposed to be explained by the assertion, that they were caused by sympathy.

10. Even in more modern times the same opinions prevailed; for Boyle attributes the motions in vipers &c., before mentioned, to life and volition.

11. Glisson, however, in 1677, as before stated, distinguishes the motions of decapitated animals from those produced by volition, and accompanied by sensation. He says, they depend on a natural perception*, which is not sensation, but which, when it acts on the mind, produces a *perceptio sensitiva*, or sensation, which he thus calls *perceptio perceptionis;* the former causes motion, the latter rules it when voluntary. The sympathetic, or, as I have termed them, the natural actions, he says, are produced by impressions being reflected from one motor nerve to another by consent, just as vibrations in one string of a lyre are capable of producing them in others. I pass over the opinions of Stahl (1692), who attributed the vital and sympathetic actions to a rational soul †, and the other writers of about this period (such as Woodward and Simson ‡), who drew, from the experimental phenomena, arguments to prove that the soul does not reside in the brain; because they are important neither as explanations nor as showing that the facts themselves had been generalized.

12. Up to the time of Whytt, therefore, no clear ideas had been entertained on this subject, and, in mentioning the views of this author, I really think that Dr. Hall has not allowed sufficient weight to his opinions, nor considered enough the time in which he wrote, and the ideas prevalent at that time. To place the word soul § in *italics*, and make a note of admiration, put after it, the only reply, gives rise, in the minds of those who have not carefully read Whytt's works, to an erroneous idea, and seems an answer unworthy of being returned to the opinions entertained in a work containing much that is very valuable, and writen by an author to whom Haller did not think it beneath him to reply at far greater length.

13. Much credit appears due to Whytt for the generalization of the phenomena which he made. Thus he attributes to the same kind of action the motions of the iris *, from the stimulus of light on the retina; of the muscles of the internal ear † from the impulse of sound; of inspiration ‡, from an uneasy sensation produced by blood accumulating in the collapsed lung; of the acceleratores urinæ §, from the stimulus of the semen; sneezing, cough, and hiccup ‖ from irritation of the nose, larynx, and lower part of the œsophagus; the involuntary drawing up of the leg ¶, when a drop of boiling water falls on the foot; cough *, from increased secretion in bronchitis, or obstruction in peripneumonia; closure of the eyelids †, when a blow is aimed at the eye; involuntary contractions of the diaphragm and abdominal muscles ‡, in tenesmus and strangury; and contractions of the panniculus carnosus in brutes §, when their skin is tickled, or stung by insects; these things are all mentioned by him, and considered evidently as forming, with others, a class of motions induced in response to a stimulus; though it must be remembered that he considered them as of the same nature as the movements of the heart, stomach, intestines, &c. ‖

14. In explaining these things, Whytt seems to have been somewhat puzzled by the mechanical philosophy on the one hand, the *animus et anima* of Epicurus on the other, and the singular soul of Stahl between the two; but believes the mind and soul to be one and the same principle acting in different ways through the brain and nerves, and being "the cause of all the vital and involuntary actions, as well as of the animal and voluntary." It seems to act necessarily, and as a sentient principle, only when its power is exerted in causing the former; but in

* De Ventriculo et Intestinis, p. 161, et seq.

† Epist. de Motu Tonico Vitali (1692).

‡ An Inquiry into how far the Vital and Animal Functions of the more perfect Animals can be accounted for independent of the Brain. Edin. 1752.

§ "And what are the conclusions of this writer? 'If the motions of the muscles in a cock's limbs, after decollation, are, without doubt, owing to its *soul* (!), may we not ascribe to the *same* principle the *like*, but less remarkable, motions in men and quadrupeds, after their heads are struck off; and consequently the tremulous motions and palpitations of their *hearts* (!) too, after death, or separation from their bodies?'—'Nor is it (the soul) influenced by any rational motives, but merely by the stimulating causes affecting the different organs—*i. e.* it acts as a SENTIENT, and not as a RATIONAL principle (!).'—'The various sympathetic motions of animals, produced by irritation, are owing to particular *sensations* excited in certain organs, and thence communicated to the *brain*, or spinal marrow.' Quoted by Dr. Hall, vide Memoirs, &c. p. 53.

* Works of Robert Whytt, &c. Edin. 1767. p. 59.

† Ib. p. 79.

‡ Ib. p. 96.

§ Ib. p. 58.

‖ Ib. p. 58.

§ Ib. p. 198.

¶ Ib. p. 132.

* Ib. p. 95.

† Ib. p. 196.

‡ Ib. p. 107.

‖ See also p. 11, for some remarks on the difference of stimuli.

producing the latter it acts freely, and both as a sentient and rational agent *.

15. The motions occurring after decapitation or separation of parts are referred to the sentient principle also, and not the rational; a statement which calls from Dr. Hall another note of admiration †, which appears to be as little needed as the former, for, with writers at that time, sentient did not always mean conscious, any more than with us saying that muscular fibre was sensible to a stimulus, or that a fine balance was exquisitely sensible, would imply that either the one or the other was conscious of impressions made on it. Indeed, Whytt himself speaks of motions in separated parts, "as owing to some kind of feeling, or simple sensation," "though not attended with any *reflex consciousness, a power the soul only exercises in the brain* ‡;" which shows that he had distinguished between the two, and though in other places he appears to consider them as co-existent, still the facts he has stated, and particularly his generalization of them, deserved, in a history of the "Opinions of former Physiologists," a longer notice than the quotation of three passages much at variance with the opinions of the author who quotes him, and their illustration by a few *italics*, capitals, and notes of admiration.

16. We have thus obtained a generalization of the natural phenomena (though others are included with them which more properly belong to another class), the attribution of them, together with the experimental, to one and the same principle, though that principle appears erroneous, and the distinction of some from consciousness and volition §.

17. This distinction is made more clear in the writings of Unzer, who appears certainly to have recognized the principle, that various motions were produced, independently of the mind or brain, in living animals. Thus he says ‖, "the external sensory impression upon the nerves, although it does not reach the brain and is not felt, may produce the same animal motions as if it had been felt." Again: "the internal sensory impression on the nerves may, although not excited by ideas, nay, even though not in the brain itself, produce the same animal motions in the body as the agency of the mind ¶;" and "external sensory impressions may, on their way to the brain, but before they have reached it, produce nervous actions (*nervenwirkungen*), which in so far are not excited by the operation of these external impressions on the mind itself *."

18. I now turn to Prochaska, whose writings, though most singularly overlooked, contain more matter relative to the subject under consideration than those of any other author, except Dr. Hall. The views of the former physiologist are developed in a treatise on the Functions of the Nervous System, published in 1784, but reprinted in the *Opera Minora*, published in 1800, fromwhich, for the sake of convenience, I have quoted. In translating passages, I have endeavoured to be as literal as possible, in order to ensure accuracy; and in illustrating them, I have put into notes some extracts from Dr. Hall's memoir, and also his lectures in the Lancet, in order to show how just were the views of Prochaska, and how nearly allied to those at present entertained.

19. The *division* of the nervous system given by this physiologist, appears different from that usually made. He says †, "I think it may be divided into three parts, as its functions may also be conveniently arranged: 1st, into animal organs, or thinking faculties, which constitute the cerebrum and cerebellum; 2d, the common sensorium, which would appear to be the medulla spinalis and medulla oblongata; not excepting such parts of the cerebrum and cerebellum as may afford immediate origin to the nerves; 3d, the nerves properly so called," &c.

20. As to the *power* by which this system acts, he says ‡, "To the almost inorganic structure of the medullary pulp of the nervous system, if we trust to the senses alone, much is wanting to enable us to understand its wonderful functions; but there seems, beyond doubt, to be joined with it another and an invisible element; and the union of the two constitutes the productive cause of all the functions of the nervous system. This (element) since it is so obscure and altogether unknown, just as is the attractive power of bodies, appears to me to have been rightly named the *vis nervosa*, &c."

21. With regard to the *action* of this power, he remarks §, "The vis nervosa, that it may act, requires a necessary stimulus;" and compares it to a spark latent in flint or steel, for the production of which the conjunction of both is required. He also divides‖ the stimulus into the "mechanical or corporeal, and the stimulus of the mind;" and mentions the variety of effects produced by different stimuli ¶.

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* Works of Robert Whytt, &c. p. 153.
† Vide passage before quoted.
‡ Ibid. note to p. 206.
§ Vide § 5.
‖ Erste Gründe einer Physiologie der eigentlich thierischen Natur. Leipsic, 1771, p. 349.
¶ Ib. p. 351.

* Ib. p. 355.
‡ Ib. p. 65.
‖ Ib. p. 66.
† Op. Min. Part II. p. 51.
§ Ib. p. 66.
¶ Ib. p. 68.

22. The vis nervosa, he states, is increased *, or diminished †, by various circumstances, and devotes two sections to the consideration of the conditions under which this increase or diminution takes place; noting especially the influence of opium, which, indeed, had also been remarked by Whytt ‡.

23. The vis nervosa is next affirmed to be divisible, and to subsist in the nerves without the cerebrum §; and after shewing this by cutting the spinal cord in the back of a frog, and producing convulsions in the lower limbs by irritation of the inferior portion of the cord ‖, he proceeds:—"Moreover, the vis nervosa not only remains in the spinal cord and nerves when separated from the brain, but also exists in the nerves which never had any connexion with the brain. Acephalous fœtuses clearly shew this, which, without the brain, by the power only of the nerves and spinal cord (if this be also not deficient,) live for the whole time in the uterus of the mother, are nourished, increase, and when brought to light often afford no obscure signs of life. Animals in which there are nerves but no brain, also shew the same thing ¶."

24. In proceeding to treat of the *functions* of the nervous system, he divides ** them into animal actions—actions of the sensorium, and of the nerves; and beginning with the last, treats first of their action in producing sensation and motion, and after mentioning that external impressions on the extremity of a nerve are propagated to its origin, or internal on its origin propagated to its extremity, he says††, "but if an impression be made in the middle of its length, it is propagated in the same short time both to the origin and the end." This aptitude for receiving and transmitting impressions he calls the vis nervosa of the nerves, and soon after proceeds to treat of the functions of the common sensorium‡‡; his remarks upon which are so important, that I must give them to you nearly in their entire state.

25. "What is the common sensorium—what its office, and where its seat?" is the question proposed at the commencement of the chapter devoted to its solution, which chapter is nearly in the following words:—

26. "External impressions which may be made on sensory nerves (nervos sensorios) are very quickly propagated through their whole length, even to their origin, where, when they have arrived, they are reflected (reflectuntur) by a certain law, and pass into definite and respondent motor nerves (nervos motorios) by which again, being propagated rapidly even to the muscles, they excite definite and determinate motion." Here we have the *principle*.

27. "The *place* in which, as in a centre, the nerves allotted both to sense and motion, meet and communicate, and in which the impressions of the sensory are reflected into the motor nerves, is called by a term, now received by many physiologists, the common sensorium."

28. After giving a full account of the opinions of his predecessors as to the seat of this common sensorium, he states his own. "The whole cerebrum and cerebellum certainly do not appear to constitute the sensorium commune, which parts of the nervous system would rather seem to be the instruments which the mind uses to perform its actions called *animal*; but the sensorium commune, properly so called, appears not improbably to extend through the medulla oblongata, the crura of the cerebrum and cerebellum, a part also of the optic thalami and the whole spinal cord; in a word, the parts included within the origins of the nerves (*quam late patet nervorum origo.*) That the sensorium commune extends even to the spinal cord, motions remaining in decapitated animals teach us, which cannot be produced without communication and consent of the nerves arising from the spinal cord, the seat of which consent must be in the spinal cord, the remaining portion of the sensorium commune." Here, then, we have its *localization*.

29. "The reflexion of sensory impressions into motor, which is done in the common sensorium, is not performed according to physical laws merely; where the angle of incidence is equal to the angle of reflexion, and where, as is the action, so is the reaction; but this reflexion follows peculiar laws, written, as it were, by nature in the medullary substance of the sensorium, and which laws we are able to know by their effects alone, but can by no means understand by our own ingenuity.

30. "Yet the general law by which the common sensorium reflects sensory impressions into motor, is self-preservation (*nostri conservatio,*)* so that certain motor

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* Ib. p. 68.
† Ib. p. 77.
‡ Vide sec. 5.
§ P. 82.
‖ P. 84.
¶ Ib. p. 85.
** P. 101.
†† P. 102.
‡‡ P. 150 to 158.

* "It is on this part of the nervous system that the preservation of the individual and the continuation of the species depend."—Dr. Hall, *Lancet*, p. 687.

"In the preceding brief account I have endeavoured to give a just idea of the state of the science when I began my investigations; it consisted of mere experiments, mere facts, untraced to any just principle of action, *unapplied to the phenomena of life.*"—*Ibid*, p. 653.

impressions follow external hurtful impressions upon our body; the motions to be produced aiming at this, that the noxious thing shall be repelled and driven away: and on the other hand, internal or motor impressions follow external or sensory impressions pleasing to us (*profuturas*,) the motions produced tending to this, that the grateful state (*status gratus*) shall be longer preserved.

31. "Many examples which might be adduced prove certainly this general law of the reflexions of the sensorium commune; of these it will suffice to mention only a few. Irritation applied to the internal membrane of the nostrils excites sneezing, because that impression made by the irritation upon the olfactory nerves is carried through them to the common sensorium; there by a certain law it is reflected into the motor nerves, affecting the muscles allotted to respiration, and by these produces a strong expiration through the nostril, by which, from the air passing out with force, the irritating body is displaced and ejected. So it happens when irritation is caused in the trachea by a crumb of food, or a little drop of drink falling in: this irritation conveyed to the common sensorium, and there reflected into the nerves allotted to the motions, acts so that a strong cough is exeited; the most fit remedy for expelling the irritating body, which (cough) does not cease till the irritant shall have been expelled. If a friend approach his finger to our eye, although we are persuaded that no harm will be done, yet now, that impression is conveyed through the optic nerve to the common sensorium*, and is so reflected into the nerves of the eyelids allotted to motion, that the eyelids are shut involuntarily to avoid the injury. These, and innumerable examples which might be adduced, shew plainly in what mode the reflexion of sensory impressions into motor made through the common sensorium, have regard to the conservation of our body." Here we have the generalization and final cause of these natural phenomena.

32. "Whilst, therefore, the chief function of the common sensorium consists in the reflexion of sensory impressions into motor, it is to be noted that this reflexion may be made either without or with the knowledge of the mind (*anima inscia vel vero anima conscia.*) Thus the motions of the heart, stomach, and intestines, by no means depend upon the consciousness of the mind; and since no muscular motion† can be excited unless a stimulus applied to the sensory nerves pass into the motor nerves by this reflexion, then it is certain that the reflexion of impressions fit for the excitation of these motions may be made without the knowledge of the mind if they be made in the common sensorium. But it is asked, whether these reflexions must reach even to the common sensorium to be reflected, or whether without this they are reflected more quickly in the ganglia, whence the parts derive most of their nerves? Concerning this more will be said hereafter.

33. "Still, that reflexions of sensory impressions into motor may be made in the common sensorium, the mind being altogether unconscious, certain actions remaining in apoplectic patients in whom all consciousness is gone, teach us; for they have both a strong pulse and breathe strongly, even lift up the hand, and very frequently apply it unconsciously to the affected part.

"34. The common sensorium also acts without the consciousness of the mind, in producing the convulsive motions of epilepties, and also those retractions of the limbs when pricked or tightly pinched, which (besides the action of the heart and the breathing) are sometimes observed in those buried in profound sleep." The independence of these actions, of sensation, and volition, is here abundantly insisted on. He proceeds,

35. "To this, also, have reference all those motions which remain for some time in the body of a decapitated man or other animal, and are excited on pinching the body, but especially the spinal cord, which are produced certainly without knowledge of the mind, and are *regulated* by the remaining part of the common sensorium, which is in the medulla spinalis.

36. "All these actions result from the organization and physical laws proper to the common sensorium, and are, moreover, spontaneous and *automatic*.

37. "Those actions which are produced in the animal body *with the knowledge of the mind*, are either such as the mind, of its own will, has no power over, or such as the mind is able to produce or impede at its pleasure. These, as far as they may not depend on the mind, are ruled by the common sensorium; yet those which may be produced without the knowledge of the mind are *automatic* actions; such are sneezing from a stimulus applied to the nostrils—cough from a stimulus falling into the trachea—vomiting from tickling

* Vide Exp. by Mr. Mayo. § 5.

† "These remarks lead me to observe, that all the acts of this system are the result of excitation, by stimuli applied to the nerves which proceed to the spinal marrow, whence other nerves take their origin, and pursue a reflex course to the parts to be moved. The system of incident nerves, of the true spinal cord, and of reflex nerves, is like the agency carried on through it, *new to physiology*"—Dr. Hall, in *Lancet*, p. 654.

of the fauces, or an emetic—tremors and convulsions in chorea, and in the paroxysms of intermittent fever, &c. &c. But the actions which the mind directs and moderates by its own will (though even in producing these the common sensorium has its part), we call animal, not automatic; and these will be treated of in the next chapter;" on turning to which I find the following passages:—

38. * "It has been already shewn, that most of the automatic motions in man are made by this vis nervosa of the sensorium commune; yet nevertheless, though many animals live, and are regulated by this power of the common sensorium only (viz. those which are altogether destitute of brain and of the higher functions of animals), and so may be termed true automata† in man and many animals allied to him, an addition is made to the nervous system in the cerebrum, and besides this, a certain principle which we call mind."

39. The animal actions, he says, are *perception*, judgment, and *volition*, to which may be added imagination and memory‡.

40. After mentioning at length the double movements of the animal body, voluntary and automatic, and pointing out the distinction between involuntary and voluntary muscles, he proceeds, § "Yet it sometimes happens that all these muscles (the voluntary) reject the government of the mind, and either without its knowledge, or else against its will, are from some mechanical stimulus agitated more strongly than is natural, which may be observed in the convulsions of hysterical persons, epileptics, infants, those labouring under chorea, and in the motions which, though performed by muscles named voluntary, cannot but be called automatic.

41. "In the fœtus‖ in utero matris, and in the new-born, these muscles are not yet moved voluntarily, but altogether automatically, since the cerebrum at this age is not yet fit for thinking, until the organs of the faculty of thought being successively evolved, the mind learns both to think, and use the muscles subject to its power."

42. Thus I conclude my extracts from Prochaska, in making which, I know not whether I have been more surprised at the accuracy of his views, or the fact of their having been so entirely overloooked. Of these two circumstances I cannot convince you more clearly than by making a short recapitulation of his views, and, indeed, of those of others here mentioned, and comparing it with that given by Dr. Hall in his Memoirs on the Nervous System; which comparison will also shew the discoveries and views of the latter physiologist, and serve to exhibit at the same time a proof of how two minds may, by pursuing the same subject, arrive at similar conclusions, independently (?) of each other.

Recapitulation of Facts, &c. in this paper.

43. We have seen from the facts and opinions stated in the previous pages the following things:—

44. I. That a principle of motion has been laid down, *distinct* from *sensation*, volition, and *all functions of the cerebrum, or the mind.* §§ 32, 33, 34, 35, 36.

45. II. That this principle is distinguished as being a function of *certain parts* of the nervous system, viz. the spinal marrow and nerves (with the parts from which the latter take their rise, exclusively of the cerebrum and cerebellum.) § 28.

46. III. That it is the *same* as that called into action by stimulation of the spinal marrow or nerves in their direct course; in other words, *the vis nervosa of Haller.* §§ 20, 23.

47. IV. That it acts by *impressions* made on the extremity of a nerve *passing to its origin*, being there *reflected*, and *passing back* to the muscles, exciting motions. § 26.

48. V. That it has an extensive *physiological* application, and has been applied to the explanation of those phenomena termed *sympathetic*, such as sneezing, coughing, vomiting, &c. &c., in all which *the spinal cord* (with the parts above mentioned) has been considered as the *regulator or disposer* of the different movements. §§ 31, 36.

49. VI. That it has been applied to *pathology*, in the explanation *of the class of convulsive diseases.* §§ 37, 40.

50. VII. That it has been shewn to be capable of *increase* or *diminution*; the latter by *therapeutical* agents. § 22.

51. VIII. That it has been applied to the *classification* or generalization of *experimental, natural,* and *morbid phenomena*, as mentioned in this paper. § 26 to 38.

52. IX. That it is the principle of life in the fœtal state.

53. X. That the *principle* itself, the *seat* of its action, and the *theory*, having been developed, attention should now be turned to the observation of more facts and phenomena.

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* Op. Min. Part II. p. 170.

† At p. 657 of the Lancet, Dr. Hall speaks of the lowest forms of invertebrata, "in which sensation and volition are nearly extinct, and the animal lives a sort of excito-motory life only."

‡ Op. Min. p. 171.

§ Ibid. p. 188, *et sequent.*

‖ Ibid.

Recapitulation of Dr. M. Hall.*

54. "To avoid all misapprehension relative to the claims of this paper, and to the views which it sets forth, I will now recapitulate briefly, but distinctly, the principal objects and results of my inquiries:—

55. My first object is the *distinction* of the excito-motory property from sensation, volition, instinct, and *all the functions of the cerebrum*, or of the ψυχη, or *mind*.

56. My second object is the distinction of this property from the vis insita, or irritability of the muscular fibre.

57. My third object is the distinction of this property as a function of—

1. A true *spinal marrow*; and
2. A system of excitor and motor, and excito-motory nerves, *exclusively of the cerebrum*.

58. My fourth object is the identification of the excito-motory property with the motor power exercised by the spinal marrow and muscular nerves, when stimulated, upon the muscles to which they are distributed in their direct course—*the vis nervosa of Haller*.

59. My fifth object has been to prove, that the excito-motory property acts in an *incident*, *retrograde*, and *reflex course*, along incident nerves, when the first of these are stimulated mechanically, or by galvanism, in experiments, or by the natural stimuli in the living animal.

60. This investigation has led to the suggestion of a corresponding point of anatomy, consisting—

1. In some portions of the spinal marrow;
2. In incident excitor nerves; and
3. In reflex motor nerves,

distinct in some instances, even in the mammalia, from the sentient and voluntary nerves, as in the pneumogastric or internal excito-motory nerve, and probably distinct altogether in the invertebrata.

61. My sixth object was the prosecution of the *physiology*, as seen in the action of the orifices, the sphincters, the ingestors, the expulsors, of the animal economy in general; in the theory of the renewed acts of respiration, excited by the contact of carbonic acid with the pneumogastric nerve; in the view of the *spinal marrow*, as the *combiner* in all the complicated acts of ingestion, egestion, &c.—as the source of the tone of the muscular system—as the seat of the passions, emotions, &c. &c.

62. My seventh object was to ascertain the relation of the excito-motory property to *pathology*, to the class of diseases originating in the excitor nerves, the spinal marrow itself, and the motor nerves; in a word, to the *entire class of convulsive diseases*.

63. My eighth object was the inquiry into the *therapeutics* of the excito-motory property, as evinced in the action of strychnine, of cantharides, of hydrocyanic acid, &c., and into the mode of action of certain causes of diseases, as dentition, irritation, wounds of nerves, &c.

64. I may also add, that the excito-motory principle affords a systematic *classification* of an extensive series of *experimental facts*, and of *physiological*, *pathological*, and therapeutic phenomena, previously entirely unarranged.

65. Finally, it is the establishment of the *principle* and of the special organic *seat*, and the development of the *theory* or system, of the excito-motory phenomena, and by no means all the facts or phenomena themselves, which I claim as the peculiar result of my own labours."

[To be continued.]

* Memoirs on the Nervous System, p. 112.

OBSERVATIONS

UPON

VENTILATION AND WARMTH;

COMPRISING STRICTURES ON DR. ARNOTT'S WORK UPON THESE SUBJECTS.

By Julius Jeffreys, Esq.

Under my first head of ventilation in general, I have advocated the necessity of maintaining within our dwellings a tenfold larger supply of air than the author would allot to us. With so good a case in my hand, I trust I cannot possibly have failed before my reader, though neither my space, my time, nor my talents, may have enabled me to do it justice.

The Englishman has made his home his castle. Over all its passages he holds command; but there is a right as ancient as his own, which he has hitherto never ventured to question. He has felt bound to acknowledge the freedom of air, and every where to maintain an open chimney for its passage. Of this freedom he has made a proverb. He has carried the metaphor into his politics, as expressive of the state he would himself desire to attain to. As in his politics he would dread the un-

dermining of his excellent constitution, by allowing his ancient rights to be disturbed, so at home, in his eeonomies, let him not create a fearful precedent. Let him not deny to air its long-preseribed and very ancient right of way by the ehimney; for if he does, not more surely would his political eonstitution be undermined in the former case, than will his bodily eonstitution in the latter.

The arrangement adopted in my former paper requires that I should consider now the second division of my commentary, namely, the art of ventilating and warming by the aid of recovered animal heat—a subjeet at once novel and important, and therefore very interesting. Having read with attention the several articles in our author's work which may with propriety be arranged under this head, I find myself arrested at the very threshold of my subjeet, by the painful consideration that there is hardly one passage of our author's with which reason or faet will allow me to agree. In my former paper, greatly as I had to differ from him in the main, I had the pleasure of bearing my humble testimony to the truth and importance of some at least of the author's observations. Under the present head I scareely know where sueh a one is to be found. Having passage after passage to maintain, without relief, a front opposed to the author, eonseious of what my reader may have been associating with his name, a duty here unfolds itself, little thought of at the first, and which I gladly would avoid.

I would hope the man is not often to be found, whose desire would be to stop the eareer of a statesman or a philosopher, who, while high in reputation, was equally high in merit, and who was wielding his reputation for the benefit of mankind; and I could not applaud that man for his invidious veracity, whose disclosure was the faet, however true, that his neighbour's reputation was rather wider than its basis, provided that that reputation, as in the former case, were exercising benefieially its influence over men. But if, about to overspread a very good foundation, the gift of the public, a favoured reputation, should be unwittingly turned against the donors, and be propagating more of error than of truth, mistaken would be that man's generosity, or unworthy his timidity, who should hesitate in the endeavour to moderate its influenee, although, in doing this, its basis should be unavoidably surveyed. And eould he be refused a personal motive also, if the party in question, resting upon principles in morals and in seience, but failing in both, had taken upon him to prejudice the mind of the public against himself and his labours, and with an unbecoming air of superiority had given force to his obloquy by interspersing with it doubtful and unaeceptable praise? Might he not be permitted to examine an object which had placed itself in his pathway, that he might ascertain how far its depth corresponded with the lofty front it was presenting, and might give to the foree requisite for its removal a proper direction?

High as are the author's professional talents, I suppose them to be equalled by those of several other able physieians in the metropolis. Among them he is distinguished chiefly as the author of the Elements of Physics, the inventor of the hydrostatic bed, and recently as that of the thermometer stove, and the author of the work before us.

I confess myself never to have possessed, nor, till very recently, to have looked into, the Elements of Physies. Whatever acquaintanee I may have with these subjeets, has been obtained from sources of an earlier date and of a different eharaeter, and from my own experiments. The work is manifestly one for which the publie are under many obligations to the author, who has, through the employment of a very elear and simple style, spread among them knowledge on several branches of physical science, more suecessfully, perhaps, than most other elementary writers on these subjeets. It bears evidence of a more general acquaintance with certain branches of natural philosophy, than ean be supposed eommon in the profession; but whether that knowledge were profound or not, eould not, I presume, be judged of from the work itself. It is not in a eompilation, at once popular and elementary, that any great depth is to be looked for; whether possessed or not, must be judged of elsewhere, when an author is upon his own resources.

With respect to the floating-bed, I may remark that that man had a bold

conception who first thought of laying us upon a fluid; and a spirited execution, too, who selected the most difficult to confine—air—for his fluid. But the mere substitution of water for air, of one fluid for another, why the very sensation of the first fluid under the body might have suggested it to any ordinary mind. It was, as an act of originality or ingenuity, one of the smallest of inventions I have ever heard of; especially as the whole merit of finding a substance that would confine the liquid permanently, was due to the inventors of India-rubber cloth. Till this cloth was forthcoming, water could not advantageously have been employed; but after its preparation, water must have flowed, as it were, spontaneously into these beds, to displace the rarer fluid—air; and I would therefore ask any unbiassed mind, wherein did the ingenuity consist?

With regard to the thermometer stove, and the other contrivances described in the work before us, and to the work itself, I will ask of my reader to suspend his judgment until this commentary shall be completed, provided its commencement has appeared to him in any measure satisfactory.

As a longer period, however, will elapse before I arrive at the third head of my subject than I anticipated, and as I have been asked to express a distinct opinion upon the advantages of the thermometer stove, when compared with others, I make a few brief remarks now, though somewhat deranging thereby the order of my subject.

Viewing the thermometer stove as consisting of two distinct parts — the stove itself, and the thermometric regulator—with regard to the advantages of the former I have to observe, that, if my reader has made up his mind to close up his chimney, he will, I believe, find the stove itself to be superior to any other *close-air stove* at present in ordinary use for domestic purposes; and that the superiority will prove mainly to consist in the surrounding of the fire with brick, and the command over the draught by close fittings, and by a regulator on the ash-pit door. These points have for centuries been attended to minutely, by careful chemists and artists, and a perfect command has thereby been obtained over the heat of the fire, and over the consumption of the fuel. These provisions have also been imitated in many domestic stoves; but in so rude and inefficient a manner, that, in practice, little command has been obtained over the draught, and little, therefore, over the consumption of fuel and heat, although the subject has not been neglected by former writers. For having drawn their attention to these important points again, the public are indebted to Dr. Arnott, and also for applying to a common domestic stove a construction long extensively employed in the arts, and, I think, in more than one kind of domestic stove in use on the Continent—namely, making the stove of greater lateral dimensions than the fire itself; by which construction the sides of the stove are, of course, further removed from the fire than in most common stoves, and therefore are less liable to become overheated. The circulation of hot air between the fire and the sides of the stove, upon which the author dwells much, does in reality take place in a greater or less degree in all stoves, especially where bricks surround the fire; but the directing of the attention of the public to the utility of the construction, and the giving to it the same effect in domestic stoves in England which it has in some used abroad, and in stoves and furnaces employed in the arts, is due to Dr. Arnott, and, so long as a common *cubical* stove is adhered to, it will afford valuable assistance to the other and principal means for preventing the overheating of the sides of the stove—namely, surrounding the fire with thick bricks, and commanding the draught. But I would caution the reader against trusting to it, to the neglect of the latter, for if he does, it will assuredly fail him. Of this I felt certain, when first I saw the interior of one of these stoves; and the other day, in a case where the stove was rightly enough made in this respect, with an abundant space round the fire, I heard a person boasting, among the other virtues of the stove, that it had *baked a pudding* set upon *the outside of it*, with a cover over it! It is proper here to caution the reader against thinking that the partition (g, h) pourtrayed in the author's sketch of the stove, in p. 43 of the work, is at all necessary. Such partitions are used in some furnaces in the arts, and to good purpose; but in the present case, the partition is not only useless, but decidedly injurious, as will be explained hereafter. The commanding of the

draught is therefore the main point. And many of the larger kinds of stoves commonly in use may be given all, or nearly all, the efficiency of the author's stove, by putting bricks round the fire, and making the doors fit well, with a sliding or stopper regulator, such as chemists use, on the door of the ash-pit. I think it proper to mention this, as it may save persons who already have stoves of this kind from incurring the expense of one of the new kind, which would be quite unnecessary.

With regard to the other part of the author's stove, the ingenious thermometric apparatus in which the expansion of air is employed to regulate the draught by giving motion to mercury in tubes variously shaped, having had no experience in a self-acting regulator of that kind, I would desire to speak with great caution. But having some little knowledge of the action of air in thermometric and barometric instruments, I have not been able to satisfy myself that this adaptation of the author's would come into common use, however well it might answer in attentive and judicious hands. For domestic purposes, any apparatus, to be generally useful, must require the least possible exercise of thought and of attention to adjustment, on the part of domestics, and of the generality of masters. Persons, often talented and learned, whose taste does not lie at all in a mechanical direction, and who are busied about other things, appear, in regard to things mechanical, like idiots to those who have ability in these things. Now, setting aside the peril of so brittle and fragile a substance as glass in so dangerous a neighbourhood, derangements, requiring attention, will occur from moving the stove, from overheating, from the evaporation of the mercury, and other causes. Hence, some of the makers of the stove have already adopted regulators of other kinds, some self-acting, and others common hand regulators. The last of these I conceive to be preferable in a stove of this kind, because, after all, the self-acting regulators will require a good deal of manual attention, when the current of air outside of the stove shall vary as much, as it will, should any proper attention be paid to ventilating the apartment. When deprived of the thermometric regulator, the stove loses all claim to originality as an invention, though it is a useful modification of the stoves long employed by others. It will, however, afford me sincere pleasure to find that the thermometric regulator shall hold its ground, and prove more useful in practice than I have supposed. It is my duty to remind the reader that the remarks I have ventured to make upon it are those of conjecture only, and not of experience. One or two of the forms the author has given it manifest considerable ingenuity. Far be it from me to speak less favourably of the author's inventions than they strictly merit. I will, on no account, imitate the example he has set me.

Were our bodies constituted like the lungs of persons in sound health, able to endure the free application of cold air to them at all times, ventilation would be attended with no difficulty. Windows thrown open would ensure all that was to be desired. But the surface of the body of all, and the lungs of many persons, cannot endure when at rest the application to them, in its cold state, of a small portion, even, of that quantity of fresh air, which, but for the cold, would prove highly salubrious to them. The difficulty of ventilating is, therefore, mainly dependent upon that of warming. Any means of warming air becomes, therefore, a more or less perfect means of ventilation. And if these means employ as their material, waste animal heat separated from accompanying impurities, such means can ventilate and warm, by the aid of recovered animal heat.

In article 83 of the author's work, under the head of "Double-current Warm Ventilation," he has proposed a method of warming rooms upon this principle, into which I shall examine hereafter. In the next article, as falling within the range of the subject of his treatise, he speaks of the *respirator*, under the title of an air-warming mouth-piece, which, under the form of that very instrument alone, which is known by the public as the respirator, I believe to be the only effectual means of ventilating and warming by the aid of recovered animal heat.

My reader, I presume, must have already heard of the respirator; but if not an invalid himself, nor a member of the profession, he may be unaware of the importance this instrument is acquiring, on account of the relief it is

found to afford, of the extent to which it is already in use, and of the still greater extent to which it will eventually be employed. Of the persons wearing it, there are very few who do not experience some benefit from it, while the great majority express relief such as could hardly have been anticipated. The number of its wearers, too, amounts to many thousands; but they form as yet only a small portion, of course, of the multitudes who stand in need of such an instrument. Among the persons who express in various and strong terms, the comfort they derive from the use of this instrument (curative it does not, like the author's stove, presume to be*,) are to be found cases of pulmonary consumption, in all its forms and stages, asthmatic cases of every variety, persons liable to pleuritic attacks from sudden exposure to cold; others to that kind of irritation of the wind-pipe which is felt upon the return to a warm room from the cold air; others to inflammation of the upper portion of the wind-pipe or larynx, with swelling of the tonsils; and others, even, whose only uneasiness has been from coldness of the extremities, and consequent indigestion. Of the sincerity with which these communications have been made, the best proof is, that they have nearly all of them been unsought for; the feelings of the parties having dictated them. The degree of relief is expressed often in language which the inventor could not in delicacy quote, and which he is quite desirous of admitting his humble exertions do little merit.

With respectful feelings towards a profession, of which I have the honour to be an humble member, I determined to introduce to the public, under its auspices if possible, this instrument, upon which I had devoted much of time and money. And I have much pleasure in acknowledging my obligations to the many distinguished members of the profession who afforded my invention their ready countenance and support. But I was not ignorant of the fact, that when known by the public, the respirator would make its way, retarded, perhaps, but still certain, should the profession, which was not possible, have proved hostile to it. The profession cannot yet, alas! exert over the public sufficient influence for restraining even the grossest empiricism; much less, then, could it suppress an instrument, regarding the action of which, all that could be said with any truth must be in its favour. That any general resistance on the part of the profession would be felt I have not allowed myself for an instant to suppose. I have refused to listen to suspicions that any of its members could be found capable of discountenancing the respirator lest it should affect in a trifling degree applications to them for medicine or advice. In one case only, and that in a low grade of the profession, has such a motive been clearly avowed to me; but even then, upon reflection, this person could not withhold the expression of his satisfaction at seeing a patient with a respirator, released from his confinement, and able to attend to his business, although it somewhat lessened his consumption of cough mixtures and narcotics. Invalids have occasionally asked me to assign a reason why their medical advisers should have dissuaded them, for months, from trying the respirator, which was now affording them so much comfort. In each such case I have been careful to express the conviction I truly felt — that prejudice alone had been the cause of it. Of the persons who prefer applying at the manufactory for instruments to obtaining them from the agents, I find, when I have leisure to see them myself, no small portion to be sent by medical gentlemen, who have not suggested only, but pressed upon them the use of the instrument.

If I shall appear to have digressed a little here, I trust that my medical reader will excuse it. I could not let the opportunity pass of assuring him, that if any reflections have been cast upon the motives of such medical men as have been slow to recommend it, I have had no part in them, but have always taken pains to establish the fact, that a want of conviction alone could be the occasion of their tardiness; and I do further assure him, if any thing has been said of the powers of the respirator beyond its legitimate pretensions, they have not emanated from myself.

In reviewing the list of complaints, in which experience has abundantly proved the respirator to be productive of more or less comfort, we find them to embrace no small portion of human maladies in cold climates; in our own

* Vide p. 4 of the Author's Advertisement.

especially. In this kingdom there cannot be less, at any one time, than half a million of persons so far delicate or diseased, as to render the inhaling of cold air productive of distress to them. If we were to include all those who, though not always, are at times liable to feel inconvenience from it, the number would be greater still. Now the value of a thing is to be measured by the amount of its utility in each case, and by the number of cases to which it is applicable conjointly. Estimated by this rule, the instrument which we are called upon to consider under our present head, assumes a peculiar importance. As such, Dr. Arnott could not well have omitted to make some mention of the respirator, in a treatise upon Ventilating and Warming; nor can I, his commentator, neglect to remark upon such a notice as he has afforded it. I could much wish that he had been spared the necessity of having to take notice of the invention of another man, and myself then an exposition which my connexion with the respirator renders it no very pleasant office to perform. The subject is discussed by the author in the following manner, Art. 84, p. 69:—

"This process for transferring heat from impure air about to be dismissed into the atmosphere, to pure air about to be applied to use, will, by many readers, be deemed quite new; and yet, in a less perfect form, it has been a popular practice in Europe from time immemorial. When a person, going out of his house into the cold air of winter, ties a bulky woollen handkerchief, called "fearnothing," around his neck and face, or holds any such porous mass over the mouth that he may breathe through it, he is really applying the same principle. His warm breath going through the handkerchief, or other mass, warms it; and then the cold pure air, drawn towards the mouth through the heated mass, absorbs a great part of the retained heat, and enters the chest of the individual much less cold than the air of the atmosphere around. The hackney-coachmen of Paris are rarely seen, in winter, without such a defence; and in all countries, medical men commonly desire patients with tender chests not to pass even from one room to another, through a cold staircase, without such protection. Lately, a useful modification of this simple process has been presented to public notice by a medical practitioner, returned from India. He substitutes for the bulky handkerchief, or texture of common thread, a less bulky texture, yet more heat absorbing, of metallic thread or wire; in fact, folds of fine wire gauze, or pierced plates of metal, which, when fixed in a light frame, form a compact mouth-piece, fitting closely around the lips, and leaving free passage for the air between the numerous apertures. The folds, or plates, are heated by the warm breath going out, and then give up the heat so acquired to the pure air going in. The contrivance may be called the fearnothing of metal; and it has the advantages of being less bulky, of retaining less of the impure air in it, and of allowing speech to be heard through it, almost as if there was no interruption. The effect of any means of this class is to give in reality an artificially-warmed atmosphere to a person walking out in the open air, as if he were still in his room, heated by a fire. It may be called a portable warm-room, or a suit of warm clothing for the lungs. It affords a valuable security to persons labouring under certain kinds of weakness, or disease of the chest, and who are compelled by their duties to leave home; and it will allow many persons who, without it, would be confined to their rooms all the winter, to walk abroad with impunity. It is an addition, made by art, to the beautiful provision already existing in nature, of the long narrow channels of the nostrils and throat, which have considerable effect by their warmth in tempering the air which passes through them to the lungs. The arrangement of metallic threads, or plates, forming the air-warming mouth-piece, has been called by the proposer a *respirator*, and has been made the subject of a patent. We may regret that, in this country, there should be no other provision for rewarding men who make useful suggestions, but by allowing them, even where the public health is concerned, to levy a tax, and which often falls heavily on persons who can little afford to pay it. In the present instance, a great many of the persons needing any new means of relief will be among the poor, already, perhaps, suffering from want of clothing and fuel; and the price charged for the first of the wire-gauze instruments was so high, that only the rich could afford to buy them: they are now cheaper. We

may remark, with respect to the wire-gauze, that the temperature of the air inspired through it is very unequal, being much higher at the commencement of the inspiration than towards the end, when the metal has already given up nearly all its heat. But a perfect uniformity of temperature is attainable, by using a suitable modification of the double-current apparatus, of which I have spoken above. For this purpose is required a mouth-piece, from which one or more tubes of thin metal, or other substance, may carry the hot breath to the atmosphere, and to which another larger tube, surrounding the first, may bring pure air from the atmosphere, the currents being kept quite distinct by simple valves in the tubes. The warmth of the departing air will thus be given to the coming air, and the degree of warmth retained will depend on the length of the tubes, or on the action of a valve placed over a lateral opening in the outer tube, near the mouth-piece, and made obedient to a thermometer. Thus also will uniformity of temperature be secured. The apparatus may be made to resemble a cane held to the mouth, or a Turk's pipe, or may bend round the neck and be concealed in a cravat. Because twice as much heat issues with the breath as is wanted again, a person will not lose the advantage of the apparatus by detaching his mouth from it occasionally, to take his part in ordinary conversation."

Had an unlettered mechanic fallen, by an accident, upon the construction of a respirator of some sort, he might willingly have received, at the author's hand, any notice, however ungracious, which promised the furthering of his sales; but let it be remembered that the inventor of that instrument is the author's equal, as a member of society, and must have all the same feelings of an educated mind. It will then readily be perceived, any remarks appearing to recommend it, associated as they are in this quotation, must be any thing but acceptable to him; that it would be less unsatisfactory to him if the author's reflections had been all of one piece with those that are most objectionable. As to the author's remarks in favour of the action of "any means of this class," that is, of any instrument having the same *object* as the respirator, they could hardly have been omitted; knowing, as he did, that thousands of persons are speaking to the same effect. Moreover, he had a mouth-piece of his own to propose, and most properly to propose, if it shall appear, in the sequel, to be any improvement, or defensible even upon principle. Lastly, he had made to me, long since, the gratuitous offer of speaking favourably of the respirator in a book he was about to publish. Its fulfilment, therefore, and the book, are now before us.

The reader is already apprized, that the respirator, or a more efficient instrument, if such a one can be discovered, is becoming of great importance to the public; but he may be unaware of the fact that I hold the exclusive right, not to the particular form only which I have given to the materials, but to the use, in any form, of that class of matter, of which alone an efficient respirator can be made, *metal* having never before been employed for this purpose. Indeed no instrument, having the avowed object of the respirator, has ever been previously made public. My primary and exclusive right, then, is to metal, whatever kind and whatever form of it may be employed; so that a Turk's-pipe instrument, as recommended by the author, or any other deviation from the proper form, would become, if of metal, no less an infringement of the patent than would the copying of the respirator itself. Moreover, twelve years yet remain during which this exclusive right will be possessed.

When these several facts are considered, it becomes a matter of no small importance to the public, that the individual who has possessed himself of this right is in all respects a trustworthy person—trustworthy both as to the ability and as to the morality with which he will exercise the power. If there were no exclusive right possessed by the inventor, a person would equally be chargeable with a want of candour and of generosity, or a want of knowledge and of caution, *provided his statements were incorrect*, who should step in between the public and the inventor with reflections against the scientific skill and moral feeling he had evinced in the undertaking. But where the right was absolute and exclusive, such an objector, namely, one whose statements were incorrect, should have to answer to the public for undermining their confidence in a provider, out of whose hands they could not escape; and the more so, if

they were tantalized by his assuming towards such an instrument the air of a well-disposed patron.

If the author's reflections had reference to principles in science merely, however erroneous, I might trifle with the error, as the result of inattention to this subject from a devotion to his own; but when the highest of the moral principles—humanity, and the lowest of the natural passions—a love of gain, are the points upon which I am assailed, as lost to the former in my devotion to the latter, I owe it to myself to put to silence such calumniation; and I owe it to the public too, among whom there may be many an invalid, upon whose mind the author's remarks are producing the impression, that whatever benefit he may be deriving from the respirator is but from an ill-constructed instrument, bearing a fraudulent price, and is an earnest only of the relief the principle would have afforded him, at little cost, if, instead of having been tripped upon accidentally by a sordid mind, it had been the discovery of the talented and liberal donor of inventions to the public.

During the progress of my operations I paid Dr. Arnott the compliment of calling three times, and I explained to him very clearly the construction of the instrument. In the first place, I pointed out an important principle, which required that every layer of metal should be distinct from the rest, and all of them kept a certain distance apart, so that folds of any kind, in which, of course, there must be a continuity of the layers, would be faulty. I presented to him, moreover, a pamphlet, in which the importance of this was clearly explained. I next shewed him that the instrument was not made of wire-gauze, and I assigned reasons, which I will shortly explain, why that substance would not answer for the purpose. With these reasons he appeared to be satisfied, and remembered them a year afterwards, repeating one of them himself. On this occasion I shewed him the very metal-work of which the respirator is made. Lastly, I mentioned that however dear the instrument might then appear, it had yielded me no profit, but rather a loss. If the reader will again examine the quotation from the author, he will perceive, upon each of these points, statements which justify the opinions of those who have pronounced his remarks to be written in the studied language of disparagement.

Holding a power over the respirator in every practicable form, sordid, indeed, would be my feelings, if there were any foundation for the charge of avarice and inhumanity brought against me, in no measured terms, by the author. It offers a striking contrast to his own generosity, ably displayed in his preface. If, to make his own liberality stand out in high relief, he could not resist the opportunity of placing behind it a darkened picture for a background—though, to afford this contrast, another's fair fame must be tarnished, and his honest motives cast into the shade—we can then understand why the author has here omitted to mention the fact of his having heard, from my own lips, more than once, that the respirator had afforded me, at that time, not any profit, but was rather a cause of loss; and that I was most anxious, as soon as able, to place it within the reach of the poor, many of whom I had observed greatly to need it.

I request that my reader will permit me now to lay open my account against this instrument. In part of the expenses for a patent for this kingdom and France (for I do not charge those instruments with the whole expense), in a series of expensive experiments in my first machinery, much of which has been laid aside, and in the manufacture of the first 700 instruments, I had expended upwards of 1400*l.* before I knew that one would be purchased, or such a novelty be worn—before I knew, indeed, how they would be received by the profession. Who, in this state of things, would have expected me to put a lower price upon them? With the exception of 50, which were given away, they all sold, and for about 1500*l.* Of this, 300*l.* were the profit of the retailers, and I received 1200*l.* in place of the 1400*l.* which I had spent in money; no charge for my time, nor for any personal expenses, having been put upon the *experiment;* for such it was, at first, though it engrossed my attention for a year. No sooner had the profession and the public given me good proof of a sure support, than I, on my part, incurred more, and heavier, expenses in machinery and various apparatus, and in the establishing of processes by which the principle of the

instrument was fully carried out, and its price reduced by more than two-fifths—28 shillings being the vending price of the instrument, which had been sold, at a loss, for 50 shillings. The sales of the present season have been very large, and consequently remunerating, but the profit on each of the new instruments has not exceeded four shillings, or about 12 per cent., on the retail price. Were there no patent, a *lower priced* instrument I have no doubt there would be, but *as cheap*, or as perfect an one, there would not, for the pains and expense, to be detailed hereafter, would never have been incurred, unless protected *. The poor I have not forgotten. Already an instrument is in their hands, I believe nearly profitless to myself, and very elaborate in its execution, the *retail* price of which is from nine to twelve shillings. I have, moreover, invited the wealthy, who are deriving benefit from the respirator, to come forward, and assist my efforts to supply the poor with them still cheaper, and the destitute gratuitously. If guarded against loss by an order for ten thousand of the kind for the poor, the cost would be greatly reduced; for in this manufacture especially, the expenses are moderated by extensive operations, and I would rejoice to give to the society every portion of the benefit. Such a course as this I believe to be far better for the poor, who should be the chief objects of our concern, than the injudicious one of throwing a patent open to the public. Professing, at all times, an anxious concern for the well-being of the poor, and I trust feeling it too, the reader must pardon me for any warmth in my expressions when such sentiments as the author's are lying before my friends. More than I have already done to place the respirator, in a really efficient form, within the reach of all classes, has not lain within my power. I have not returned from the East with "the wealth of Ormus or of Ind." Had I brought home the property my occupations in that country at one time promised rapidly to afford me, I, too, could have made a gift of my labours to the public, though they would not then have been served so reasonably and so carefully. Or, if a passion for applause had predominated over the softer ones of our nature—if, to be independent of calls for money, I had avoided the ties of a parent, money would, of course, have had no value in exchange for my labour. There could be no desire, on my part, to substitute it for the higher sounding credit I might establish with the public; and the public might not be slow in granting this acceptable credit, rather than being called on to make payment in gold. The transaction might go on, too, without either the public or myself being conscious of the motives which were ruling us. In me an abundant indifference to money might have accompanied a most ungenerous spirit. In such cases, therefore, the proper touchstone for our motives should be our conduct, not in regard to the money of the public, which we had no families to need, but in regard to the fair fame of a brother, which our appetite might grudge him.

On the subject of medical men profiting by their own inventions, the author writes thus in his advertisement, page 4:—

"I had decided not to take the patent; because the stove was originally planned as a means of preventing and curing diseases—purposes for which it will always be important, whatever other advantages be derived from it; and in this country it is usual for members of the medical profession to make an offering, at once, to the public of any means for the benefit of the general health, which they may discover or devise, without stipulating for private advantages."

I think the public, and I hope the profession, will agree with me in the opinion that it would be sufficiently preposterous for medical men to take to themselves a dignity so much above others, as to establish such a rule as the

* I may here mention that, so far from putting too high a price on the instruments, I have, contrary to the repeated remonstrances of my friends, been rating them too low, the profit on them not being an adequate return for the expenses, and for the very great trouble bestowed upon the manufacture. I anticipate, therefore, being under the necessity, next winter, of raising the prices of all instruments but those for the poor. I have been submitting to a pecuniary sacrifice (as any reader will readily believe when its construction is described to him), in order to give full effect to the principle of the instrument, and to make it as convenient and elegant as possible. The public, therefore, next winter, when the demand is certain of keeping pace with the supply, must be prepared themselves to bear whatever addition I may find it necessary to make to the price, in order that the present efficiency of the instrument should be preserved; for I cannot consent to compromise these, with the view of saving them a few shillings in its price, should it become requisite to raise it. At present, however, the prices will remain unaltered, as I wish to maintain the employment of my establishment steady during the summer.

author has here arbitrarily laid down. Just as well might they feel too ennobled to touch the guinea, in the handling of which the public well know they feel as much pleasure as would any other men, and reasonable pleasure too; for how needful are guineas for the preservation and welfare of the fleshly man, and his offspring (not to say the intellectual also) in this our world of gross matter, ever suffering decay! The ruling motive has been no less a private one, whenever medical men have given up their plans to the public, prudential feelings causing them to prefer looking for indirect gains, from an increased professional connexion unattended with risk, rather than to the less certain and the expensive course of seeking profit through a patent. Moreover the practising physician cannot well be occupied in other operations of business, without appearing to neglect that of his profession.

Granting, however, that a man, already independent, or wholly regardless of the interests of himself and of those around him, and *exalted too, above the love of fame* (which is by far the noblest and most difficult exaltation to attain to,) shall be disposed to throw open his invention to the public, I cannot consider such a course to be a judicious method of bestowing one's liberality.

Three things make up the price of a patented article; the materials, the ordinary profit of manufacturing and selling them, and the profit due to the inventor, for the employment of his time and talents. These are the legitimate charges upon the article; and if it be really useful it will sell readily, affording a proof that the purchaser feels himself a gainer by it, after having paid these charges.

No proper reason can be assigned, why he should be exempted from the payment of *any one* of them, more than of *any other;* why he should get the valuable *form* for nothing, any more than why a present should be made him of the *materials*. A valuable invention may be rendered a very gratifying means of bounty, but it should be employed in the most useful manner for the benefit of the needy, and not lavished indiscriminately among those, often the wealthiest, who are already gainers by the utility of the invention. For this end a patent should be taken out, and its resulting fruits be given to public charities, or distributed in other suitable ways. In this manner the people who were deriving benefit from the invention would be made to contribute towards the support of some excellent institution, or towards some other work of good; whereas to give indiscriminately to the persons benefiting by its use, whether rich or poor, the legitimate profit of the invention besides, appears to be a very mistaken course. However well it may be intended, it exactly corresponds in effect with the ostentatious profusion of Eastern monarchs, who on grand occasions throw guineas, during their processions, to the populace on all sides, reckless whether the feeble and aged, or the avaricious and sturdy, shall pick them up, so that their object of an ostentatious display be but fulfilled. So different is my own opinion upon this subject from the author's, that while he is recommending upon principle that patents should not be taken out, upon principle would I urge every one, who has the means and the heart to incur the risk, on no account to omit doing so where there was a reasonable prospect of gain; and I would then exhort him to devote all, or a portion, of the fruits to works of beneficence for the needy; and in other acts of charity, which the eye of man shall never behold, nor his applause ever vitiate*.

ON THE

REMOVAL OF CAPSULAR CATARACT

THROUGH THE SCLEROTICA.

By Richard Middlemore,

Surgeon to the Birmingham Eye Infirmary.

(For the Medical Gazette.)

There are certain forms of capsular cataract which are not oftentimes removed by the surgical methods now employed, without considerable difficulty. It is my object, in the present communication, to make some remarks—1, On the various forms of capsular cataract, and on lamellar lymphatic (false) cataract; 2, On the means usually employed for their removal; and 3, On a new mode of operating, by the intro-

* I find the pressing occupations of business ill to afford me time for the production, weekly, of these papers, of which the subject matter even has to be prepared. The next paper will not, therefore, be published until the 21st inst.—J. J.

duction of an instrument through the sclerotica.

Pathological states of the Eye to which the Operation about to be proposed is adapted.

1. Primary double capsular cataract is represented by Gibson to be in some instances congenital; but I do not remember to have seen a single instance tending to confirm this opinion. It consists of an opacity of the two layers of the capsule, which are generally tough and thick, sometimes united by an organized medium, but more generally in mere apposition. This form of cataract may, or may not, have contracted abnormal adhesions to surrounding parts.

2. Another condition of disease, to which the operation I am about to suggest is especially adapted, sometimes succeeds the operation of keratonyxis, performed on the eyes of children suffering from soft lenticular cataract; and this is particularly liable to happen if the operator neglect to lacerate the anterior capsule very freely at the first operation*. The condition of disease produced very closely resembles that which is left when congenital cataract has been suffered to remain until adult age; that is, the lens is absorbed, the anterior and posterior layers of the capsule become opaque and thickened, the anterior capsule falls down upon the posterior capsule, and these two layers constitute, in effect, a tough membrane, firmly fixed in its situation by other than its normal extent of connexion†.

3. A portion of capsule may be left behind, after an operation for extraction, by a careless and defective operator—perhaps, indeed, by an attentive and skilful one; and this, in as far as it may seriously impair or destroy vision, may require removal.

4. After the operation of extraction (the anterior capsule being freely lacerated, and the lens readily removed), the posterior capsule may inflame, its texture may be rendered thick and opaque, and strengthened by intertextural lymphatic deposition (*secondary posterior capsular cataract*), or a layer of lymph may be deposited upon its surface (*lamellar or membraniform lymphatic* (*false*) *cataract*). However, secondary capsular cataract, or opacity of the posterior capsule, the anterior capsule and the lens being removed from the axis of vision or absorbed, is now somewhat rare, owing to the comparative disuse of depression and reclination, and to the improved mode of operating for extraction, by which the posterior capsule is either quite uninjured or entirely removed from the axis of vision at the time of the operation.

What forms of Operation are usually employed for the removal of the preceding conditions of disease?

Wenzel* advised, that whenever the capsule has become opaque after the displacement or extraction of the lens, it (the opaque capsule) should be removed by a small forceps introduced through an opening made by his extraction knife in the cornea.

I can scarcely tell what was the method advised by Saunders: he appears to have relinquished the matter in despair, for he states that the united capsules resulting from the long continuance of congenital cataract constitute a substance "which the surgeon will in vain endeavour either to extract or depress†."

The treatment proposed by Gibson was essentially the same as that advised by Wenzel, except that the incision of the cornea was smaller, the opaque matter being punctured with the point of the knife, and removed by a hook instead of a forceps‡. His directions upon the subject are very minute, and are marked by great precision. Since the appearance of Gibson's little publication, no material improvement in the surgical mode of treating the *primary double capsular cataract*, requiring particular notice, has taken place.

In performing this—which may be called Wenzel's and Gibson's—operation, we find that the part divided is transparent, and anterior to the pupil; we find, also, that several instruments are generally used; that the operation is liable to be greatly protracted; and that a necessity for the frequent introduc-

* See my remarks on this subject in the British Annals of Medicine, No. 19, reprinted in the Continental and British Medical Review, No. 4.

† This condition of disease is fully described in one of my clinical lectures on Congenital Cataract, published in the London Medical and Physical Journal, for June 1833.

* Wenzel on Cataract, by Ware. 1791.

† A Treatise on some Practical Points relating to the Diseases of the Eye, p. 156. London, 1816.

‡ Practical Observations on the Formation of an Artificial Pupil, &c., p. 127. London, 1811.

tion of instruments may arise. Sometimes the operation cannot be effectually completed; sometimes serious inflammation occurs, and the patient's vision is destroyed by a condition of disease far more difficult of relief than that the operation giving rise to it was intended to remove. I have seen these inconveniences take place even in the hands of those accustomed to practise ophthalmic surgery. It is the main object of this paper to propose a more simple and perfect method of cure. By this method, the tough opaque membrane is drawn through the opening in the sclerotica, by an appropriate instrument—by an instrument which shall admit of easy introduction, which shall not make a large wound, and which shall be so constructed as to grasp and withdraw, through the sclerotic aperture, the opaque membrane. The instrument I have had made consists of a fine spear-shaped needle, inclosed by the points of a forceps, so adapted as to form a continuous and smooth surface when employed in puncturing the sclerotica; but, when duly introduced, the needle is withdrawn, the blades of the forceps (by being in the minutest possible degree retracted) slightly severed, and adapted to some convenient part of the membrane, which is then grasped and withdrawn. I am not, however, satisfied with this instrument, and hope some of the readers of the Medical Gazette, whose ingenuity is not so much *below par* as mine, will be able to perfect the construction of this; or, by having their attention drawn to the subject, devise some more convenient instrument for the purpose. It has occurred to me that a delicate flattened trochar and canula may be constructed: with this the sclerotica may be punctured at a suitable distance from the margin of the cornea; the sharp needle, or flattened trochar, may be withdrawn, and through the canula a fine and properly adapted forceps (made of a material resembling very fine watch-spring) may be passed through the flattened canula within the eye, when, on appearing at the extremity of the canula, it (the forceps, in right of the material of which it is made) will sufficiently expand to seize the tough membrane. By a slight impulsion of the canula, the blades of the spring forceps are closed upon the membrane, and the whole instrument is then withdrawn. It is intended that the needle, or flattened trochar, shall be very small, shall project but very little beyond the extremity of the canula, and shall be so introduced that its point shall pass through the opaque membrane; upon which, on the removal of the needle, it is intended the canula shall rest. The spring forceps being introduced, may be so manœuvred that one blade shall pass through the aperture made by the needle, in front of, and the other behind the opaque membrane. This manœuvre may, I think, be readily enough accomplished without causing the escape of at least any material quantity of the vitreous humour, by a slight elevation of the external extremity of the canula.

Operation performed on the eye of a young rabbit.—I carefully punctured the lens, by passing a fine needle through the cornea, on three separate occasions, at intervals of about a fortnight. At the expiration of four months from the time of the first puncture, there existed, in the place of the convex lens, &c. a white, tough, flat membrane.

Concluding Operation.—Passing the needle and forceps* through the sclerotica, about the sixth of an inch, behind the cornea, I gently pressed it forwards until its point just penetrated the opaque membrane (I then ceased to press it, when, by means of a spring, it retreated behind the points of the forceps,) then urging the forceps until its *nearly sharp* extremities passed through it, I carefully separated the blades†, by a slight retraction of the instrument, and grasping the opaque membrane, brought a sufficient portion of it away to make a very good pupil. There was no difficulty in introducing the forceps, which were very accurately adapted to the neck of the needle with which the sclerotica was punctured; but having done this, and pushed the forceps forwards, then it became almost impossible so to separate the blades as that one of them should pass before and the other behind the opaque membrane; when, however, this was accomplished, then, in consequence of the delicacy and sharpness of the points of the forceps, the tough capsule was so pinched and torn that it was removed only partially and irregularly.

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* This instrument, which has been already described, was constructed from a rough model made from my directions, by an ingenious optician.

† The difficulty of doing this was considerable; but this part of the difficulty may, I am satisfied, be wholly overcome by employing a more perfectly and delicately-constructed instrument.

In another communication I intend to complete the details of the plan of which I have now done little more than furnish an outline. In the meantime, as the operation at present performed for the relief of the forms of disease under consideration is avowedly susceptible of great improvement, I trust that some of the large number of surgeons who are now devoting a portion of their time and attention to the advancement of ophthalmic surgery, will endeavour to supply the admitted deficiency.

ANGINA PECTORIS,

ACCOMPANIED BY

A Peculiar Affection of the Left Hand.

WITH REMARKS.

By E. Headlaw Greenhow.

(*For the Medical Gazette.*)

Alice Crake, æt. 63.—Is a countrywoman, and has been accustomed to work in the fields all her life. In the earlier part of her life was very delicate, but has latterly enjoyed good health; has occasionally suffered from subacute rheumatism, and has been always subject to occasional, but slight, attacks of palpitation. She is of spare and weak habit, and had the influenza in January last, from which she speedily and perfectly recovered. Some months ago I attended her for a disease of the fifth toe of the left foot: it commenced suddenly with most excruciating pain, and in the course of three or four hours the toe became black, with a tendency to vesicate; a lancet was plunged into it, which occasioned considerable hæmorrhage, and some relief; a light stimulant dressing was applied, with a poultice over all; her bowels were kept moderately open, and she took a dose of calomel and Dover's powder each night. Under this treatment she recovered in twelve or fourteen days, and has continued quite well until the present attack.

About six weeks ago she told her husband she was afraid she would not be able to work at the harvest, as she felt a pain in the breast: it, however, left her, and she worked for five or six days without suffering at all. She then began to feel a sense of tightness or suffocation in the chest when she stooped down, which obliged her to discontinue her labour for a few minutes, and then left her spontaneously, but after three days she gave over working altogether, and then continued free from complaint until about a fortnight ago, at which time, whilst going up stairs, she felt a sort of contraction in the chest, producing suffocation, and obliging her to rest, when it presently went off. The next day, on using some exertion, she had a return of it, and this time the pain extended to the back and shoulders. These attacks did not continue longer than five minutes, but the succeeding ones gradually increased in frequency and duration, sometimes continuing nearly an hour, and occurring three or four times in the course of the day; they also extended upwards towards the throat, and successively to the attachment of the deltoid muscles, then to the elbows, and latterly to the wrists, both arms being affected alike. The pain commenced about the centre of the chest, and was sometimes, though by no means invariably, attended by palpitation. It then extended backwards towards the spine and shoulders, and lastly to the arms.

Monday, 16th October, 1837.—She sent for me this evening during a paroxysm, but it had gone off when I saw her, leaving a sense of numbness and soreness in the parts affected by the pain; her face was covered with a cold, clammy perspiration; countenance anxious; pulse 70; impulse and sounds of the heart feeble. On auscultation a slight *bruit de soufflet* is distinguishable. Bowels regular; tongue clean; appetite good; she has no cough, expectoration, or dyspnœa; the sense of suffocation experienced during the paroxysm not affecting the respiration; has no œdema of the lower extremities, and makes her urine quite plentifully. The stomach frequently feels distended during the attacks, and at these times an eructation of flatus either terminates or very much mitigates the violence of the paroxysm.

℞ Pulv. Ipecac. Comp. ℈j. Aloes Socot. g. x. Extract. Gentian. ʒss. M. ft. massa in pilulas xij. divid. Cap. ij. spasmo urgente.

Oct. 19th.—Had a very violent attack last evening, attended by palpitation: says the pain in the shoulders during the latter attacks, which have been induced by the most trifling exertion, is

as if some one was forcibly twisting her arms out of their sockets; her general health is becoming affected, she having now lost all relish for food; pulse 78; respiration natural; is much relieved by the pills, which she takes freely during the attacks. I desired her to keep perfectly quiet, and to take simple and nourishing food; may have a mutton chop, but no stimulants.

℞ Mistur. Camphoræ ℥viiss. Tinct. Colchici Comp. ℥ss. M. Cap. ℥i. ter die.

October 20th.—She had no further attack yesterday, but has had two today, although she has kept quiet and in bed since my visit of yesterday. The first attack, which was unaccompanied by palpitation, was very severe, the pain extending to the tips of the fingers, and continuing about three quarters of an hour. The second, which had just gone off when I saw her, was much less severe, having only continued a few minutes, but came on suddenly as she was lying quiet in bed; pulse 84, rather feeble; surface cool; respiration quite easy and natural; the action of the heart is extremely feeble, the impulse being scarcely perceptible, and the sounds very dull.

22d.—The attacks have been less frequent and much less acute since my last report, but they are now accompanied by vomiting, and considerable soreness and numbness remain afterwards; does not herself experience any palpitation, but another person, on applying the hand over the lower portion of the sternum, feels the heart pulsating strongly and rapidly during the attacks; complains much of weakness and loss of appetite; tongue clean; bowels regular; sleeps badly; pulse 84, feeble.

Rep. Mistura addens. Tinct. Colchici Comp. 3ij.; Tinct. Hyosciami, ℥ss. M.

24th.—She has had no paroxysm since my visit on Sunday, the 22d; but her spirits are depressed, and without amounting to actual pain, she has a constant uneasiness in the chest. Pulse 80.

25th.—Had a very violent paroxysm last evening, and another at two o'clock this morning, which did not entirely leave her for several hours: the pain is less acute than it was, but it now extends quite down to the tips of the fingers. Thinks the attacks are most acute, and commence rather to the left side of the chest.

26th.—She sent in haste this morning, and in consequence of my absence was seen by my father, who found that a most excruciating pain had suddenly come on in the index finger of the left hand; the finger was cold and livid from the tip to the proximal end of the second phalanx. She had a paroxysm of angina this morning previous to the finger becoming affected.

℞ Spir. Camphor. ℥iss.; Tinct. Opii, ℥ss. M. ft. Lotio pro digito.

℞ Mistur. Camphor. Hj.; Sp. Æther, Nitrici. 3vj.; Tinct. Opii, gtt. lxxx. M.

Capt. Cochl. ij. magn. 3tia quaque hora.

Evening.—The pain in the finger is quite relieved, but it is deadly cold, still of a leaden hue, much shrunk, and quite numb; pulse 90, feeble; has no appetite; has suffered much from occasional palpitations during the last twenty-four hours; tongue moist; had complained much of headache all day, which has left her this evening; had a paroxysm of angina this evening when she was taken up to have her bed made. I allowed her some strong beef tea and a single glass of Port wine.

27th.—Has had two bad attacks of angina since last evening, but unattended by vomiting; the last one continued three quarters of an hour; has a constant paroxysm at the precordia, and has suffered much from palpitation this morning; bowels costive; tongue clean; pulse 84, feeble.

Capitat cras mane, Olei Ricini. ℥i. To have a mutton chop for dinner.

28th.—Her bowels have been freely opened by the castor oil. Has had no attack of angina since I saw her yesterday morning, but still complains of uneasiness in the chest; the apex of the heart is felt pulsating about the centre of the lower portion of the sternum. Between eight and nine o'clock last evening the pain returned into the finger; but on this occasion the hand also was affected; it continued for several hours until relieved by the camphor and laudanum lotion; towards this morning she had a little disturbed rest, but otherwise passed a sleepless night. The whole forearm and hand feel cold, and the pulsation of the radial artery ceases to be at all perceptible a little below

the elbow; but the vessel itself can be traced downwards to the wrist, feeling quite hard, as if ossified or very cartilaginous, and not being collapsed. The right arm is of its natural temperature and colour, and the pulse is perfectly distinct at the wrist, although the artery feels hard on this side also, though less so than on the left. Pulse 86, rather fuller than it has hitherto been; complains much of soreness on pressure over the epigastrium.

To have mild nourishing diet. To apply a common poultice, containing two tea spoonsful of mustard in it, to the left hand and wrist.

To use a pediluvium, with two table spoonsful of mustard in it.

Cont. Mistur et Lotio.

℞ Pulv. Opii, gr. iss.; Camphor rasæ, gr. v.; Cons. q. s. M. ft. Bol. hora somni sum.

29th.—Has had two attacks of angina since last evening, and justly remarks that it seems to alternate with the complaint in the hand; she has had a more comfortable night, having slept for several hours; she still complains much of precordial oppression and of soreness at the epigastrium; the hand and arm are much warmer to-day, though still below the natural temperature; the pulse is scarcely perceptible at the left wrist, but gradually increases in strength as it ascends towards the elbow; pulse 86, rather feeble; had complained rather of headache this morning; has suffered from occasional palpitation to-day. The last attack, she says, is the worst she has yet experienced, and was attended by vomiting; feels very feeble, and lies upon the back.

May have a glass of Port wine and a mutton chop for dinner.

To continue the mustard poultices and pediluvium, and to apply the lotion whenever the pain returns into the hand.

Rep. Mistura. Rep. Bolus, hora somni.

30th.—Is easier to-day, but continues much the same in other respects. The temperature and circulation of the hand seem to be merely sustained by the stimulating poultices.

Capiat bol. ut heri hor. som. et ℥j. Ol. Ricini, cras mane.

31st.—She has had no paroxysm of angina since Sunday (the 29th), but has been suffering much from the hand all night; cannot bear to have it touched, without the greatest gentleness, the index-finger and palm being especially painful. The whole hand is shrunk, shrivelled, and livid, excepting one or two reddish spots near the knuckles, about the size of a sixpence each. The pulse ceases abruptly in the radial artery a little below the elbow, from which place it gradually increases in strength as it approaches the trunk. Complains of soreness, on pressing ever so gently, along the course of the vessel; the venous circulation of the left hand is very feeble; left hand and arm much cooler than the right, and bedewed with a cold clammy perspiration; feet warm; complains much of a sharp, sudden, momentary pain, shooting through the chest at irregular but frequent intervals, this morning. Pulse 60, feeble; slept for a couple of hours after the bolus, last night; bowels have been freely relieved by the castor oil; tongue red and furred; appears much sunk, and has no appetite. To continue her mutton-chop and beef-tea, and may have a little port-wine occasionally. To continue the mustard poultices and pediluvium.

Rep. Bol. h. s. s. Cont. Alia.

November 1st.—The pain in the hand and arm entirely left her in the course of last night, and her daughter informs me that the warmth and circulation had quite returned to it this morning. She complained much of precordial oppression, and a sense of suffocation. At 8 A.M. an attack of angina, attended by vomiting, came on, which continued until a few minutes before her death, which was very tranquil, and took place about 9 A.M.*

I could not obtain permission to examine the body, all the friends being opposed to it.

There are few diseases respecting which more various and dissentient opinions have been formed, than of that at present under our consideration. Whilst post-mortem examinations have never yet shown any invariable morbid appearances as peculiar to the disease, there is scarcely any organic change of

* I should have mentioned before, that this poor woman had the character of being formerly addicted to drinking, but was said to have left it off latterly: she had experienced great reverse of fortune, and consequently much mental distress.

the heart, or lungs, which has not been found after death from it. These changes, however, since on the one hand they do not always produce angina, but, on the contrary, most frequently exist independently of it, so likewise, on the other hand, angina is frequently found where not only none of these changes but no morbid appearance whatever can be detected, ought, as we shall see presently, to be considered rather as concurrent than as exciting causes of the disease.

Some authors have supposed that this disease arises from an irregular distribution of the blood, and especially an accumulation of it in the heart and large vessels, and they have endeavoured to account for this by an obesity about the heart, by ossification of the coronary arteries, and by general plethora; but well-ascertained facts amply prove all these opinions to be incorrect, for many cases of angina pectoris have occurred where the heart was remarkably lean and flabby, where no ossification of the coronary arteries existed, and, as in Crake's case, where, instead of being plethoric, the patient was of spare and weak habit. I myself well remember the case of a very plethoric individual, about whose heart I found an immense quantity of fat, and who, indeed, died from disease of that organ, and yet, during his long illness, he never had any symptoms at all resembling angina pectoris; also of another, who was not supposed to have any disease whatever of the heart, in whom the coronary arteries were much ossified.

But perhaps the most generally received opinions at present are, that it is a spasmodic or neuralgic affection of the organs supplied by, or connected with, the pneumogastric and cardiac nerves. The only reason that there seems to be for attributing it to spasm, is the description of the paroxysm given by persons labouring under the disease; but who in this, as in other cases, are very apt to apply the term spasm to any violent pain of internal organs, and from some supposed analogy to diseases of a confessedly spasmodic character.

It appears to me, however, that it is more truly considered as a neuralgic affection, which, according to various circumstances modifying the individual case, affects sometimes the heart, or the lungs only, whilst at other times both organs, and in more violent cases the stomach also, are affected. Upon this supposition the agonizing and peculiar pain in one, or (as in Crake's case) in both arms, is easily explained by the connexion existing between the pneumogastric nerve, the accessorius, and the cervical plexus.

Certain authors, at the head of whom is Dr. Chapman, who has devoted much attention to the elucidation of this disease, have supposed that it is merely a form of misplaced gout; and, certainly, when we consider the great frequency of its occurrence in gouty and rheumatic individuals, and the numerous cases which these authors record, in which persons suffering from angina pectoris have experienced immediate and perfect relief on the appearance of the regular gouty paroxysm, there seems, at first sight, to be considerable justness in this opinion. I am myself, however, disposed to consider that its connexion with gout is merely incidental, and may easily be accounted for when we consider that both diseases seem to originate in a morbid state of the nervous system, in individuals who are generally verging upon middle life, and who have, by their mode of living, been exposed to the action of causes which tend to diminish the energy, or derange the function of the nervous system; such are, intemperate persons (either in eating or drinking), persons who have led an indolent and inactive, or who, on the other hand, have led a studious and sedentary life, including also such as have been exposed to disappointment, or severe continued mental distress or excitement. It appears, then, quite natural to suppose, that diseases arising from similar causes, and existing in similar constitutions, may very frequently coexist in the same individual, without their being in any other way connected; and the preceding case seems to throw some light upon the strongest evidence which has been adduced of the supposed identity of angina and gout, viz. the subsidence of angina on the appearance of gout in the extremities, and *vice versâ*. In Crake's case the angina alternated in a very remarkable manner with the external disease of the left hand; and yet I feel assured that no one can for one moment suppose that disease to have borne the slightest resemblance to gout. The simplest mode of explaining this curious and in-

teresting fact, is, that the disease changed by metastasis from the chest to the hand, and *vice versâ*, according as the predisposition to the one or the other affection prevailed; or we may, perhaps, very aptly compare the external disease to a counter-irritant, which tends to relieve the internal disorder, by concentrating, as it were, the morbid energies of the system to the newer and more acute external affection; and it is precisely in like manner that I would explain the relief experienced in angina, on the appearance of gout externally, and not by supposing angina to be misplaced gout.

From these facts, then, it would appear that angina is a disease *sui generis*, originating in a morbid state of the nervous system; and, if this view be correct, whilst every thing tending to produce debility or depraved function of that system, and especially of that portion of it specially affected in angina, will predispose to the disease, the various diseases of the heart, lungs, &c. (such as ossification of the coronary arteries, valvular disease, hypertrophy, atrophy, or obesity of the heart, &c. &c.) occasionally found after death, may be considered rather as concurrent causes of angina than as bearing any more essential relation to it; and gout, various derangements of the alimentary apparatus, fits of anger, disappointment, or any other of the depressing passions, may act as the exciting causes of the disorder.

Newcastle, March 1838.

CREOSOTE AS A LOCAL APPLICATION.

To the Editor of the Medical Gazette.

Sir,

Should you deem the following cases, illustrative of the efficacy of creosote as a local application, worthy of a place in your journal, you will oblige me by inserting them at your convenience. To avoid falling into the too common error of overrating the actual merits of a comparatively new medicine, no less than encroaching unnecessarily on your time and space, I shall give the cases in the simplest possible form, and allow facts to speak for themselves.

I have the honour to be, sir,
Your most obedient servant,
George Fife, M.D.

Newcastle-upon-Tyne,
22, Eldon Square, March 17, 1838.

Case I.—Mr. W., æt. 86, suffered in November last from erysipelas of the left leg and foot. A large vesicle formed on the outer side of the fibula, about three inches above the mallcolus, burst, and left below a large ulcer, about the size of a half-crown piece, but of an irregular oblong figure. The surface of the ulcer was of a dusky brown colour, and consisted of a firm tenacious slough, which shewed no disposition to separate, nor did it yield in the least even to the application of a dressing forceps. A thin fœtid sanies was very copiously discharged from the sore, which, after the application of the carrot poultice, and the usual constitutional remedies, for some days remained precisely in *statu quo*. To counteract a decided typhoid tendency, stimulants, bark, and anodynes, were liberally employed. In consequence of the unimproved condition of the sore, an ointment, composed of forty minims of creosote to an ounce of lard, was applied, with a simple white-bread poultice over it. The first of these dressings was removed at the lapse of twelve hours, when the surface of the slough was evidently less firm. At the next dressing a more decided change was apparent, portions of the surface of the slough being readily detached by the forceps, and a distinct line of separation between the slough and edges of the sore. The discharge also now began to assume a more healthy character; and in about four or five days from the first application of the creosote, in consequence of the ulcer presenting a florid, healthy, granulating surface, with a copious and well-digested purulent secretion, the poultice was discontinued, and the ointment used alone. From the formation of a sinus, an injection was found necessary, composed of eight minims of creosote to four ounces of distilled water. In the course of a few days more the calamine ointment was used, with gentle pressure, and cicatrization went on rapidly. The creosote was first used on the 2d of December;

the cure was completed in the first week in January.

It is proper to state, that this patient had suffered from varicose veins, and has had at different times decided indications of defective circulation in the lower extremities. He continues well at this time.

Case II.—Mr. ——, æt. 20, complains of sore throat; right tonsil ulcerated. The ulcer is of an ashy colour, irregular in form; edges slightly elevated; no distinct induration. Is suffering at this time from the chronic stage of gonorrhœa; has never had primary symptoms of syphilis. The ulcer not improving under the use of the muriatic acid gargle, it was touched three or four times with the nitrate of silver; at the same time the pil. hydrarg., with James's powder, was taken internally. No improvement taking place, the creosote gargle was prescribed, and in consequence of the gums being tender and tumid, Plummer's pill was ordered in lieu of the blue pill, &c. In about ten days the ulcer was perfectly healed. The Plummer's pill was continued for ten days longer; and up to the present time the throat remains quite well.

In this case there was what I considered as sympathetic bubo, which yielded to leeches and poultices, cold applications having done no good.

Case III.—Master ——, æt. 11, has for several weeks suffered from an ulcer on the foot, which he cannot clearly account for, but which, from its appearance, I consider to have been the result of chilblain. It has been healed two or three times, but has always broken out afresh on the re-appearance of frost. It has the appearance of a foul ulcer, and is accompanied with a thin ichorous discharge. The creosote ointment, after being used for a few days, converted it into a healthy granulating sore. It is now granulating under the application of the calamine dressing, and is very nearly healed.

In this case, from the induration of the edges, the nitrate of silver was twice used, subsequent, however, to the discontinuance of the creosote.

Case IV.—Mrs. W., æt. 83, suffered from ulceration, threatening gangrene from pressure on the nates, occasioned by a protracted confinement to bed. An ulcer formed, equal in size to a shilling, its surface foul, and the adjacent parts presenting the appearance common in such cases. In a few days after the creosote ointment was applied the ulcer assumed a healthy character, and the surrounding parts their natural appearance. Cicatrization was complete in about three weeks.

Case V.—For the present very important and interesting case I am indebted to my talented friend Mr. W. W. Browne, surgeon at Jarron, near South Shields. Whether we regard the malignity, if not incurable nature, of the disease, which has long been admitted to be one of the opprobria of surgery, or the effects of the remedy in question over it, it is equally entitled to consideration. I shall give the case as communicated to me by Mr. Browne:—

"*Lupus.*—The patient is a woman, æt. 35, strong and generally healthy; married; has had four *bairns;* never had syphilis. States that seven years ago the disorder first appeared in the form of a small knot, attended with heat and pain on the mucous surface of the right ala nasi. A hardened crust or scab soon formed, which she removed; and although the ulcer beneath was painful, paid no attention to it, till the skin of the part began to assume a livid aspect. The ulceration gradually involved the anterior borders of the lateral cartilages, septum nasi, and alar cartilages of both sides, to some considerable extent. During this time has submitted herself to the care of several members of the faculty, without experiencing any relief. She applied to me early in December 1837, when her appearance was really pitiable. The ulceration had extended some way within the cavity of the nostrils, with the secretion of a thin fœtid ichorous discharge; the skin surrounding the nose, livid in colour, was studded with a number of superficial ulcerations, concealed beneath dark brown incrustations. The tip and wings of the nose were entirely destroyed. Having seen several obstinate ulcerations yield to the employment of creosote, it was made trial of in this. The form chosen was the ointment, in usual proportion, 3ss. of creosote to an ounce, not of lard, but cerat. calamin.; the incrustations were care-

fully removed with the forceps, and the ulcers dressed daily. The bowels were moved by ordinary medicine, and grs. x. of Plummer's pill prescribed daily. Under this treatment the ragged nature of the ulcer first disappeared, and cicatrization speedily followed. If we except the livid nature of the skin surrounding the part, the patient was well at the end of the month."

The severe frost has affected it slightly, but no ulceration has reappeared. The lupus attacking the nose was clearly the "deep erosive" of Biett; that attacking the cheek was, I think, more allied to the "superficial" of the same author.

The above cases will, I think, be received as tolerable evidence of the utility of creosote in not merely obstinate, but even malignant ulcers; and to these many others might be added. It is, however, only an useless expenditure of time to do so. I shall content myself with affirming, that in no case of ulceration in which I have yet tried it has it disappointed my expectations. I was first induced to try it from the perfect success attending its application to a case of cancer in the ear of a dog, about two years ago, a disease which I have no doubt many of the readers of the GAZETTE know to be most intractable; in fact, to defy every other means. In the case in question it was used in its concentrated state. At the time I had not seen Dr. Gully's admirable little work. The following cases tend to establish its efficacy in a very opposite class of affections:—

CASE VI.—On the 23d of January I was sent for to visit a young married lady, at the time nursing her first child. Her symptoms were such as to lead me to apprehend a decided attack of continued fever. The head being a good deal affected, leeches had been applied previously to my visit. An emetic, followed by calomel and antimonial powder, with the common black draught, were immediately prescribed.

On the 24th I found all the febrile symptoms subdued, and the pain of head limited to the left side of the forehead and temple.

> The emetic ordered to be repeated each evening. Hydr. cum cretâ and antimonial powder at night, and the black draught each morning.

26th.—Pain of the head has assumed a distinct periodical character, and is entirely confined to the supra-orbitary branch of the fifth pair; the pain so severe, that the eye can scarcely be kept open when exposed to the light. The paroxysm most severe from eleven o'clock A.M. to the evening.

> Prescribed the creosote ointment, to be rubbed on the part affected twice daily; also the sulphate of quinine, to be taken internally.

The first friction with the ointment was on the morning of the 27th; the paroxysm less severe, and shorter in duration.

28th.—Made my visit about two P.M., at which time the pain was generally most intense; present paroxysm very slight.

> Ordered the friction to be continued, and used three times daily. The quinine also to be persevered with.

31st.—Much better. After this date I had not occasion to repeat my visit.

CASE VII.—Of the details of this case I am not professionally aware, the subject of it not being my patient, and for whom I merely wrote the prescription at the request of an intimate friend. The subject, a lady above the meridian of life, I was informed had for fifteen or sixteen years been subject to the most excruciating pain in the nerves of the face, and was at the time suffering intense pain. She used the creosote ointment, and with immediate relief. I have not heard any additional particulars.

CASE VIII.—Master C., æt. 15, has suffered for five or six weeks from a very severe attack of acute rheumatism, accompanied with some gouty appearances, the left hip, both knees, ankles, feet, and metatarso-phalangeal joints of the great toes, being severely affected. I was requested to visit him on the 7th of January, when I found him extremely reduced both in flesh and strength. Heat, pain, and swelling, in the parts above enumerated. Such was his dread of the night, that he entreated me to give him something to make him sleep. For especial reasons I abstained from opiates, but ordered him the colchicum mixture internally; and after fomentation of the affected parts with a hot and strong solution of salt in water, the creosote ointment to be used as a

friction so far as it could be borne; at all events to be lightly smeared over them.

Jan. 8th.—I found on my visit this morning, that he had passed the first good night since the commencement of the attack, having fallen into a sound sleep soon after the fomentation, &c., and slept undisturbed by pain until about eight o'clock. The parts affected free from pain; swelling much reduced; redness gone.

9th.—Better; no pain.

Contin. medicamenta.

10th and 11th.—Better.

12th.—So much improved that the colchicum is discontinued, and the quinine prescribed. Progressed from this date to perfect convalescence up to the 19th, when I discontinued my visits. Notwithstanding the intensity of the weather, has up to this time remained quite free from ailment.

This case may, in my opinion, be regarded as one of rheumatic gout, not only from the symptoms present, but also from the fact of gout being a disease to which the family are prone.

Case IX.—The subject of this was the same as in Case IV. She fell in getting into bed, which, from a very severe contusion she received on the left hip, led me to suppose at first that there was fracture of the neck of the femur, or about the trochanter. On a most careful examination no crepitus could be discovered, nor did there exist any of the symptoms indicative of luxation of the hip-joint, which, from the extreme attenuation of the subject, should have been readily discovered. She suffered much from pain and spasmodic twitching of the affected limb. Fomentations not relieving it, and ordinary anodyne liniments having no better effect, I ordered the hip, knee, and ankle, to be rubbed with the ointment, which she used as a dressing to the sore. The pain was from the first materially relieved by it, and in a few days subsided altogether. Whenever she has been threatened with a return of it, she resorts to the ointment, and invariaby with benefit.

To the above cases may be added two of severe pain in the ankle after sprain. One of them was in a young lady, who, after all the severer symptoms concomitant on such cases had subsided, was confined for three or four months to a sofa from intense pain, and who gradually, and in a very short time, recovered under the use of creosote, after bloodding by leeches and blistering had proved of no avail. The other was my own case: the particulars are briefly these:—In riding over to Durham, twelve months ago, my horse fell in the snow, with my right leg under her; the consequence of which was a very bad sprain—the worst I have ever witnessed. After the acute inflammation had subsided, pain continued, and, until the creosote was used, effectually debarred me from sleep. The first friction relieved me, and in two or three days I was free from suffering. It is hardly necessary to say, that in these cases the pain must be ascribed to injury of the nervous filaments of the parts.

NEW FORM OF PESSARY.

To the Editor of the Medical Gazette.

Sir,

In a case of prolapsus uteri, which recently came under my care, accompanied with the most distressing spasmodic irritability of the muscles of the vagina and adjacent parts, I was repeatedly disappointed in my expectations of retaining either a circular or oval pessary, in consequence of the violent muscular contractions, which almost invariably whirled the instrument from its situation, thereby increasing the distress it was intended to relieve. Under these circumstances I made trial of one of the stem pessaries, recommended by Dr. Ramsbotham, and described and figured in the 16th vol. of the Medical Gazette, page 534, but was compelled to desist, in consequence of the agonizing pain occasioned by the irritable state of the vagina when the broad surface of the crown of the instrument came in contact with it. It occurred to me, that could I contrive to affix, by means of a hinge, a stem to a circular or oval instrument, *after* its introduction into the vagina in the usual way—*i. e.* in the antero-posterior direction, or long axis of the opening—that I should overcome the difficulties which perplexed me. This I effected. The success attendant upon

the first introduction of my pessary was complete. The patient has now worn an instrument of this kind for some months, with the most perfect relief to her sufferings, and without inconvenience of any kind, either in its introduction or removal.

The annexed two drawings represent the instrument as it is introduced and withdrawn (No. 1), and worn by the patient (No. 2).

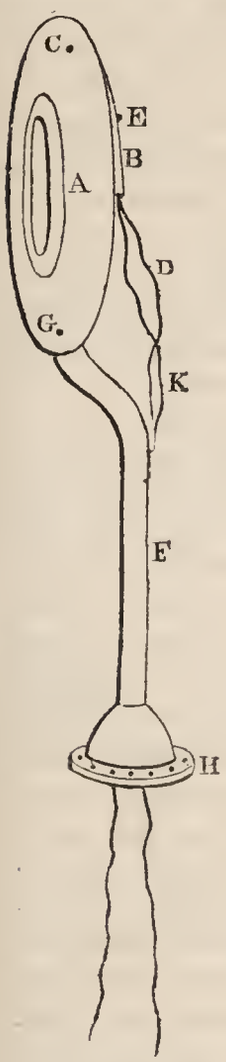

Fig. 1.

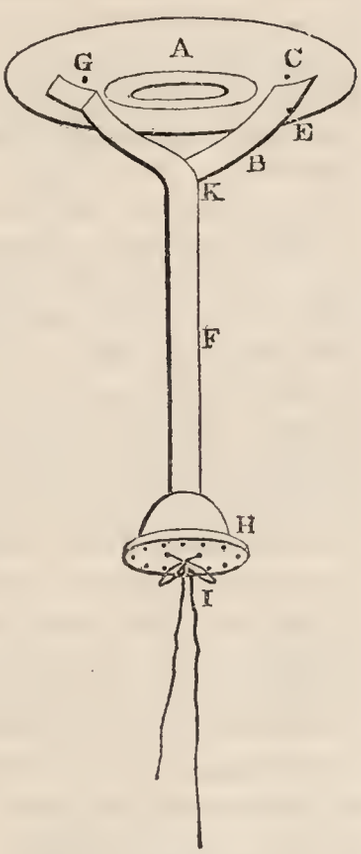

Fig. 2.

a, The crown.

b, Moveable arm (with joint at *c*) having a doubled piece of strong silk (*d*), secured at *e*, passing through it to

f, A hollow stem or leg (with joint at *g*), to receive the silk at *k*, which is seen coming out at two apertures on the under surface of

h, The base, which screws on to *f*, and to which a piece of broad tape is to be sewn; this is to be connected by four narrower pieces to a belt worn round the waist.

After the crown of the instrument is introduced into the vagina in the long axis of the opening, and has reached the inferior chamber of the pelvis, the silk is to be gently drawn down, whilst the stem is at the same time to be carried upwards. This double movement has the instantaneous effect of bringing the arm *b* to the leg *f*, at the point *k*, whereby the instrument is placed in the proper position (No. 2) to support the prolapsed organ. The ends of the silk have only then to be tied firmly together as at *i*.

In order to withdraw the instrument, this knot must be previously cut. The fore finger of the left hand is then to be passed up the vagina to the crown of the instrument, whilst the stem is gently drawn down by the right hand. The crown will, by this movement, be raised into the perpendicular direction, and should it not already be in the long axis of the outlet of the vagina, it can be readily so placed by a slight rotatory motion of the stem. The moveable arm (*b*) is now to be pressed by the index finger towards the crown (*a*), as in No. 1, and the instrument may be then withdrawn with the most perfect ease.

A piece of sponge may be worn round the lower end of the stem and base (*h*), to prevent the instrument injuring the soft parts. The stem can be lengthened at pleasure, by screwing an additional piece to its lower end, before the base (*h*) is affixed. The hinges must be sufficiently easy to admit of the swelling of the wood without impediment to their action.

The instrument may be procured at Messrs. Weiss and Son's, surgical instrument makers, Strand, London.

I have the honour to be, sir,
Your obedient servant,
James Pickford.

Brighton, 21st March, 1838.

SCALDED GLOTTIS—LARYNGOTOMY.

To the Editor of the Medical Gazette.

Sir,

If you consider the following case worth publishing, will you do me the favour to insert it in your very excellent journal?

I think it illustrates well the efficacy of calomel in scalded glottis, as recommended by Dr. Wallace of Dublin, and also the necessity of sometimes assisting it by the operation of laryngotomy, without which it was clear in this case

the calomel would have had no time to produce its effect.

Your obedient servant,

F. E. Hicks.

7, Henrietta Street, Cavendish Square,
March 29th, 1838.

On Thursday, March 1st, 1838, I was sent for by my friend Mr. Richardson, surgeon, of Bayswater, to see with him a little girl aged five years, who a few hours before had taken a tea-kettle of boiling water from off the fire, and drank from the spout a portion of its contents. At first the symptoms did not appear to be very urgent, but at the time I saw her the difficulty of breathing had become so alarming, owing to the inflammation and tumefaction which had taken place about the upper part of the larynx, as to threaten immediate suffocation; indeed, it was clear that unless something were done to relieve the breathing the child could not survive many minutes. I accordingly (assisted by Mr. Richardson) proceeded to perform the operation of laryngotomy, opening into the larynx in the usual situation between the thyroid and cricoid cartilages. The child being exceedingly restless, it was not thought right to introduce any tube into the opening; neither did we find that it was afterwards required, as our patient continued to breathe very freely through it for three or four days, when the air began to find its way through the natural channel, and it healed. For the first week or ten days the child refused to swallow any food, and required to be nourished with injections of broth and milk. Small doses of calomel, however, mixed with sugar, and placed dry in the mouth, appeared to make their way into the stomach, as on being repeated at short intervals for three or four days the system became mercurialized. Under this treatment the sloughs in the mouth rapidly separated, and at the expiration of a little more than three weeks from the accident, both the sores in the mouth, and the wound made by the operation, have healed, and the child is nearly restored to health.

ON MYDRIASIS.

To the Editor of the Medical Gazette.

Sir,

I was recently consulted by a young lady of rather full habit, but enjoying good general health, on account of an affection of the right eye, which appeared to have been considered of an amaurotic nature, and had been treated without advantage during two months, by leeches near the eye, blisters to the temples, and mercury internally. On examination, there were no marks of inflammatory action, but the pupil was considerably more dilated than that of the left eye, and the iris scarcely acted on exposure to strong light; vision was also affected. On making the patient close the sound eye, and look through a pin-hole made in a piece of blackened paper, she could see to read distinctly, proving that the nerves of the iris were alone implicated, and that the retina was sound. She was subject to headaches and bleeding from the nose, and was also latterly often affected with pain of a neuralgic character in the right eyebrow and corresponding part of the forehead. She could not assign any cause for the supervention of the affection, and menstruation was regular. I treated the case on revulsive principles; leeches to the inside of the thighs, aloetic and saline purgatives, which, after a few days, were repeated every third day: 3ss. tinct. valer. ammon. to be taken three times a day on the intervening days: a blister was subsequently applied to the nape of the neck. These measures had produced, at the expiration of a fortnight, a marked amelioration; the sight was more distinct; the pupil less dilated; and the iris acting more freely on exposure to light. It was my intention to have subsequently recommended the carbonate of iron, but the patient having been obliged to go into the country, I was unable to follow the case to its termination, which I have no doubt will be the complete restoration of the impaired function.

I should not have brought forward this case but to notice the circumstance that mydriasis is usually, and with reason, considered to depend upon an affection of the third cerebral nerve; and that, consequently, the muscles of the eyeball supplying this nerve are simultaneously paralyzed. This was not the case in the above instance; the movements of the eyeball were not in the least affected, and from the co-existence of the neuralgic pain of the brow, I have no doubt that the ophthalmic branch of the fifth, which also sup-

plies the iris, was the part principally if not exclusively implicated. Although mydriasis is sometimes sure to follow wounds of the forehead or of the brow, I believe its idiopathic occurrence, combined with frontal neuralgia, and co-existing with integrity of the functions of the motor oculi, is rare: and I should be glad if any of the readers of the GAZETTE could refer me to works where analogous cases are recorded.

EDWIN LEE.

Paris, March, 1838.

MEDICAL GAZETTE.

Saturday, April 7, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

ANOTHER JURY OF MATRONS.

WE are surprised to see that, in a case which has occurred during the present circuit, in which pregnancy was pleaded by the prisoner to obtain remission of sentence, a jury of matrons has again been empanelled to decide the validity of her declaration.

We have never been so fortunate as to hear any rational reasons assigned for retaining such an absurd custom as this; and it would be difficult to imagine what even a lawyer could urge in favour of it. What proofs could he adduce that, as a general rule, any portion of twelve women selected by hazard from the crowd of ignorant idlers that commonly frequent the criminal courts, can be deemed competent judges of the condition of another, or of the value of any evidence presented to them? He would most probably advance, that as each had been, perhaps often, in the same condition herself, she must be able to determine its existence in the prisoner; he might as well propose, according to his own old axiom "cuique in suâ arte (*suo morbo*) credendum," that twelve half-blind men should be called in to decide on the visual powers of a thirteenth, or that all commissions *de lunatico inquirendo* should be held in Bedlam, that a jury of lunatics might be ready to be empannelled.

The proceedings of the matronly scrutiny seem never to be divulged (for here only does matronly garrulity permit secrecy;) but it is certain that the only evidence by which they can judge, must consist of the statements of the prisoner herself, and a general examination of the abdomen. The former are, of course, valueless, and are legally inadmissible, yet they are always received, although (unless in some cases of first pregnancy) the prisoner knowing as much about the general signs as the matrons themselves, may easily deceive them in every particular. The latter, made by an inexperienced hand and eye, are as useless; and they are all, even when deception is not intentionally attempted, and in the hands of able practitioners, signs so uncertain, and so often produced by other totally different conditions, that no reliance whatever can be placed upon them in so important a question as is here concerned.

The only other plea he could advance would be, that of precedent—a mode of argument happily peculiar to his own profession, but here valueless, and even mischievous, because it excludes the admission of evidence as certain as can be given by a medical man on any subject on which he may be called to give an opinion in a court of justice. We would unhesitatingly assert that the examinations by *ballottement* and the stethoscope, and of the breasts and os uteri (no one of which is ever understood or resorted to by the matrons), may afford to any competent judge a *certain* conclusion on the state of a person supposed to be as far advanced in pregnancy as is interested in cases of this kind.

It is notorious that old married women are very frequently mistaken in supposing themselves pregnant without any reason; and the errors they must make in judging of others from what they had learnt from their own cases, must be far more numerous still. The mistake, however, of judging the prisoner pregnant when she is not so, is of far less importance than the opposite one, for it leans towards mercy, and only causes a delay for a short time of the execution of the sentence. But if the life of a fœtus *in utero* be worth preserving, it is fearful to consider how many must have been sacrificed to such ignorance; for the decision of the matrons is absolute, and admits of no appeal without the greatest and most difficult exertions; so that for one case in which error has been detected, ten at least must have gone undiscovered. It is but five years ago that a case of the most flagrant description occurred at Norwich. A woman, who was condemned for murder, pleaded pregnancy in bar of execution; a jury of matrons (some of whom were, we believe, of unusual respectability) was at once empanelled, and, after careful deliberation, they declared her not pregnant. Three of the leading surgeons of that city, however, considered that they had positive evidence of the contrary, and very properly made an urgent representation to the judge, who obtained a remission of the sentence, and four months after, the woman was delivered of a child *a terme*. We took that opportunity of calling attention to the absurd condition of the law on this subject, and of urging that it should be altered. We hoped that so narrow an escape from what every one would have deemed an act of the grossest inhumanity would have been sufficient to show the propriety of an immediate improvement; but this law, and all the others relating to the same subject—every one of which is full of inconsistencies—remain unaltered. In the midst of the mania for legislation which has raged ever since, they have been preserved, and only for the very same reason for which many others far less absurd have been annulled—because they were old.

There seems, indeed, to be a remarkable antipathy among lawyers against admitting any innovation by the application of improvements attained in the knowledge of other sciences—a love of adhering to old and effete doctrines, in which absurdities have been for centuries clearly demonstrated. Among the subjects of the same class as that we are now considering, we need, in proof of this, only to allude to the law on which abortion is punished. Both here and in the case before us, the child is still not esteemed to be alive till it has been supposed to move in the womb, or till it is what they call *quick;* and, therefore, while the procuring of abortion after the fifth month is punishable, under the Ellenborough Act, with death, the same, during the earlier periods of pregnancy, can be visited at most with transportation for fourteen years, and may be punished only by a fine. If, on the contrary, the question relates to the inheritance of property, the same fœtus may, in its first month or first week of life, be heir to estates of any value, which, when four or five months' old, would be hanged with its mother for her crimes, with as little remorse as any excrescence she might have on her body, or might be murdered without the perpetrator being punishable with death. Such inconsistencies as these, based as they are on a doctrine proved centuries ago to be most foolish, can surely be retained only for the sake of the profitable confusion they must give rise to.

They are not greater, however, than some others which occasionally come

under our notice in questions of legal medicine. Take, for instance, the laws on injuries to the person, as administered in some cases that occurred under Lord Ellenborough's Act, which was deemed a remarkable improvement on those that had previously existed. These inconsistencies were only partially remedied in 1828; for under Peel's Acts, others, almost equally remarkable, have occurred. It is necessary now, in order that a man should be punished for more than a common assault, that he shall at least have divided the skin of him whom he has attacked. "If," said Lord Lyndhurst, when Chief Baron, "the skin is broken, and there is bleeding, that is a wound. The definition of a wound, in criminal cases, is an injury to the person, by which the skin is broken." Hence one man may break any bone in another's body, may fracture his ribs so that they may wound his lungs, and he may nearly die of hæmorrhage, or may beat in his skull, and all but kill him of the injury to the brain; yet, if the skin be not broken, he is not indictable for felony, under the present acts.

We should not notice legal errors, which are rather out of our usual beat, if lawyers were not perpetually pestering medical practitioners with the bad figure they cut in the witness box, the only reason of which is, the inconsistencies that abound in their own laws, and the impossibility of making medical science, or sometimes even common sense, to harmonize with them. Thus in cases of insanity, in which, more than in any other, they say that we fail in courts of law. The only reason is, that they constantly ask for a definition which they do not intend to receive; and it is evident that a definition of it, such as would be available in the administration of law, cannot include all the cases which should be classed together in medicine. Many of the former have been given, that would, if adopted, fulfil all the necessary requirements; but a *medical* definition of insanity can no more be fairly made, than one of fever, or of life or death, or of numberless other things of which the common meanings are quite well enough known, can. Insanity, medically, must be defined with reference to the *general* characters of difference which the mind labouring under it presents from those of the healthy condition; but in law, the definition must be based on the *particular actions* to which the morbid condition gives rise. The one must consider, if possible, the cause of all degrees of mental aberration; the other is concerned only with some of the effects of that cause in some cases, and these must be determined quite arbitrarily, for the benefit of society. Horace's

> ——— "Haud ignobilis Argis
> Qui se credebat miros audire tragœdos,"

was as mad, medically speaking, as he who murdered children to people heaven, or who gave away all his property to build churches; yet, if the former committed no crime, and squandered no money, the law would leave him at peace, with his plays still ringing in his ears. In medicine, insanity must be considered to exist where the aberration is concerned in the simplest trifle, as where it leads to a heinous crime; but in law, so sweeping a rule would be both ridiculous and mischievous.

We have rather wandered from our original subject, to shew that the laws, on points where our professions meet, are not by any means perfect, and thence to defend ourselves against the charge which is so commonly brought against our profession, that the confusion and misunderstandings that constantly arise in medico-legal inquiries is their fault. Such a charge is absurd, so long as such foolish inconsistencies

as those we have pointed out exist: so so long as lawyers will adhere to errors simply beeause they have been committed before, there will of necessity be confusion when they come in contact with those who have less respect for antiquated mistakes. For the *precedented* absurdity of the jury of matrons, though it has survived the ravages of ages both of bigoted devotion and of destructive reformation, we entertain so little regard, that we shall lose no opportunity of pointing out its folly on every fitting oecasion, till the plan is constantly resorted to, if not espeeially ordained, of requiring, in all eriminal eases, as has already been done in some eivil ones, the opinions of two or three competent medieal witnesses.

INTEMPERANCE OF MEDICAL CONTROVERSIALISTS.

Dr. Marshall Hall has appealed from the tribunal of the profession at large to that of his own elass; and in his lecture published last week has made this journal the theme of severe animadversion. After referring to the conduet of those whom he represents as throwing discredit on his diseoveries, he adds, "espeeially with what ineffable eontempt must you look upon the reeent coarse and malignant remarks in the Medical Gazette—remarks whieh are disgraeeful, by their ungentlemanly style, both to our literature and to our profession." Now we challenge Dr. Marshall Hall to adduee a singe eoarse or malignant phrase from any of our articles respeeting him; and we refer to the subject, not because the dispute between Dr. M. Hall and ourselves is of any moment, but because, as it now stands, it becomes of importance in medical ethies. Dr. Grant and Dr. M. Hall simultaneously published an attack upon a talented and rising young man, couched in terms of the bitterest and coarsest invective, and bringing before the public private transactions in a manner the most ungenerous and indelicate. Mr. Newport was no correspondent of ours, and we knew nothing of him but that a eombined attempt was being made by the parties above mentioned to crush him. We therefore stepped forward to give him our humble support; and though, in doing this, we expressed strong indignation, and some contempt, yet we deny that we were betrayed into any thing "ungentlemanly,"—except in so far as we quoted the words of Dr. Grant and Dr. Hall. We regret to feel compelled to add, that it is not the first time both these individuals have detraeted from the dignity of seience, by the use of language not tolerated among gentlemen even in the heat of eontroversy. It is of great importance that sueh displays should be met by unequivocal marks of publie eensure; and as we may now be supposed to be in some measure personally involved, and therefore not impartial, instead of adding any farther remarks of our own, we shall quote the sentiments of the editors of the *British and Foreign Quarterly Review*, as contained in their number just published:—

"In the position in which we are placed (say they), as one of the public organs of the profession in this eountry, we hold it to be our duty, in vindieation of the honour of that profession, to deelare that its members, as a body, hold no sympathy with the low and evil passions disclosed in the communications of Dr. Grant and Dr. Hall, and repudiate the employment, in scientifie warfare, of the weapons of mere obloquy and personal invective so fiercely and lavishly wielded by them against Mr. Newport. Had every one of the charges of delinqueney brought against this gentleman been true, instead of being, as he maintains, utterly groundless, and the offspring of misapprehension or malevolenee, although we might, in this case, have made allowance for much of

the severity of the language used by Dr. Grant and Dr. Hall, it would have been impossible to justify or excuse, or even to regard without disgust, the detail of private circumstances intruded into the controversy by the last-named physician. Often before now has it happened,—and often, we trust, it will again happen, — that men of science have mutually given and received assistance, both personal and pecuniary; but we hardly think that it ever before occurred, among men in the upper ranks of society, that such pitiful and paltry obligations as those conferred on Mr. Newport by Dr. Hall, were trumpeted forth to the world by the individual himself, and with all the pomp and circumstances of offended patronage. We will not disfigure our pages by enumerating them. We think the circumstances disclosed in no degree humiliating to Mr. Newport; but we are greatly mistaken if Dr. Hall will not ere long make the discovery—a discovery which all that he has already made or imagined will not lightly counterpoise—that, on the present occasion, he has allowed himself to be betrayed by his passions into a forgetfulness of his own dignity and self-respect, and that he has sustained, in consequence, a proportionate loss in the estimation of his friends."

THE REFLEX FUNCTION AN OLD DISCOVERY.

We beg to direct the attention of our readers to the elaborate and interesting paper by Mr. George, in the preceding part of the present number. From this, it is clear that Dr. Marshall Hall was completely anticipated by Prochaska in his views of the reflex function; and the same opinion, we perceive, is expressed by the learned Editors of the British and Foreign Medical Review. It is a very remarkable fact, as stated by Mr. George, that Dr. Hall, though he has published the "opinions of former physiologists," makes no mention of the author above named; and certainly this fact will not be regarded as less remarkable, when we inform our readers that there is a copy of Prochaska's work in the library of the Medical and Chirurgical Society, and that, little as the volume was known to others, one member, at least, has shewn that he duly appreciated its value, by the fact of his having repeatedly taken it out: that member, need we add, is—Dr. Marshall Hall!

POISONING BY HOMŒOPATHY.

In the Brussels papers of late, there has been an admirable case of supposed poisoning by an homœopathic physician. It is that of a young lady who had measles, but in whom, after retrocession of the eruption, symptoms of cerebral congestion and effusion ensued, and terminated fatally, in a few hours after two regular physicians were called in to share the responsibility. The plaintiff was public fame. The defendant declares in his statement, that the treatment on the four days preceding the fatal attack was as follows:—

On the 6th, one thirty-third part of a decillionth of a grain of hellebore.

On the 7th, one thirty-third part of a quadrillionth of a grain of bryony.

On the 8th, one hundredth part of a decillionth of a grain of anemone.

On the 9th, one hundredth part of a decillionth of a grain of belladonna.

On the 10th the patient died—of what? of those enormously small doses of medicine? Undoubtedly, if the homœopathic doctrine be true, that the effect of a dose is inversely as its quantity. But still it is in this case a miracle unexplained, how we, who drink Thames water, can be preserved from poisoning by that common reservoir for the washings of all druggists' shops; or how we can survive an hour in the exhalations of a dispensary.

The Doctor says, and not too boldly, that he would take the same doses a hundred times over. However, he scarcely deserves such a punishment, for we should certainly acquit him of active murder, though he might yet be accused of what is always esteemed as high a crime in our profession — letting the patient die. *En passant*, the homœopathist was a magnetizer, too. We shall be surprised if any of the supporters of one are enemies to the other absurdity.

CLINICAL LECTURE

ON

FIBROUS CYSTS IN THE HAM.

Delivered at St. Bartholomew's Hospital,

By Wm. Lawrence, Esq. F.R.S. &c.

Fibrous Cysts in the Ham, containing thick viscid fluid.

There is in the hospital at present, a patient with a tumor in the ham, in the situation of popliteal aneurism. Although the disease in this case is not of very rare occurrence, and liable, by its situation, to be confounded with other affections, I cannot refer to any surgical work for an account of its characters, nature, and treatment. It is not mentioned in the recent valuable publication of Dr. Warren, on Tumors, nor in the two most modern French medical dictionaries.

George Brock, 28 years of age, a person of middle stature, and rather muscular frame, has usually enjoyed good health. He has had some pain in the left side and ankle since February: for the last seven weeks he has felt stiffness in the left knee, with a swelling in the ham, impeding the action of the joint, and causing a little lameness. He was received into the hospital on July 15th, 1837. An indolent tumor is seated in the ham, where it causes a slight projection, a little nearer to the inner than the outer ham-string, being imbedded between these, like a popliteal aneurism, which it exactly resembles in situation and appearance. When the knee is extended, and the muscles of the thigh are put in action, the swelling is tense and firm, and projects obviously in the popliteal space; it appears equal in bulk to a middle-sized orange. When the knee is bent it becomes softer, and it cannot be felt at all in extreme flexion of the joint; hence it must contain fluid, although it has a firm and solid feel when the limb is extended. Even in the latter state it presents no symptoms belonging to aneurism, nor does it exhibit any communicated pulsation. The tumor is unattended with pain, and causes no other inconvenience than a little uneasiness about the knee, and slight lameness; these being so inconsiderable, that the patient, when I first saw him, did not intend to come into the hospital. I considered the case to be of the nature of a ganglion, and probably bearing the same relation to some bursa connected with the inner ham-string, that ganglions of the wrist and feet do to the fibrous sheaths in these situations. A dozen leeches were applied; the surface was subsequently blistered, and then dressed with the savine cerate. No change was observed from these means. At the end of a fortnight the synovial membrane of the knee joint became inflamed, and there was effusion into the joint. Cupping, leeching, and blistering, were necessary to remove this attack; the joint and the swelling were subsequently covered with the emplastrum ammoniaco c. hydrargyro. As yet, there is no material diminution of the swelling.

This patient continued in the hospital for several weeks. He became affected with severe pain in the limbs shooting down the left lower extremity, the muscles of which were sometimes spasmodically contracted, so that he could hardly move the thigh or knee, and he was consequently confined to bed. The synovial membrane of the knee inflamed again. He was cupped three times in the loins and once on the knee, and was blistered more than once in both situations; on different occasions he used the warm bath, and took the acetous extract of colchicum in doses of three grains, at bed time. He left the hospital in the beginning of November, completely free from the pain in the loins and the spasmodic affection of the thigh. The knee had recovered; the popliteal swelling was less, but not completely removed. He has been at the hospital since, expressing to the sister of the ward that he felt quite well and free from lameness; but the ham was not examined.

In another patient, whom I have seen lately, there was a swelling in the ham exactly similar to the preceding in size, position, and other circumstances. He is between 50 and 60 years of age, and employed as a private watchman; so that he is much on his legs, and exposed to the atmosphere in all weathers. He has suffered occasionally from rheumatism. The swelling in this case caused considerable lameness. I prescribed medicine, and directed that the swelling should be covered with the emplastr. ammon. c. hydrarg. In ten days the swelling had disappeared, although some weakness and stiffness of the joint remained.

I had an opportunity, a few weeks ago, of ascertaining more precisely the nature of these affections. I was consulted by a patient, nearly 50 years of age, on account of a softish indolent swelling in the ham, which had existed for some time, and caused little inconvenience. It was immediately under the skin, and movable in all directions. I did not observe that its state was altered by any motions of the knee. In its indolence, its softish doughy feel, its subcutaneous situation, and complete mobility, it presented the characters of an adipose tumor, and I considered it

to be of that nature. There was no lameness, but the patient stated that she had sometimes experienced a little weakness in both knees. I represented that the swelling was probably a mass of fat; that if it did not increase, and caused no inconvenience, there was no necessity for doing any thing; under other circumstances it might be properly and safely removed. The patient said that it was increasing, that it had caused some pains in the knee, and that she wished to have it removed, if the operation could be performed safely. On dividing the integuments, I immediately perceived that the swelling was a cyst, containing fluid: it was loosely connected to the surrounding parts, and thus easily detached. The mass of the swelling was immediately under the skin, but it had a deeper continuation in the direction of the inner ham-string. Having traced this as far as appeared to me advisable, I cut it off, when the contents of the cyst flowed out in the shape of a viscid fluid, so thick that it could not be taken up with a sponge, of a light dull yellow tint, and nearly transparent. It was thicker than white of egg, and very similar to what is contained in ranulæ. The cyst was fibrous, thin, and semitransparent; it was slightly sacculated internally, so as to give an irregular edge to the swelling, like the lobulated margin of a fatty tumor. As far as could be ascertained by the finger, the cyst terminated by a blind extremity, near where it had been cut through. The quantity of the fluid was about two or three table-spoonfuls. No blood was lost in the operation, which was quickly finished, and gave much less pain than the patient had expected. The edges of the wound were brought together by adhesive plasters, and rest in bed was enjoined. When I saw the patient the next day, I was shocked at seeing her with a flushed and oppressed countenance, and a drowsy look, like that of a person in typhus. I found that in an hour or two after the operation, the wound and the knee had become painful; that bleeding had began soon after, and proceeded to such an extent, that a neighbouring practitioner had been sent for, who had opened the wound and applied cold, under which the hæmorrhage had ceased. He supposed that about twenty ounces of blood had been lost. The severity of the pain had induced him to administer an opiate, from which some relief was obtained. The pulse was rapid and rather feeble; the tongue dry, with a brown middle streak; and there was that kind of restlessness, without apparent cause, which is always a most unfavourable symptom. The wound and the knee were easy; but there seemed to me a little general tumefaction of the latter. The bowels had not been relieved since the day before the operation. I ordered aperient medicine, with light cordials and nourishment, and bread poultice to the part. The bowels were freely acted on by the purgatives, but no material change was produced in the symptoms. The pulse increased in rapidity and feebleness, deliriam and incoherent talking came on, and the patient sunk in about sixty hours from the operation, having made no complaint of the wound or knee after the first night, but on the contrary expressed that they were quite easy. I cannot ascribe the unfortunate event of this case to any thing connected with the peculiar affection. It rather seems to exemplify the fatality sometimes attending even slight operations, when performed on individuals of unsound and irritable constitution. This patient had suffered repeatedly from serious illness, and was obliged to live most carefully and quietly. She had lost a brother from constitutional irritation, in some respects analogous to that which she herself experienced, excited by a slight accidental injury. One reason why she wished for the operation, which I did not recommend, was, that she felt herself at the time in remarkably good health.

About three weeks ago, a gentleman thirty years of age consulted me for a swelling on the inner side of the right knee, a little below the joint. It was nearly as large as a walnut, and had all the characters of a ganglion. I burst the cyst by means of a sharp blow with a stick, and its contents were diffused in the surrounding cellular substance. The cyst did not fill again. This must have been a ganglion connected with the bursal apparatus of the flexor tendons, where they play on the tibia.

[Since the occurrence of the foregoing cases, Mr. Lawrence had a man between thirty and forty, following the occupation of a smith, under his care at the hospital as an out-patient. He had a swelling in the ham, precisely similar to that of George Brock, causing a little stiffness and lameness, but no other inconvenience. Mr. Lawrence attempted to burst the cyst by striking it, but could not accomplish the object, as the tumor, being surrounded by soft parts, yields, and thus eludes the effect of the blow. He ordered the emplastr. ammon. c. hydrarg., and has not seen the patient lately.

One of these tumors was met with in a male subject brought into the anatomical rooms for dissection since the beginning of the season. It was a thin, slightly sacculated, fibrous cyst, containing the viscid fluid already described. The swelling lay in the ham, near the inner hamstring, between the muscles of which it was con-

tinued. It was firm when the knee was extended, and became looser on bending the joint. It terminated in a blind extremity between the muscles of the hamstring, and could not be verified as an enlargement of any normal bursa.]

The nature of these swellings in the ham is shewn clearly by the facts now related, which prove also that the affection is not uncommon. I have lately seen another instance of it in a gentleman of gouty habit, between 50 and 60, who had inflammation of the synovial membrane of the knee from an accident. In examining the joint as he was recovering from this attack, I found a swelling in the ham, but I did not mention it to the patient, as he was not aware of its existence, and as the stiffness remaining from the inflammation confined him to moderate use of the part. The free use of the knee was soon restored, and I have heard no complaint of swelling or stiffness since.

In the composition and contents of the cyst, these tumors correspond to the ganglions so frequent on the back of the hand and wrist, and occasionally observed on the back of the foot; they are quite different from the swellings formed over the patella and olecranon, in consequence of external injury or irritation; these being inflammations of normal bursæ, and containing either a thin fluid of almost watery consistence, or a purulent fluid, according to the degree of the local disturbance. If they should cause inconvenience by their size, and if they cannot be lessened or removed by stimulating plasters, liniments, friction, or blisters, I think there could be no danger in letting out their contents by puncture, and closing the opening—a proceeding which is employed with safety in ganglions. The fatal case is well calculated to inculcate a lesson of caution in operating on such affections, although its unfortunate termination was probably owing to peculiarity of constitution rather than of disease. I must, however, observe to you, that serious consequences have ensued in other instances from operations performed on ganglions, and as these affections are in themselves quite free from danger, and produce at most an inconvenience, with perhaps a slight deformity, no dangerous method of treatment should be adopted. Free incision and exposure of the cyst was resorted to in two cases of ganglion: one was connected with the tendons of the peronei, near the external malleolus; the other with the extensor tendon of the fingers on the back of the hand. Inflammation was excited in both cases; several abscesses formed; and both the patients, who were military men, were obliged to leave the service*. A seton carried through one of the ganglia sometimes formed about the flexor tendons of the fingers, where they pass under the annular ligament of the wrist, caused violent inflammation, suppuration, and death on the fifteenth day†. Dr. Warren mentions that he punctured one of these tumors, and discharged the synovial contents: the opening healed, and the patient was relieved. He repeated the proceeding in another case, and let out synovial fluid with several cartilaginous bodies. Fearing that there might still be some of the latter in the ganglion, he introduced a bit of dressing to keep open the wound. Inflammation and suppuration followed, and the patient was near losing the limb‡.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

March 27, 1838.

The President in the Chair.

On the Results of Poisoning by Sulphuric Acid. By John Wilson, M.D., Physician to the Middlesex Hospital.

The object of the author in this paper was to describe the condition of the œsophagus in a patient who died forty-five weeks after having swallowed a considerable quantity of sulphuric acid. The history of this case had been previously detailed in a paper read at the College of Physicians, in July 1834, the patient being then alive, at which time a cylindrical tube, eight or nine inches in length, which had been ejected by a violent fit of coughing, was placed on the table. At that time the patient had survived the injury six months, and was soon afterwards discharged from the hospital in a great measure recovered. She was admitted, however, in the following September, much reduced, and after very great suffering died on the 17th November.

On examination, *post mortem*, the lower two-thirds of the œsophagus were thickened and narrowed, and very vascular and softened internally; the upper third of the tube shone like an old cicatrix. In the stomach, opposite to the spleen, was an opening to the size of a half crown, with softened edges; and the abdomen contained a quantity of dark-coloured

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* Dict. de Méd. et Chir. Pratiques, t. viii. p. 628.

† Dict. de Médecine, en 21 tomes, t. xii. p. 536.

‡ Surgical Observations on Tumors; p. 507.

fluid, but no signs of inflammation. Preparations of the ejected tubular membrane, and of the œsophagus, were exhibited to the Society. A coloured model was also shewn to the meeting of the stomach of another patient, who had died twenty-two hours after having swallowed two or three ounces of sulphuric acid, which had remained in the stomach a quarter of an hour. The lining of the mouth, pharynx, and œsophagus, in the latter case, was of a silvery grey speckled appearance, like a snake's skin, and the lining membrane of the stomach was coated with a black pitch-like substance, which did not easily wash off, but which, when scraped off, left the membrane entire.

On the Use of Arsenic in some Disorders of the Uterus. By HENRY HUNT, Esq. of Dartmouth. Communicated by Mr. PERRY.

The author was first led to observe the effects of arsenic on the uterus in a case of carcinoma of that organ, in which it relieved the suffering of the patient in a remarkable degree, and in exact proportion to the increase of its poisonous effects upon the system. This fact, added to the circumstance of the genitals being occasionally observed to be inflamed where arsenic has been taken as a poison, induced the author to hope that it might be found to be serviceable in some disorders of those parts, an opinion in which he was encouraged by the experience of Dr. Locock, in a case in which that mineral had been administered by him for the cure of a disorder of the nose, he not being aware at the time that the patient was also affected with menorrhagia. The author relates six cases of menorrhagia, in which the patients had taken arsenic with very marked benefit. In these instances there was no organic disease of the uterus; but the excessive flow of the menses appeared to be the consequence of exhaustion, originating, however, in the several individuals from different causes. The success attending the use of this medicine in menorrhagia induced the author to employ it in some other disorders of the uterus; and some favourable cases of its agency are annexed. Some of the most favourable of these are a case of irritable uterus, and one of neuralgia, occurring regularly about the menstrual period, both of which were entirely relieved. The paper concludes with some observations on the effects of arsenic on the genital organs, when given in poisonous doses, from which the author infers that its agency depends on its action on the mucous membrane as a stimulant.

An Account of a Case in which the entire Lower Jaw was removed, on account of Disease: with Observations. By JOHN G. PERRY, Esq. Surgeon to the Foundling Hospital, and one of the Secretaries to the Society.

The case here related occurred in the St. Marylebone Infirmary, during the last year, in a young woman of a scrofulous habit, aged 20 years. According to the history given by the patient of the progress of the disease, as well as from the anomalous position of some of the teeth, it appeared probable that it had been of very long duration, and that the commencement of diseased action was at least coeval with the period of the permanent dentition. From the appearance of the bone (which was exhibited to the Society) there could be, as the author conceived, no doubt that the disease by which it had been destroyed was necrosis, or that it had, up to a certain period, undergone the natural process of cure, since the entire bone was inclosed, even to the condyles, alveoli, and coronoid processes, with a case of new osseous matter. This reparative process had been, however, interrupted by the separation of the periosteum from the new deposit, the consequence of extensive disease of that membrane, and suppuration within it. Thus the new bone also perished, and the process of reparation became arrested. The author removed the bone without difficulty, by dividing it into three portions, and the wound healed kindly. Care was taken to leave as many as possible of the teeth suspended in the gum, in the hope that a sufficient degree of solidity would be regained in the process of healing to render them serviceable in mastication. The patient is now, nine months after the operation, quite well, with the exception of an inconsiderable sinus, which discharges in a trifling degree, and has the power of masticating solid food with surprising facility, when it is considered that the process is chiefly performed between the tongue and the upper teeth. There is no reproduction of bone, but the patient can bite with considerable firmness, and the countenance is less altered than might be expected after the removal of so large a portion of its skeleton.

UNREASONABLE REQUEST
FROM THE HOME OFFICE.

To the Editor of the Medical Gazette.

SIR,

A FEW days since I received a letter from the Secretary of State's Office, requiring

from me, in reference to the Borough Gaol and House of Correction of this town, a return for the year 1837, shewing "the name of the prison; the county or district in which the prison is situated; the average number of prisoners; the total number of prisoners admitted in 1837; the total number of cases of sickness occurring in 1837; the average number of persons on the sick list in 1837; the number of deaths in 1837; the amount of the surgeon's salary; the amount of extra charges connected with the medical treatment in 1837; on what terms the medicines are supplied; the prime cost of the medicines to the surgeon; the name of the surgeon; and explanatory remarks."

Of these twelve items of inquiry, the first and second relate merely to the locality of the prison; the third and fourth entirely to the duties of the gaoler; and the fifth, sixth, seventh, and last, are repetitions from a return made a short time ago. Striking out these as superfluous, we have the eighth, ninth, and tenth, which are the only questions relating immediately to the subject under investigation, and the eleventh, which is certainly a curiosity, as far as I know unique, and a proof that the rulers of the nation are resolved to keep pace with the most diligent inquirers after truth. The connexion between the state of prisons and the state of the drug market I do not understand; nor do I see what the prime cost of each individual pill and potion has to do with the question of prison discipline. Whatever may be the ulterior object of the inquiry, the attempt to extract, with an air of assumed authority, any such information from the medical officers of gaols, is a most inquisitorial as well as a most absurd proceeding, indicating a degree of prying curiosity equalled only by the ignorance of the party conducting the investigation, and meriting from every one the contempt which such conduct in private life would not fail to excite. It would be well if public functionaries, requiring these and similar returns, would bear in mind the value of time to medical practitioners, and ask only such information as is intended to answer some useful purpose.

Your obedient servant,

EDWARD BARBER.

Stamford, March 17, 1838.

THE NEW PHARMACOPŒIA.

To the Editor of the Medical Gazette.

SIR,

ALLOW me a corner of your work—in which classical knowledge never fails to meet with the attention it is entitled to expect from a learned profession—in order to point out two or three errors into which the translators of the new Pharmacopœia have fallen. The first passage occurs at page 33 of the smaller edition;—"medicamentorum, quam desideramus, sinceritas." Dr. Collier, though so excellent a classical scholar, has gone wrong here; probably from the haste with which his translation was executed. He has rendered it, "the purity of the medicines we require;" making the *quam* relate to *medicamentorum*, instead of to *sinceritas*. Dr. Castle translates it, "the purity of the medicines we order;" Mr. Phillips,—"the purity of the medicines required;" and Mr. Futvoye,—"the purity of mixtures which we desire." Mr. Haselden has it,—"*when* we desire the genuineness of medicinal substances" &c., which would be correct if it were *quum* instead of *quam*. Dr. Spillan seems to have hit the true sense; for he translates it,—"the purity of the several medicines, about which we are so anxious." The comma after "*medicines*," seems to indicate that "*which*" relates to "*purity*." It is certainly the "*purity*" of the medicines that the College express their anxiety about.

Another passage, in which their meaning seems to have been mistaken, is that in which the compilers give their reasons for adopting an alphabetical arrangement:—"scial nos medentium commodis, non placitis philosophorum, neque otiosorum delectationi, servire."—(Page 33, smaller edition.) The word *otiosorum* here has been translated "*indolent*" (Collier and Spillan); "*idle*" (Futvoye); "*unconcerned*" (Haselden); and "*theo ists*" (Phillips.) All these imply a censure, which I am persuaded the College did not intend; for it would not be easy to see how a *scientific* arrangement would favour indolence, more than an *alphabetical* one; rather the reverse. They evidently intend to designate those who have more time than physicians in the hurry of practice, to attend to scientific arrangements. Dr. Castle has come nearer to the true meaning, in rendering it "*amateurs*."

The phrase,—"medicamentorum que opifices doctique, vel indocti," in the last paragraph of the preface, Mr. Phillips (followed by Dr. Castle and Mr. Haselden) has translated—"the compounders of medicine, whether learned or unlearned;" overlooking the *copulative* conjunctive "*que*," after "*docti*"; though the translation is perhaps an improvement on the original. Drs. Collier and Spillan, as well as Mr. Futvoye, have given the correct rendering.

Your obedient servant,

N. ROGERS, M.D.

March 1838.

ACCIDENTS ADMITTED AT THE LONDON HOSPITAL.

Surgeon, Mr. Scott.
Assistant-Surgeon, Mr. Hamilton.

Mar.	Sex.	Age.	Case.
13.	M.	41	Injured testicle.
	M.	36	Fractured tibia.
	M.	28	Cut head.
	M.	15	Bite of a dog.
	F.	26	Fractured ribs.
	F.	20	Fractured ribs.
14.	M.	45	Fractured metatarsal bones.
	M.	43	Fractured cranium; ruptured spinous artery; death on the fifth day.
	M.	12	Fractured fibula.
	M.	50	Severe laceration of thigh and foot.
	M.	60	Contused back and hips.
	F.	44	Fractured tibia and fibula.
15.	M.	35	Lacerated scalp and fractured clavicle.
	M.	12	Bite of a dog.
	M.	31	Injured foot.
	F.	55	Fractured humerus.
16.	M.	14	Injured chest.
	M.	18	Fractured ribs.
	M.	43	Fractured patella.
	F.	47	Sprained ankle.
17.	M.	30	Fractured fibula.
	M.	20	Injured foot.
	M.	28	Retention of urine from stricture.
	M.	10	Fractured femur.
	M.	5	Burn.
18.	F.	50	Cut head.
	M.	50	Cut face.
	M.	38	Fractured fibula.
	M.	89	Hernia, (reduced.)
	M.	56	Lacerated face.
19.	M.	25	Wounded thigh.
	M.	24	Lacerated leg.
	M.	52	Fractured ribs.
	M.	33	Fractured lower jaw, attended with very considerable hæmorrhage.
	M.	48	Fractured tibia and fibula.
	F.	46	Scald.
	M.	32	Punctured arm.

In-patients 37
Out-patients 48

Total 85

Surgeon of the Week, Mr. Luke.
Assistant-Surgeon, Mr. Curling.

March.	Sex.	Age.	Case.
20.	M.	56	Severe scald on the leg.
	M.	45	Contused back.
21.	M.	36	Extensive burn on the leg.
	M.		Contused foot.
	M.	47	Fractured femur.
22.	F.	70	Cut throat.
	M.	50	Fractured ribs.
23.	M.	17	Compound fracture of the thigh and lower jaw.
	M.	15	Compound fracture of the tibia.
24.	M.	43	Contused legs.
	M.	8	Fracture of the tibia.
	M.	67	Fracture of the ribs.
	M.	65	Fracture of the ribs (*a*.)
	M.	23	Fracture of the ribs.
26.	M.	31	Retention of urine.
	M.	26	Lacerated hand.
	M.	41	Contused leg.

In patients 17
Out-patients 40

Total 57

(*a*.) This patient is a Polish Jew, who had come over to this country to see his sons, who are settled in business in London. He was about to return when he met with his accident. The case has excited some interest, in consequence of his head affording a good specimen of the plica polonica. A long lock of hair, closely and intricately matted, and loaded with dark coloured pediculi, extends from each side of the head. The scalp and roots of the hair are sound.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO HAVE RECEIVED DIPLOMAS.

March, 1838.

W. Rundle, White Street, Southwark.—F. C. Beard. – J. Shillito, York. — W. Deans, Pontefract.—J. A. Edwards, Stratten, Cornwall.—W. H. Wood, Ash Hall, Glamorganshire.—H. F. Jones, Malmesbury.—A. M. A' Beckett, Golden Square.—S. Cartwright, Old Burlington Street. — J. B. Stevens, Madras. — Flaxman Spurrell, Southwark.—C. F. Waters, Madras.—G. Crondace, Chester-le-Street.—W. S. Wigg, Great Yarmouth. — R. Q. Couch Polperro, Cornwall.—G. Jones, Birmingham.—T. Browne, Alford.—G. Lloyd, Limerick.—G. Wilson, Leeds.—F. H. Fenn, Neyland, Suffolk.—E. L. Wollaston, London.—W. Griffith, Llanrwst.—G. Butlin, Daventry.—Owen Kiernan, Cavan.—J. W. Davis. — J. Hawkins, Carmarthenshire. — N. Stowers, London.—J. H. Nankivell, St. Columb, Cornwall.—W. B. Kesteven, London.—A. Young, Harpur Street, Red Lion Square.—J. P. T. Williams, London.—W. Crooke, co. Cork.—R. B. Kinsey, E. I.—C. Heaton.—H. Stanbrough, Isleworth.—W. Wood, London.—J. Helmer, Ronaldkirk, Yorkshire.—J. C. Smart, Hutton Bushell.—T. W. Glanvill, Edgeware Road.

OBITUARY.

Several medical men have recently fallen victims to the fever which has now for some time been prevalent in London. The character of the epidemic seems very analogous to that which it has presented in Ireland and Scotland, in both of which countries a considerable number of our professional brethren have died of it.

On the present occasion we have to announce the premature decease of Dr. Fergus, Professor of Medical Jurisprudence in King's College, which took place on the 3rd instant. This gentleman was only in his 28th year, and is deeply regretted by those who knew him. He had lectured during but one season, but had, nevertheless, earned for himself a high reputation among his colleagues and pupils.

Another young physician, who died of the same disease last week, is Dr. C. J. Johnstone, Physician to the Foundling, Fellow of Caius College, Cambridge; in whom also the profession has lost a promising and respected member.

In Edinburgh, Dr. John Home, son of the Professor of Physic, has been added to the list of those to whom this fever has proved fatal: he is spoken of as a young man of great promise, and is said to have caught the disease in the discharge of his duties at the Fever Hospital, to which he was physician.

IRISH COLLEGE OF SURGEONS AND APOTHECARIES' HALL.

We have received the communication signed M.D., T.C.D., but we must decline to insert it, or any other on the same subject, except in the *extra limites* department, as we do not feel justified in placing papers so purely controversial in the body of the journal. The necessity of adhering to this principle has compelled us to deal in a similar manner even with the short note of Dr. Maunsell which follows.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, April 5, 1838.

William Daniell.—John Wm. Joy.—Charles Frederic Edwards.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Apr. 3, 1838.*

Abscess	6	Heart, diseased	3
Age and Debility	49	Hooping Cough	11
Apoplexy	12	Inflammation	19
Asthma	14	Bowels & Stomach	6
Childbirth	7	Brain	7
Consumption	47	Lungs and Pleura	5
Convulsions	32	Insanity	8
Croup	1	Liver, diseased	1
Dentition or Teething	6	Measles	5
Dropsy	13	Paralysis	3
Dropsy in the Brain	6	Small-pox	9
Dropsy in the Chest	1	Sore Throat and Quinsey	2
Epilepsy	1	Venereal	1
Fever	19	Unknown Causes	63
Fever, Scarlet	1		
Fever, Typhus	5	Casualties	6
Hæmorrhage	1		

Increase of Burials, as compared with the preceding week . . . 164

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

March.	Thermometer.		Barometer.	
Thursday . 15	from 40	to 48	29·87	to 29·87
Friday . . 16	23	50	29·85	29·62
Saturday . 17	30	45	29 45	29·44
Sunday . . 18	30	47	29·55	29 59
Monday . . 19	33	47	29·57	29·38
Tuesday . . 20	38	50	29 23	29 26
Wednesday 21	29	44	29·34	29.40
Thursday . 22	from 26	to 46	29·45	29·50
Friday. . . 23	24	41	29·55	to 29·53
Saturday . 24	30	46	29 50	29·49
Sunday . . 25	29	51	29·78	29·98
Monday . . 26	20	53	30·04	30·10
Tuesday . 27	24	56	30·21	30·31
Wednesday 28	28	57	30·41	30·44

Winds S.W., N.W., and N.E., S.W. prevailing.

Except the 15th, 19th, 22d, and 23d, generally clear, with frequent showers of rain; hail on the 21st, and snow on the 23d. A beautiful Aurora Borealis on the evening of the 17th, extending from N. to N.W., and several meteors fell during the evening.

Rain fallen, ·1375 of an inch.

Charles Henry Adams.

NOTICES.

Communications have been received from Dr. Griffin, Dr. D. Griffin, Dr. Mackenzie, Mr. Mayo, Dr. Fergusson, Dr. Wilson, Dr. Thompson, Mr. Thornhill, Mr. Sandall, Dr. Ward, Mr. Saunders, Mr. Deane, Dr. Burne, Mr. Leach, "A Retired Physician," "K.," two papers.

Wilson & Son, Printers, 57, Skinner-st., London.

(Extra limites.)

IRISH COLLEGE OF SURGEONS.

To the Editor of the Medical Gazette.

SIR,

A LETTER, signed "B. Alcock," having appeared in your number of this date, in which I am alluded to by name, may I beg your permission to state, that having, as I thought, done my duty towards the body to which I have the honour to belong, by taking measures to expose the author of a letter calumniating that body, I did not conceive that I had any further *personal* concern in the matter. It is, I believe, still under the consideration of the proper tribunal, and in that situation I must still leave it. I beg to say, however, that your correspondent, Dr. Alcock, has in his letter, published by you on the 10th February, 1838, and signed, "A Member of the Royal College of Surgeons in Ireland," made use of the following expressions:— "Hitherto I have looked on; because it did not appear to me that the time had arrived when interference was likely to prove seasonable or effectual:" and that he has, since, in his place in the College, acknowledged himself to be the author of a letter *upon the same subject*, signed "A Professor in the School of the Apothecaries' Hall of Ireland," which was previously published in your number of the 20th January, 1838. Under these circumstances it is totally impossible for me to enter into any controversy with a gentleman whose views of the obligations of honourable men with regard to truth differ so remarkably from mine. I shall, therefore, trouble you no more in this matter.

I may, however, add, that my name was never, at any period, the subject of concealment, as you had full liberty to make it public, or to communicate it to any person concerned.

I have the honour to be, sir,
Your most obedient servant,
H. MAUNSELL.

London, March 31st, 1838.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 14, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton-Street, near St George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XVII.

Diseases of the Pleura (continued)—Further Effects of Pleurisy: Solid Effusions; their Effects: Contraction of the Chest; Signs and Effects: Liquid Effusions; Empyema—Relations of Acute and Chronic Pleurisy—Treatment of Pleurisy; Acute; Chronic—Paracentesis Thoracis; Indications and Objects of the Operation; Causes of Failure—Directions for the Operation.

In the last lecture we were occupied with the pathology and signs of acute pleurisy, from its commencement to its period of fullest effusion. Before we can understand the further history of pleurisy and of its consequences, we must revert to the pathological changes which accompany it.

If, in consequence of the treatment, or of the mildness of the irritation and its relief by the effusion, the inflammatory orgasm has ceased, the re-absorption of the fluid and lymph is a matter of course, and the ordinary efforts of respiration may suffice to expand the lung in proportion as this absorption proceeds. You will then have a gradual diminution and removal of the signs which marked the increase of the effusion. I need not go over these in detail, for if you know how the signs are produced, you will as readily perceive how they are removed. The return of ægophony, and of the sound of respiration, which reappears at the parts where it was last heard, generally the upper parts of the chest, are the most available signs of improvement; and these, together with diminution of the side, and a gradual improvement of the sound on percussion, are to be watched from time to time as proofs that the improvement is progressive.

In very moderate cases, the liquid is absorbed away before the lymph or albuminous matter is removed; and when the pleural surfaces covered with this come together, a sound of rustling or rubbing is sometimes heard, which soon ends in the permanent adhesion of these surfaces by bands of false membranes. Now, if these false membranes are formed after the liquid has been removed, and the lung has recovered its full extent of expansibility, they are adapted to its free motions, and do not to any material extent interfere with them. Hence, we often find in dead bodies adhesions which are lengthened in the lower parts of the chest, where, from the action of the diaphragm, the lungs descend as the ribs rise, whilst in the upper parts the adhesions are short, because the lungs there follow more exactly the movements of the walls of the chest. But in severer cases, which are also very common, inflammation continues after the liquid has been abundantly poured out, and not only increases and perpetuates this liquid effusion, but throws out albuminous matter also in various conditions, which, by its present qualities or future changes, may produce a variety of prejudicial effects, all tending more or less to interfere with the perfect restoration of the organs to a healthy state. Now, arising, as these consequences do, out of continued inflamma-

tion, which either has been imperfectly treated, or has been neglected from being latent, they are strong proofs of the value of the physical signs, which are never absent, and which will scarcely ever fail to declare the presence of the lurking mischief. Let us now glance at some of these solid products of pleuritis.

1. Healthy and highly organizable lymph, when deposited in a thick layer, must in some measure restrain the expansion of the lung, and thereby retard the absorption of the fluid. This lymph may be diminished by absorption; and the membranes formed of it may ultimately adapt themselves to the full expansion of the lung; but there will be less chance of this, in proportion as the liquid effused is copious, and its removal slow.

2. In cases similar to that just mentioned, but with a lymph less organizable, the product of a less active inflammation, or in which there is much of the colouring matter of the blood, its organization is more tardy, and the membrane resulting is more rigid, and of a less yielding nature; consequently the lung is more permanently confined in its compressed state. The membranes which are formed on the pleura in these cases are sometimes quite cartilaginous in density, and of considerable thickness; and I have met with several examples in which they have become partially ossified. If these are formed, and acquire their density before the liquid has been removed, it is quite clear that they must for ever bind down the lung; but I have seen several cases in which there have been proofs of contraction after the absorption of the liquid; and I should explain this by the fact that certain newly-formed tissues have a tendency to contract for some time after their production. You know how remarkably this is exemplified in the contraction of newly-formed skin after burns and other extensive wounds of the surface. It is also shown in the puckering of the tissue around false membranes lining cavities in the lung, in the contraction of the valves of the heart, and in many other cases where false membranes have been slowly formed, and tend to assume a fibrous or fibro-cartilaginous, rather than a serous or cellular, character.

3. But the inflamed pleura may throw out a lymph of still lower vitality, susceptible of but imperfect organization, and wholly incapable of throwing out more of an organizable character; hence, when the pleura is coated with it, if the inflammation continue, the overflow of the nutritive secretion will be in the form of a curdy matter, or of mere loose shreds of albumen.

4. The solid matter may be thrown out in a disintegrated state, utterly insusceptible of organization, and diffused through the fluid in flakes or particles, forming a mixture more or less resembling pus, which is the fluid of empyema. Although in many instances this is the result of a less active form of pleurisy, and owes its persistence or tendency to increase to the want of vitality in its solid matter, yet we do meet with cases of empyema which arise from very acute forms of inflammation. In these instances the fluid is more strictly purulent, the solid matter being in form of globules like those of pus, and seems to be the result of what may be called a suppurating diathesis, in consequence of which all the albuminous products of inflammation tend to assume a purulent character; nay, in such a case, I have seen pus formed within fibrinous clots of the blood itself in the heart; and it is difficult to avoid the supposition that it is something in the condition of the blood that determines this less usual product from the acute inflammation of a serous membrane.

5. Lastly, as the solid accretions on the inflamed pleura, you may have the various morbid productions called tuberculous, scirrhous, fungoid, and melanotic; these being commonly the result of some constitutional taint developed by the local inflammation, but sometimes also, as I believe, the product of peculiar modifications of the inflammation itself. This, however, is a subject that we cannot enter into at present.

Now you are not to suppose that the products of pleurisy, in every case, belong exclusively to one of the kinds now described, or that they are so simple as to be rigidly divisible by such a classification. You will rarely examine a case of pleurisy, after death, without finding traces of several forms of the products of inflammation: you cannot examine many without perceiving that these pass by insensible gradations into one another, and that although you may meet with many that are clearly referable to one denomination, you will find others in which the products of inflammation are mixed or intermediate. I assure you that I have long sought for a positive line of physical demarcation between lymph and pus, pus and yellow tubercles—between false membranes, serous, fibrous, and cartilaginous; and I have sought in vain; for although, in a majority of instances, it has not been difficult to say which name is most applicable, yet cases not unfrequently present themselves in which these products of inflammation have such a dubious and intermediate character, that it is difficult to avoid the conclusion that they pass by insensible gradations into each other. This is not a matter of merely speculative interest; for

if we find proofs of this transition in different instances, there is full reason to suppose that it may take place in the same case; and it becomes a matter of great importance to determine under what circumstances such transitions take place, and how they may be influenced so as to be directed in the most favourable way.

If you reflect on the consequences of all these products of prolonged inflammation of the pleura, you may perceive that they all tend to keep down the lung in the compressed state to which it was reduced by the first effusion, and they do this by the rigid false membranes which are formed when the solid effusion is susceptible of organization, and by the persistence of the liquid effusion when the solid matter is destitute of organization, and acts as an extraneous irritant. We must suppose, too, that the absorbing properties of the pleura must be altered by the long continuance of disease, and that various lesions are propagated to the adjoining tissues; which, however they may have escaped the immediate effects of inflammation in its acute form, can scarcely fail to be affected by the changes of nutrition and action induced by the slow, but less limited influence of chronic inflammation. Hence the parenchyma of the lung, the bronchi, the pericardium, the bones and cartilages of the chest, occasionally become the seat of various changes. Thus the lung becomes at first consolidated and then atrophied; the tubes secrete pus and become dilated; the pericardium forms adhesions to the heart, and becomes thickened; and in cases of empyema, the ribs, vertebræ, and their ligaments, sometimes become carious. Nor must we forget the unfavourable operation of the disease on other functions; the obstruction to the circulation by pressure on the pulmonary and adjoining vessels, the abridgment of the function of the lungs themselves, and the irritating or depressing influence occasionally extended from the seat of lesion to the various organs of the abdomen. In fact, besides the injury done to the respiratory organs, an imperfectly cured pleurisy may, in an insidious manner, oppress the whole system, and bring it into an anomalous cachectic state, in which morbid conditions of various kinds may be produced or brought into activity. Did our time admit, I would illustrate by many cases all these effects and sequels of pleurisy; and these cases would serve to show, too, that the form of disease that leads to such results, is often of the most insidious kind, either marked by no prominent symptoms at all, or losing them before the mischief is half subdued. But we have not time for these illustrations, which belong rather to a course of clinical medicine; but your own observation in hospitals or private practice will soon supply you with them, for such cases are by no means uncommon.

Now, for practical purposes, let us divide all these cases into two classes:—1st, those in which absorption ultimately predominates over effusion, and the liquid is sooner or later removed; and 2dly, those in which effusion predominates, and the liquid can only be removed through a perforation of the pleura. In the first of these, as the absorption of the fluid proceeds, what is it that supplies its place? The lung, as we have seen, is either so bound down by rigid false membranes, or so condensed and obliterated by the long-continued pressure, that it is not susceptible of its former expansion to effect this purpose. In the great majority of cases, as the liquid is absorbed away, the walls of the chest are contracted and fall in; so that the diseased side, which at the height of the effusion measured, perhaps, an inch or two more than the healthy side, now gradually becomes considerably smaller, sometimes to the extent of two or three inches. The contraction is first perceptible in the upper part of the chest, and, with the depression and more fixed condition of the shoulder, contrasts remarkably, on inspection, with the full development and active motions of the sound side. There is still some apparent fulness of the lower part of the diseased side, and this appearance will often continue even when it measures less than the sound side. If you examine the contracted side more narrowly, you will see in detail that the ribs are lower at the sides and closer together, the scapula more prominent and nearer to the spine, and sometimes the sternum, and occasionally even the spinal column also, is curved concave towards this side. In all this you see the results of the atmospheric pressure, together with unantagonized muscular efforts, acting on the walls of the chest. You may also have atmospheric pressure, contributing to the same end, from the abdomen: thus the diaphragm is pressed permanently upwards, carrying with it the mass of the liver on the right side, and the resonant stomach on the left; and on watching the abdomen, you do not see it swell on that side, as on the other, with the acts of inspiration. In some cases you will find that, even within the chest, the same atmospheric pressure is exerted from the sound side, causing displacements the very reverse of those which had been occasioned by the pressure of the previous effusion. Thus I have seen several cases in which the healthy lung had displaced the mediastinum towards the contracted side, not only under the whole sternum, but even under the cartilages of the ribs, to the ex-

tent of an inch beyond it; so that these parts sounded well on percussion, and the intercostal spaces there showed the movements of respiration, which scarcely affected any other part of that side. Nay, in one instance, I have seen the heart drawn over to the right side, after a latent pleurisy affecting that side, so that it was felt pulsating to the right of the sternum*.

But I have not described to you the auscultatory signs of a side contracted after pleurisy. In many cases, where the effusion has been copious and of long standing, the sounds of respiration and percussion continue imperfect permanently, although the liquid may have been completely removed; and in almost every case they are more or less impaired for months after the attack; in fact, they correspond pretty well with the appearance and diminished motion of the affected side, and are to be referred to the same causes. The improvement is generally to be found in the upper parts of the chest, and near the spine. With the return of a weak respiratory murmur, and slight resonance on percussion, some degree of vocal resonance may also accompany the removal of the liquid—in the superior parts of the chest amounting to bronchophony—in others being only the diffused pectoral fremitus, according to the size of the bronchial tubes, and the degree and permanency of their compression. The bronchophony in these cases is generally of a cracked or buzzing character; and I have known it very loud and diffused over some extent of surface. This is one of those instances in which some physical signs may deceive you, unless you pay attention to others, and to the general history of the case; for if, for the first time, you see a patient in this condition, and he happen to have an attack of bronchitis, you may be led to believe that the resonance of the voice and dulness on percussion are caused by consolidation, by recent inflammation of the lung, or by tubercles; but this error may be avoided by attending to the history of the case, and the signs of contraction that are also present. The dulness on percussion in the contracted chest is less owing to the absence of air, than to the compressed, drawn in, condition of the walls, which are no longer free to vibrate as usual; and although there be air in considerable quantity in the lung underneath, this air gives no spring to counterbalance the atmospheric pressure, which is continually acting as a dead weight on the contracted side. Sometimes you may get more sound by pressing the fingers strongly on the side, and then striking on them; this pressure brings the walls beyond the atmospheric weight, and within the influence of the contained air, so that they may then vibrate more freely. This is according to the principles of the sound on percussion which I formerly explained to you, and need not dwell on further.

You will observe that, in these cases of pleurisy, the condition of the side of the chest was, at the period of copious effusion, the reverse of what it becomes when that effusion is removed; then it was dilated, and the adjoining parts pushed from it; now it is contracted, and the adjoining parts are drawn into it. Is there not, then, an intermediate stage in which neither of these conditions is presented, and the side has the usual shape and dimensions? My experience leads me to say no; for the transition from one condition to the other is not generally uniform, but partial. The common case is, that the contraction begins in the upper part of the chest, before the dilatation and displacements have ceased in the lower; and it seldom happens that there is not, during the diminution of a pleuritic effusion, an irregularity in the shape of the chest, a comparative bulging of the lower portion, which may serve to distinguish it from the case of a consolidated lung.

In the cases which we have hitherto considered, the effusion was supposed to be general into the whole cavity of one side of the chest, and so was the contraction which succeeded it; but what contraction can obliterate a cavity left by partial effusions, limited by rigid adhesions, such as you see in these drawings? [See figures 4 and 5 in the last lecture.] I have had occasion to see the *post-mortem* examination of a few such cases: in two of them the cavity was not obliterated, but contained air, secreted, I presume, from the membranes, for there was no perforation of the pleura. In another there had been partial contraction; but a considerable space remained, and was filled with a semi-solid curdy matter, like what is found in old vomicæ, and probably of the same kind;—but more of this by and by.

Well now, what would you think of these cases of contracted chest, whose respiratory organs are reduced by nearly one-half? You would suppose them to be a crippled set of beings, capable of dragging on but an imperfect state of existence; yet it is curious enough that there have been some such individuals who have continued to enjoy good health,

* Dr. Stokes, in his late work, has also described this mode of displacement of the heart. Soon after these lectures were delivered, I met with another case of the same kind; the patient is now under the care of Dr. Symonds, of Bristol, who has lately informed me that the pulsations of the heart are still most distinct to the right of the sternum.

and to be actively engaged in the pursuits of life. Laennec tells us of the case of a distinguished surgeon of Paris, who, although he had one side contracted in a very marked degree, from an attack of pleurisy in his youth, yet enjoyed excellent health, and was in the habit of lecturing twice a day without inconvenience. I have known two or three instances of the same kind; but I must say that here the contraction was not excessive, and the respiratory murmur was by no means abolished. In other cases—and they are, I think, the most common—extensive contraction of the chest causes such an habitual shortness of breath, as to incapacitate the person from all active exertion, and to make any slight bronchitic attacks, or febrile excitement, cause severe and distressing dyspnœa. I have further had occasion to observe, that before the system becomes accommodated to the abridgment of respiration which this lesion produces, there is an enfeebled or cachectic state of the body, in which various trains of disorder may arise; and unless care be taken to counteract them by remedies and circumstances most favourable to the general health, scrofulous or dropsical disorders may be engendered, and develop new mischief in the respiratory organs, or elsewhere. Although, therefore, we may look on contraction of the chest as a mode of curative termination of pleurisy, it is one of the least favourable kind, and liable to many detracting circumstances.

We have now to advert to the other class of cases, in which effusion preponderates over absorption, and the liquid can only be removed through a perforation of the pleura. This character is to be traced, in some instances, to the condition of the membrane, which, either from its continued irritation, or from a change of structure, secretes more than it can absorb. Sometimes the accumulative tendency of the effusion may arise from some obstruction in the circulation, dependent on disease of the heart or great vessels, or even on the partial pressure of the effusion itself. But the more common cause of increasing effusion is the nature of the matter effused, which, when of a purulent character, is not readily absorbed, and constitutes the *empyema* of authors. This, when accumulating rapidly, proves a source of oppression to the breathing, and of immediate danger; if slowly, of irritation, obstruction, and consequent disorder to the vital powers generally. With the presence of pus there is not uncommonly associated an ulcerative process, which may give vent to the matter through the lungs, the walls of the chest, or the diaphragm, and which, in cases of long continuance, not unfrequently extends to the vertebræ and ribs, and their cartilages and ligaments, and to form extensive abscesses between the muscles and integuments of the chest and abdomen. The prejudicial nature of such consequences of pleurisy are too obvious to need further comment from me.

You may have remarked that in this review of the pathology I have not given any separate notice of *chronic pleurisy*; and my reason is, that the transition of the acute to the chronic is so indefinite, and the symptoms of the recent disease sometimes have so little of an acute character, whilst that of a long duration occasionally manifests so much greater an intensity of irritation, that the terms acute and chronic would be less applicable to pleuritic affections than to inflammations in most other organs. And we can see some reason for this, in the fact, that the pleura being a shut sac, is liable to have its acute inflammations made a chronic disease by the retention of its product; and chronic pleurisy is perpetually liable to be excited into an acute state by the distending or irritating influence of the effusion. Still, differences of disease are very apparent in many cases, in the prevalence of high inflammatory fever in some, and in there being no fever, or one of a hectic kind, in others; in the sthenic condition of the circulation in some, and its depressed weak state in others, whatever may be the degrees of pain or nervous irritation accompanying them. These differences, although not easily included in a general view of the pathology, must not be disregarded in clinical observation, nor in the direction of the treatment.

I shall be very brief in speaking of the treatment of pleurisy, for you will find this pretty fully described by recent authors. The most desirable object is to destroy the inflammation at its onset, when the only physical sign may be the friction sound, and before the signs shew that the effusion is considerable. For this purpose, the most effectual remedy is a full venesection, carried, if possible, to such an amount as to remove all pain on full inspiration; or if there have been no pain, until all hardness of the pulse ceases. This should be followed by free leeching or cupping the affected side. I think leeches are rather to be preferred, but they should not be applied sparingly, and should be immediately followed by a large warm poultice covered with flannel. These depletory measures must be repeated if within a few hours the pain returns, or the pulse resumes its hardness. Of internal remedies, those are most useful, in the first instance, which will aid the bloodletting in producing an impression on the circulation, especially brisk purgatives

containing mercury and antimony, which act fully on all the secretions. Calomel and James's powder, followed by an active salts and senna draught, generally answer best. Tartarized antimony is less effectual in this than in other inflammations of the chest; and I have seen it do harm by exciting vomiting; but in doses short of that effect it may prove useful.

It commonly happens that such measures take off the edge of the disease without destroying it entirely, which must be a work of time; and although the pain, dyspnœa, and cough, be much relieved, they are not removed, and the physical signs show that the effusion to a greater or less extent has taken place. Under these circumstances the proper means are those which tend to remove the remaining inflammation, and to promote the absorption of the matter already effused. The most powerful of these is mercury, which may be combined with enough of opium and ipecacuanha, to prevent it from passing off too freely by the bowels; and to this may be added digitalis or colchicum, in a saline mixture with an excess of alkali, to keep down the action of the heart and arteries, to lower the inflammatory condition of the blood, and to determine to the kidneys. The beneficial influence of mercury is sometimes apparent when it does not affect the gums, especially in young subjects, its operation being only manifest on the hepatic and alvine secretion, which is green, dark, or high coloured, from different conditions of the bilious matter in it; but in more cases the gums exhibit its operation before these excretions are produced.

Venesection can seldom be repeated with much advantage after the first few days, unless on the occasion of a fresh access of pain, or other symptoms which prove the renewal of acute inflammation. Occasional leechings continue to be useful; but after the inflammatory fever has been reduced, the most effectual external remedies are blisters, which should be used large, and not left on too long; from six to eight hours is generally time enough to make them vesicate without irritating the system by the absorption of their matter. Where the effusion is abundant, a succession of blisters will be necessary; or they may be varied by a suppurating counter-irritant, such as the tartar-emetic ointment or solution. The diet, of course, must be of the most starving kind in the first instance, and the patient must remain as quiet as possible in bed. When the inflammation is subdued, on the other hand, sitting up, and if he can bear it well, using a little exercise about the room, will tend to promote the absorption of the fluid. If, from insufficiency, or in spite of treatment, the signs of effusion continue beyond two or three weeks little or not at all diminished, with more or less constitutional disturbance, it is to be apprehended that the disease may take a chronic form, in which the character of treatment must, to a certain extent, be changed. If the strength continue to be lost, the pulse being weak, and the fever, if any, of a hectic kind, a more nutritious and tonic plan must be cautiously pursued to the extent that the patient can bear it; the heat of skin, pulse, cough, and condition of the breathing, being referred to as tests of the suitableness of the change. But external counter-irritation should still be continued, especially by blisters, which may, perhaps, be useful, not only to remove internal inflammation, but also to restore energy to the external muscles of respiration, as they do sometimes to those of a paralyzed limb. The secretions must be kept free by medicines of a milder class than those used in the acute stage; and except with this view I have not found much benefit from the use of mercury in the late asthenic stages of simple pleurisy. In fact, when the effusion has a purulent character, I believe the constitutional action of mercury to be injurious. In these stages I have seen the most salutary effects from the employment of the hydriodate of potass, in the dose of two or three grains, three or four times a-day; or in more asthenic cases, the iodide of iron, in rather smaller doses. To preserve the latter from decomposition, it should be kept in solution with a piece of clean iron or a coil of iron wire in the bottle, as recommended by Mr. Squire. As there is occasionally apt to be, in both these medicines, some free iodine, which often occasions gastric irritation and nervous symptoms, I always desire patients to eat a little bit of bread or biscuit after each dose; the starch in this combining with the free iodine, removes its injurious property. These medicines act on the kidneys, and seem especially to promote absorption; and I know of none which seem so often to succeed in hastening the removal of a pleuritic effusion. Various other medicines may be useful in particular cases; but we have no time even to name them. Where a dropsical diathesis prevails, the tartrate of iron is also sometimes a very good diuretic. Gentle exercise is serviceable in promoting the expansion of the chest and lungs, and the absorption of the fluid; and in the more chronic cases, it is of the utmost importance to promote the general health by free exposure to a pure, mild, and suitable air, which, in cases disposed to scrofula, will be best found at the sea-side.

It only remains for me now to say a

few words on the subject of tapping th chest, an operation which, although be longing properly to the province of surgery, should only be performed under the guidance of a competent knowledge of the pathology and physical signs of pleurisy. There are two kinds of cases in which it becomes proper to give exit to the liquid accumulated in the pleural sac. One includes the examples of the recent disease in which the effusion takes place so quickly and abundantly as to endanger life by the pressure which it causes on the lungs. A sudden effusion may have this effect, where its quantity is not sufficient to compress the lung totally, or to displace the viscera to a great extent; but there will be always some enlargement of the side, which, with the dulness on percussion, and the absence of respiration and vocal resonance, will sufficiently indicate the nature of the case. Here the liquid is generally serum with more or less lymph, and it generally deposits a further clot of gelatinous fibrine after it has been drawn from the chest; but the same liquid is sometimes yielded by pleurisies of long standing. The other class comprehends those cases in which the pleurisy has existed for a longer time, and the effusion, instead of showing a disposition to disperse under the influence of remedies, either increases or remains stationary, and whether it cause a dangerous degree of dyspnœa or not, it must cause mischief by perpetuating the compressed state of the lung, as well as by the various other structural and functional affections which I have before alluded to. The cases of true empyema are generally included in this number, and are considered the more legitimate subject for operation, because it is believed that there is little chance of purulent matter being removed by absorption; but I must add, that experience proves that the operation is not so often successful in these cases as where the effusion is not purulent.

Now the object of the operation is to remove the fluid, which either from its quantity oppresses functions, or by its quality extends and perpetuates structural lesions within the chest. With this view an external vent is given to it, any number of times, until its quantity is so far diminished, and its quality improved, that it shall not prevent the re-expansion of the lung, as far as that is possible, and the contraction of the chest to fill up the remaining deficiency. But you can easily perceive that, after the first pressure is relieved, the remaining fluid cannot be drawn out of the chest without something to occupy its place; and unless great precautions be taken, that something will be air, not drawn into the lung, but through the orifice into the cavity of the chest. Now air thus introduced often seems to have the effect of causing an unfavourable change in the remaining liquid, rendering that which was serous decidedly purulent, and giving to pus a fœtid character. Further, when air gets access, it tends to do mischief, whether the orifice remain open or be closed: in the former case, the air passing in and out prevents the lung from expanding, and constantly irritates the serous membranes, which are not fitted for contact with it; and if it be closed, the air admitted tends to engender more air, by the decomposition which it causes in the remaining liquid, so that the pleura soon becomes as much distended as before the operation. Hence, although the operation generally gives temporary relief, it has often been followed by symptoms of irritation, or of increased oppression, which have ultimately led to an unfavourable result. The cause of failure in some cases, it is true, is irremediable disease in the walls or viscera of the chest, or perhaps in the constitution, which may be either the cause or the effect of a long continuance of the pleuritic effusion; but even in these cases the operation may prove the means either of great temporary relief, and considerable prolongation of life, or of just the contrary, according as it is performed or not with due reference to sound physiological and pathological principles. The operation has, it is true, sometimes succeeded where these have not been much attended to; but this has been for the most part in young subjects, where the reparatory powers are active, and sufficient to countervail very unfavourable circumstances; and I feel confident that it would succeed in many more instances, were it resorted to at the time and in the way pointed out by the structure of the parts, and their properties in health and disease. One great error has been to delay the operation too long, until some of the bad consequences of rigid membranes, obliteration of the tissue of the lung, surfaces secreting unhealthy matter, and incapable of adhesion, ulcerations and even sloughing of the soft parts, caries of the bones, morbid deposits in other parts, and depression of the vital powers generally, prevent the possibility of recovery. This error having arisen in some measure from some uncertainty in the diagnosis of liquid effusion, ought now to be prevented by the improvement of our knowledge in this respect. Another great error I consider to have been, a neglect of all means to promote the re-expansion of the lung, which is the only natural mode of supplying the place of the fluid drawn off, and is an obvious step towards a restoration of the healthy condition and function of the

parts. The common notion is, that the fluid should be drawn off, without considering whether any thing more hurtful may occupy its place.

I find that I have not time to describe to you the different modes of operation that are recommended by authors, or that I have seen performed, so I will merely tell you what appears to me to be that which promises the best chance of success, according to the views which I have been explaining to you. The spot for the introduction of the trocar must be determined with due reference to the physical signs, carefully avoiding every part where, or near which, there is any sound of respiration, resonance of the voice, or not perfect dulness on percussion. A projection and fluctuation of an intercostal space give a greater eligibility to a spot; and these circumstances present themselves most frequently at the inferior lateral parts of the chest, from the fourth to the eighth rib, where also the soft walls of the chest are as thin as any where. In all cases it is a proper precaution to pass a grooved needle first, as recommended by Dr. T. Davies, for this will determine the presence of the liquid, and in some degree its nature, and the thickness of the walls which contain it. Of course it is proper to avoid the immediate neighbourhood of the heart, or any of the known arteries or nerves. The upper margin of the sixth rib most commonly presents a favourable spot, but whether more or less at the side must be determined by the position of the heart and other circumstances. Now as I hold it to be the great object to not only remove fluid, but promote the restoration of the function of the lung, and therefore to prevent the admission of air by the puncture, I would have the whole operation conducted with this in view. The patient should be lying on his back, or not more propped up than is necessary for the state of his breathing. The puncture, instead of being in the most dependent part of the chest, should be made as near the front as is consistent with the safety of the viscera and other considerations just alluded to. The trocar should not be pushed in further than is necessary to clear the parietes; but the canula may be pushed in further after the stilette is withdrawn, and its sides should have several holes in them. The skin should be drawn aside, so that the puncture through it may not, after the trocar is withdrawn, correspond with that in the costal pleura, but may form a valvular orifice. As soon as the stilette is withdrawn, steady pressure should be applied, by the hands of assistants, to the affected side to depress the ribs and shoulder, and to press up the diaphragm, to promote the flow of liquid, and to prevent the introduction of air through the orifice, during any sudden and forcible act of inspiration. For the same reason, during a fit of coughing, if there appear any tendency to intermission in the stream of liquid, the orifice should be closed by the finger. The pressure should be steadily increased as the liquid flows; and if the stream should stop suddenly, a probe should be passed through the canula to clear it of clots of lymph or other obstructing matter, and this may be done also if the stoppage is more gradual; but if still no more flows, the canula should be quickly withdrawn, and a compress, or a large poultice, placed on the orifice; and then, but not till then, the pressure on the walls of the chest may be withdrawn. What will be the result? The walls of the chest, expanding by their own elasticity on the removal of the pressure, will draw air into the compressed lung, which being thus expanded, will begin to resume its part in the function of respiration and circulation, and will thus promote the absorption of the rest of the fluid, and improve the condition of the whole system. Even if the fluid should accumulate again, the temporary expansion of the lung will have served to restore its condition, so that when another quantity of fluid is again withdrawn, its tissue will be the better prepared for a restoration of its function. When the fluid is of a decidedly purulent character, or if otherwise its absorption is difficult, it may be useful to displace it, by injecting through a double-tubed canula warm water; to which may be added, in some cases, a weak solution of nitrate of silver or some other medicament, which may improve the condition of the membrane, and dispose it to secrete adhesive lymph instead of pus.

LECTURES ON BLOOD-LETTING,

Delivered from time to time,
At the General Dispensary, Aldersgate Street,

By Henry Clutterbuck, M.D.
Formerly one of the Physicians to that Institution.

Lecture II.

History of Blood-Letting.

It is not my intention on the present occasion, gentlemen, to enter with much minuteness into the *history* and *origin* of *blood letting*. Such a discussion, though not without interest, is rather a matter of curiosity than of practical use, and would needlessly consume time that is already too limited for our purpose. It would be a fruitless task, indeed, to seek to discover

when blood-letting was first introduced into the practice of medicine. Like most other branches of the healing art, its origin is involved in impenetrable darkness. It is certain, however, that the practice is of great antiquity, and was in general use long before the time of Hippocrates, the earliest writer on medicine whose works have reached us, and who flourished considerably more than 2000 years ago. Hippocrates appears, from his various writings, to have been familiarly acquainted with phlebotomy, or venesection, and also scarification, both with and without cupping. On different occasions he advises opening the veins of the arm, of the feet and legs, of the forehead, and those under the tongue. It is doubtful whether he practised *arteriotomy*, or was acquainted with the use of leeches for drawing blood. The purposes for which he had recourse to blood-letting are various, and not undeserving of notice even in the present day. He employed it, in the first place, simply as an *evacuant*, in order to get rid of redundant matters in the system; secondly, for the purpose of changing the determination of the blood to or from particular parts, as circumstances might seem to require. You may here observe the germ of the doctrines of *derivation* and *revulsion*—points of theory that were so much dwelt on at a later period, and which have scarcely yet lost their influence on practice. A third purpose for which Hippocrates had recourse to blood-letting was that of restoring a free movement of the blood and animal spirits in cases where they were supposed to be stagnant or obstructed, as in *apoplexy* and *palsy*. Fourthly, he used this evacuation to *cool* the body when morbidly or preternaturally heated.

Such were the views with which blood-letting was employed by Hippocrates; and, accordingly, he had recourse to it in a great number of diseases where it was supposed to be needed to fulfil one or other of the indications mentioned. In particular he recommends it in violent and acute diseases, especially *inflammations*, but only where the patient is strong and at the middle periods of life, for he thought blood-letting not adapted to either infancy or old age: he forbad it also to pregnant women, as being likely to cause abortion. He especially mentions inflammation of the different viscera—the liver, spleen, and lungs, as calling for this remedy. The boldness of his practice on these occasions deserves notice. In pleurisies, for example, he allowed the blood to flow till the patient fell into complete syncope, especially if the pain were very acute. When, however, the blood, while flowing, underwent a change of colour, as from a red to a dark hue, or the reverse, he considered it right to stop. In quinsies, and other disorders of an acute form, Hippocrates bled in both arms at once. Difficulty of breathing is another case in which this remedy was resorted to by Hippocrates; and he mentions one species of inflammation of the lungs, which he calls tumefaction from heat, in which blood is directed to be taken from various parts of the body at the same time, instancing, in particular, the arm, under the tongue, and the nostrils, as parts proper for the purpose. In cases of severe pain, he advises a vein to be opened as near as possible to the part affected. Thus, in pleurisy, the blood was taken from the internal vein of the arm of the affected side: but in milder cases he preferred taking it remotely, for the purpose of soliciting a flow of blood to a distant part of the body. He bled for the relief of pain simply as unconnected with inflammation. For the cure of diseases situated above the diaphragm he took the blood from the veins of the arm or upper part of the body; in those below the diaphragm, the blood was taken from the inferior parts, as the feet.

In fevers, even of the most ardent kind, Hippocrates did not draw blood, unless the fever were accompanied with some topical inflammation; thus making a distinction between primary fevers, (that is, where the fever itself is the disease—the idiopathic,) and those which accompany or follow inflammation (symptomatic fever.) It was in the latter only that he thought blood-letting adviseable. In general, in acute cases, the patient was bled only once, but then to a large amount, even to fainting, as stated above, if the violence of the symptoms seemed to require it. In chronic inflammations, small and repeated bleedings were preferred; and instances are adduced of the good effects of this practice. He also advises it for the cure of dropsies under particular circumstances. It is remarkable that Hippocrates no where indicates the quantity of blood to be drawn in any particular case.

From what has been now stated, as well as from the general tenor of the writings of this most distinguished physician (a man who has had no equal in our art), you may perceive, and probably not without surprise, considering the disadvantages under which he practised at that early period, when medicine was little more than a science of observation, unaided, for the most part, by the lights of anatomy, physiology, or the other auxiliary sciences,) how little reason we have to boast of modern acquirements, and how little, in fact, has been added to the stock of real practical knowledge since his time.

Erasistratus (who lived a century or more after Hippocrates, and who is distinguished as being the first, according to Galen, who ventured to dissect the human body,) prohibited both blood-letting and purging for the cure of diseases; at least he nowhere recommends their use. In hæmorrhages, instead of drawing blood as his contemporaries did, Erasistratus substituted low diet and ligatures on the extremities, with the view, probably, of restraining the movement of the blood. The reason assigned by his followers for abstaining from blood-letting in general is not of much weight; namely, that as, in the treatment of fevers and inflammations, abstinence is indispensable, it would weaken the patient too much to use blood-letting at the same time. Other frivolous objections to the practice of bleeding were made, such as that it is not always easy to discover the vein proper to be opened, and that there is even danger of opening an artery instead of a vein; that many persons fall into a state of syncope either before or after the operation; and that some have actually died of fright under it; that we cannot always tell the precise quantity of blood required to be taken away in any particular case in order to subdue the disease; and that if less than this be taken it does no good, and if more, that we run the risk of killing the patient. Others said that the escape of the blood from the veins might be followed by that of the animal spirits, which, on such occasions, were supposed to pass from the arteries into the veins.

Asclepiades, a Greek physician of high repute, who settled at Rome a hundred years before the birth of Christ, followed pretty closely the steps of Erasistratus, except in regard to blood-letting, which he employed to a considerable extent. Asclepiades pursued, on the whole, a more active practice than that of his predecessors, which, on account of its inertness, he called "a meditation upon death." His reasons for abstracting blood, however, were of the most fanciful description, founded on the Epicurean doctrines, which represent all the phenomena of living bodies as depending on the motion of corpuscular atoms through corresponding pores, and disease as consisting in a want of correspondence between those corpuseles and pores. Asclepiades used blood-letting in inflammation provided pain were present, but not otherwise: thus, he bled the patient in pleurisy, but not in peripneumony or inflammation of the lungs themselves, because, in the latter, the patient does not suffer pain. He neither bled in fevers nor in phrenzies. The activity of his treatment in general, however, is shown in his treatment of quinsy, in which he employed at the same time, venesection in the arm, under the tongue, and on the temples. He also used scarification with cupping, with the view of *opening the pores*. If these means did not suffice, he incised the tonsils, and even recommends laryngotomy in extreme cases. All this, you will perceive, does not seem very consistent with the favourite and avowed maxim of this writer, that of curing *citò, tutò, et jucundè!*

Aretæus was very friendly to blood-letting, but he preferred, in general, small and repeated bleedings to large ones, which he considered to be dangerous. One motive assigned by him for the use of blood-letting was to produce relaxation of the solids; as for facilitating the passage of calculi through the urinary passages. This, you perceive, is in conformity with modern practice. Aretæus was amongst the earliest that practised arteriotomy. Speaking of this author, Haller says, "arterias incidit, ante Galenum, in temporibus et ad aures; alibique."

The Methodic Sect, of which Themison was the founder, used blood-letting frequently, and in the same diseases as Hippocrates did, but not with the same views. Instead of bleeding, in order to cool the body, they employed it to produce relaxation, all diseases consisting, according to their tenets, either in too great rigidity, or its opposite, relaxation. They seldom bled the patient more than once, unless by scarification and cupping, which they carried to a great extent, and successively, over almost the whole body. This sect, the Methodic, comprised many individuals of great reputation.

Celsus, equally remarkable for the elegance of his style, and his intimate acquaintance with the whole art of medicine as it existed in his time, was a great friend to blood-letting, and appears to have possessed an extensive and critical knowledge of the subject. He advises it in many cases in which his predecessors had forbidden its use, as in infants and in old persons, and also during pregnancy, observing on those occasions, that it is a matter of little importance to inquire what the age of the patient is, or whether pregnant or not, but what is the strength in each case. A vigorous infant, a robust old person, and a strong pregnant woman, bear this evacuation, he says, without either danger or inconvenience. Favourable, however, as Celsus was, in general, to the employment of this invaluable means of cure, he complains of the almost indiscriminate use that was made of it in his time. His rules with regard to blood-letting are, indeed, in most respects, conformable with the best views of modern experience. The strength of the patient

is to be looked to, he observes, more than the other circumstances of the case; and this is better judged of by the quantity and quality of the blood, than by the aspect of the patient. It may, however, happen, he adds, that the disease calls for this evacuation while the body is not able to bear it. Still, if no other means offer themselves for saving life, he does not hesitate to recommend a trial of it; for, "satius est anceps auxilium experiri, quam nullum." In short, Celsus appears to have employed blood-letting in nearly the same cases in which it is resorted to at present, especially in violent fevers, where the face is much flushed and the veins distended. In pleurisy, he used it only when the disease was recent, and the pain acute. In peripneumony, he only had recourse to it when the patient was strong; if otherwise, dry cupping was employed. Apoplexies, palsies, convulsions, difficulty of breathing, internal contusions (indicated by spitting or vomiting of blood), and violent pain, are all of them cases requiring, in the opinion of this justly-esteemed writer, the loss of blood. A rather curious observation is made in regard to apoplexy, namely, that bleeding sometimes appears to save—at other times, to kill the patient. This seeming anomaly I shall have occasion to explain to you hereafter.

With respect to time, Celsus did not bleed (except in urgent cases) earlier than the second day of the disease, on account, as he observes, of the crudity of the humors that are not yet ripe for evacuation; and he objected, likewise, to taking away blood later than the fourth day, for the reason that, by this time, the bad humors would be dissipated spontaneously, or, at least, have made their full impression on the system, in which case the only effect of bleeding would be, that of needlessly weakening the patient. By the by, do you not observe here, gentlemen, as on other occasions, the baneful influence of hypothesis on practice? through which the advantages of experience itself are often sacrificed; for certainly experience does by no means sanction the limitations here suggested. Celsus makes an observation confirmatory of that of Hippocrates, which I before noticed—namely, that when the blood, as it flows from the veins, begins to assume a bright vermilion hue, the further evacuation ought to be put a stop to; as being then rather hurtful than beneficial. Celsus was no friend, in general, to large blood-letting, so as to induce syncope; but he advises rather, that if a large quantity is required to be drawn, as necessary to the cure, that it should be taken at different intervals. This justly celebrated writer makes no mention of leeches, though they were in use long before his time, Themison having employed them. Celsus's independence as a writer is manifested in the ridicule he casts on the critical days of Hippocrates, the establishment of which he ascribes to the influence of the mysterious numbers of the Pythegoreans.

We find little that is original, or particularly deserving notice, on the subject of blood-letting, in the writings of physicians between the period when Celsus lived—that is, at the commencement of the Christian æra—and the time of Galen, who flourished in the second century afterwards. Galen attached himself chiefly to Hippocrates as his guide, whose system he laboured to re-establish and bring to perfection, in opposition to the prevailing sects of the time. With the exception of Hippocrates, no individual, perhaps, ever enjoyed so high a degree of celebrity, both living and posthumous, as the subject of our present remarks; and, for the most part, his fame was well deserved; for his knowledge of philosophy in general, as well as of medicine, was of the most extensive kind. Galen's vanity, however, was at least equal to his merits. He decries, in the most opprobrious terms, all who differed from him, either in opinion or in practice. The manner in which he expresses himself in regard to Hippocrates, sufficiently shows his excessive self-esteem. "No one, before myself," he says, "has pointed out the true method of treating diseases. Hippocrates, indeed, indicated the road; but as he was the first to discover it, it is not likely that he would be able to proceed so far as was desirable in it;" and he goes on to compare his own works in medicine with the great social and political improvements effected by the Roman emperor, Trajan; leaving his readers to draw the conclusion, that as Trajan was the greatest and most beneficent of emperors, so he (Galen) was the first and greatest among physicians. In another place he thus addresses his pupils:—"I never cared about the reputation I might gain in the world; truth and knowledge have been the only objects of my ambition. On this account, I never put my name at the head of my writings, and you know I forbad your lavishing extravagant encomiums on me, as you were wont to do."

Our present concern, however, with this distinguished physician, is in reference to blood-letting, which he employed to a considerable extent. In this point, indeed, as in most others, he followed the steps of Hippocrates, drawing blood with similar views—viz. to diminish plenitude (plethora), and to make a diversion (revulsion) of the humours; but he used the remedy with more freedom than his great predecessor. Galen appears to be the first

that mentioned the absolute quantity of blood necessary to be taken on different occasions: neither Hippocrates, Celsus, nor any preceding writer, taking any notice of this. In ordinary cases, the largest quantity mentioned by Galen did not exceed a pound and a half; the smallest, seven or eight ounces. He held, that on certain occasions, blood might be properly taken till the patient fainted; and he mentions, as an extraordinary circumstance, that he had drawn from the same individual fifty-four ounces in the space of a single day. This was done at the commencement of an acute fever, where the patient was of a plethoric habit, and the blood in a state of great commotion. He limits this mode of practice, however, to the cases just mentioned; and as a caution against the general use of it in this way, he observes, that he saw two persons die in consequence of its adoption. As a safer practice, he advises that the patient should be bled to a smaller amount twice in the same day, or on successive days. Galen always bled the patient at the time of the day when the fever was at the lowest ebb, and, like Hippocrates, he took the blood from the side affected. He did not hesitate to bleed old persons when of a robust habit, but he never bled children under 14 years of age.

The physicians, many of them of high reputation, who lived in the two succeeding centuries, up to the period when the schools of physic and philosophy were transferred to Arabia, were for the most part compilers merely from the writings of Hippocrates and Galen. We are indebted to the Arabians, however, for the introduction of a number of articles into the materia medica, that were not before known or in use. We also owe to them the first description of several new and important diseases, that appear to have been totally unknown to the Greek and Roman physicians, such as the small-pox, measles, and other eruptive fevers, as we term them—diseases possessing strikingly peculiar or specific properties, and of totally unknown origin.

The Arabian physicians, of whom Avicenna was the chief, copied the Greeks in all essential respects, both as regards theory and practice. They were accustomed, however, to attach importance to some minor points, which are at present considered as of no moment. Thus, in cases of pleurisy, they said that the blood ought to be drawn from the side opposite to the disease, contrary to the injunction of Hippocrates, who directed that, for the relief of pain in general, the blood should be taken from the vein the nearest to the part affected. This difference of opinion, trifling and unimportant as we now consider it, was the cause of great and lasting dissension in the schools of physic, and entire volumes were written and published on the different sides of the question. To such a height, indeed, was the dispute carried, that the University of Salamanca, in the fifteenth century, took part with the Arabians, and made a decree that no one should dare to let blood from the side affected; and, to add authority to their decree, they endeavoured to procure an edict from the emperor, Charles the Fifth, to confirm it, alleging that the contrary practice was as prejudicial to the community as Luther's heresy itself; nor was the controversy terminated till the discovery of the circulation of the blood, by our distinguished countryman, Harvey, put at once an end to the dispute.

The Egyptians, as we learn from Prosper Alpinus*, used blood-letting in all its varieties, and to a great extent. They opened the veins of the temples, forehead, ears, corner of the eyes, nostrils, and throat, as well as others. Arteriotomy appears to have been nearly as much in use among them as venesection, especially in diseases of a chronic character. They opened the arteries about the temples, ears, and hands, with great freedom; nor had they any dread of the consequences usually apprehended from the operation. Prosper Alpinus says he had frequently witnessed the practice of arteriotomy while he was in Egypt, and observed the puncture to heal as readily almost as after venesection. They used certain precautions, however, in the employment of arteriotomy, both for insuring the success of the operation, and preventing inconvenience afterwards. By the application of a ligature at the proper part, the artery became much distended; it was then opened with the sharpest instrument, and by the smallest puncture possible, on account of the difficulty experienced in healing any large wound of an artery. When as much blood had been discharged as was thought requisite, some lint was placed on the wound, and a small piece of coin was strongly bound over it; and this was left on the part for three days, when the wound was found to be healed. The ancients seldom had recourse to arteriotomy, from an apprehension that life might be endangered by it, either from the artery not healing afterwards, or from gangrene taking place, owing to the great pressure required to stop the flow of blood. Galen †, on account of the mischief he had observed to follow in cases where the artery was accidentally opened in venesection, was not very favourable to the use of arteriotomy, unless for special purposes.

* De Medicina Ægyptiorum.

† Lib. de Curatione per sanguinis missionem.

He relates an instance, however, in which he opened the artery between the thumb and fore finger, in order to procure sleep; and he adds, that he witnessed the cure of a pain in the side, by opening the artery of the arm, and without aneurism following.

Scarification was so common among the Egyptians, Prosper says, that out of a hundred children you might meet in the street, you would scarcely find forty whose ears were not covered with cotton, on account of the scarifications they had undergone. Leeches do not appear to have been in use in Egypt; perhaps from these animals, as is suggested, not being found in that country.

Paracelsus, the prince and prototype of quacks, though far more learned and illustrious than the modern race to whom we are accustomed to apply this designation, has the merit of introducing into practice several active preparations from the mineral kingdom. Paracelsus appears to have employed blood-letting, as well as other means of cure, though the particular views with which he used it are not stated.

If, now, you inquire into the fate of blood-letting in modern times (including under this denomination the long interval between the revival of learning in the fifteenth century and the present times), you will find, that with few, and those trifling exceptions, the advantages of blood-letting have been highly, if not duly, estimated. In all ages indeed, not excluding the present, a few individuals have been found to decry the practice altogether, as fraught with evil consequences: some have even spoken of it as if it were unjustifiable on Scriptural grounds, arguing that "the blood is the life," and we are forbidden, in Scripture, "to take away life." An argument of this sort is not likely, I presume, to meet with many supporters in the present day. The opposition to the practice has been chiefly founded, I believe, in an affectation of singularity, for the sake of vulgar notoriety: it is certainly not sanctioned by experience.

It would occupy too much of your time, were I to pursue further the history of this all-important subject. I shall conclude, for the present, with observing, that at all times, and amidst the prevalence of every variety of medical doctrine, the merits of blood-letting have been fully recognized by the most observant, as well as the most experienced, physicians. Seeing, then, the almost universal use that has been made of this remedy for the treatment of disease, from the earliest periods to the present time, and that it has been made the subject of innumerable writings, you will perhaps wonder that it should be thought requisite to discuss the subject further in the present day. When, however, you are told, that many and important differences of opinion still subsist with regard to it, amongst those even who are the most favourably inclined towards it, as to its mode of operation—the circumstances that influence its use—the extent to which it ought to be carried at different times and under different circumstances—as well as various other important points—you will admit, with me, that there is still room for further investigation of the subject. This I purpose to enter upon in the ensuing lecture.

CONTRIBUTION

TO THE

HISTORY OF THE NERVOUS SYSTEM.

Read before the Medical Society of University College, March 30th, 1838.

By John Durance' George,

One of the Presidents.

(For the London Medical Gazette.)

[Concluded from page 47.]

66. In resuming the history after the preceding recapitulation, the question that presents itself is not so much "what has Prochaska done?" as what has he left undone? For after making the fullest deductions for his errors and omissions, it is impossible not to award him the highest praise for his scientific generalization and explanation of these phenomena. They are not alluded to *incidentally* in a part of the work devoted to another subject, but treated of specifically and distinctly; concisely, it is true, and yet so clearly, that the great principle, the theory, is laid down; and the chief improvements left to his successors are those of perfecting the details and extending the system.

67. When it is remembered that he wrote long before the discoveries of Sir C. Bell, on the functions of the double roots of the nerves, the omissions of the *particular* nerves by which impressions are conveyed, and into which they are reflected, so as to procure motions, cannot cause much surprise; neither can the error of supposing the stimulus made on the membrane of the nostrils to be conveyed by the olfactory nerve instead of the fifth, according to the more correct views now entertained, be much wondered at. The omissions, also, of the action of the sphincters, muscular tone, and other *details*, impair his merit as the propounder of the *system* so little, that they need scarcely be mentioned.

68. Still it is to be borne in mind, that Prochaska mentions the *existence* of both sensory and motor nerves, though he does not state that their functions may be *proved* to belong to distinct portions of the nervous cord; the *principle* is here—its anatomical confirmation was afterwards supplied.

69. For in 1812, Sir C. Bell's discovery, before mentioned, was announced, and this being applied to Prochaska's views, fills up the omissions (except those of detail) and remedies the error; for after Prochaska had stated that impressions were carried along *sensory* and reflected into *motor* nerves, and Sir C. Bell had pointed out which *part of the nervous system was* sensory and which motor, it is evident that the mentioning of the particular nerves concerned in these various phenomena became a sufficiently easy task.

70. Legallois, also, in 1812*, limited more distinctly the agency or regulation of these motions to the spinal cord; for he shewed that the respiratory movements continued after the removal of all the brain; and these are only excited motions, as stated even by Whytt†, so that Sir C. Bell certainly appears to have retrograded when he invented the respiratory tract and its properties. Legallois also shewed the *divisibility* of the spinal cord and its functions, but he erred in supposing the experimental phenomena to be dependent on sensation and volition.

71. The researches and experiments of Mayo, with the inferences deduced from them‡; those also of Flourens, distinguishing *excitabilité* from sensation and volition, with the assignment of this function to the medulla spinalis and medulla oblongata, as high as the tubercula quadrigemina, &c., although much less explicit and clear than the views of Prochaska, are very nearly allied to the doctrine claimed by Dr. Hall so exclusively for himself.

72. Professor Müller has also, independently of, though rather later than, Dr. Hall, made similar investigations, and, by a singular coincidence, has adopted the same term in designating the function, just as Dr. Hall had adopted that previously used by Prochaska. With regard to Müller's opinions, however, Dr. Hall would appear to be rather too decisive in stating that the former refers "the phenomena in question to *sensation*, and includes the *brain* amongst the central organs of the *reflex function**;" for what can Müller mean by the following passage, if he does not intend to state that *sensation* may be *absent*, and the motions occur without the presence of the brain?

"We think, also, that the *reflex functions* which occur on irritating the skin *after loss of the brain*, afford no proof that the cutaneous irritation can excite true *sensation* in the spinal marrow," &c.—"Also in health there are many reflex motions, arising from irritations, *of which we are not conscious as real sensations*, but which may, nevertheless, produce strong impressions on the spinal marrow; as, for instance, the continued contraction of the sphincters, from the irritation of the excrement and urine†."

73. In turning now to the account of the views and discoveries of Dr. Hall, in his "Memoirs on the Nervous System," I am happy to say, that he has done full justice to himself in stating his claims specifically and distinctly; for however he may have overlooked the "opinions of former physiologists," he has taken care that no one who reads his work shall fall into the same error with regard to his own.

74. There is one point, however, in which he has not treated himself with so much justice, for he acknowledges his ignorance of many things which were known to others—that is, he states that certain opinions had never been entertained until *he* published them; whereas, in fact, they were known for half a century before: and here, again, I must, at the risk of tediousness, make a second recapitulation, and compare it with certain passages in Dr. Hall's work; which comparison will illustrate, in a remarkable manner, the sentiment just expressed, and is undertaken in order (in the words of Dr. Hall) to "avoid misapprehension."

Recapitulation of Facts, &c. stated in this paper.

1. Many of the facts which depend on the reflex function have been long known. § 5.

2. And these facts *have not been confined* to the excited actions seen in the *limbs* (§ 5); whilst they *have been distinguished* from *sensation*, and *volition*, and *instinct*. §§ 32 to 36.

Recapitulation of Dr. M. Hall‡.

"1. Many of the facts which depend upon the reflex function have long been known to physiologists.

"2. But these facts *only extend* to the excited actions of the reflex functions seen in the *limbs;* and even they have been *erroneously ascribed* to *sensation*, and *volition*, or *instinct*.

* Expériences sur la principe de la vie.
† Vide § 13
‡ Vide Anat. Comment. 1023.

* Vide Mem. on Nervous System, p. 59.
† Handbuch der Physiologie, p. 698.
‡ Vide Memoirs on the Nervous System, p. 38.

3. The excited movements of the *larynx*, *ejaculators*, &c., and of the *pannicula carnosa*, have been shewn to be of the *same kind*, and *dependent* on the *same function* of the nervous system. §§ 13, 28, 34, 35.

4. It *has* also been shewn that *this principle morbidly affected* causes the *convulsions of infants*, *epileptics*, *chorea*, *tremors*, &c. &c. §§ 37, 40.

5. Also, that the *same* individual *function* may be *augmented or diminished*, the latter *by certain poisons*. § 22.

6. And this *series* of facts *has* been *associated*, and shewn to belong to the *same order*. §§ 26 to 38.

7. The *principle* of action through this series of facts *has been shewn* to be *excited* and *reflex*—to be essentially *connected* with *certain portions* of the medulla oblongata and medulla spinalis—and to be *independent of the brain*. §§ 26, 27, 28, 70, 71.

8. It *has also* been *distinguished* from the other *sources* of muscular motion existing in the animal economy, viz. *volition*, &c.; and it has been shewn to constitute the principle of *life* in the *fœtal state*. §§ 32, 33, 34, 41.

9. The reflex function appears, in a word, to be the *complement* of the functions of the nervous system hitherto known.

"3. The facts of excited movements of *the larynx*, pharynx, sphincters, and *ejaculators*, and of the *panniculus carnosus*, have *not been* shewn to be *allied* to these, and to be *dependent* on the *same individual function* of the nervous system:

"4. And *certainly not* [been shewn] that this *principle morbidly affected* constitutes, in its different forms, the diseases arising from *dentition*, and the diseases termed tetanus, hydrophobia, *chorea*, paralysis agitans, certain forms of *epilepsy*, of *tremor*, of asthma, &c. &c.:

"5. Nor that the *same* individual *function* is *augmented* or *diminished* to a fatal degree by *certain poisons*.

"6. This series of facts *has not* been duly *associated* and shewn to belong to *one* particular *order*.

"7. The *principle* of action in this order of facts *has not* been demonstrated to be at once *excited* and *reflex* in its operations—to be essentially *connected* with corresponding portions of the *medulla oblongata* and *medulla spinalis*—and *to be independent of the brain*:

"8. *Nor* has it been clearly distinguished from the other sources and principles of muscular motion existing in the animal economy, viz. *volition*, the motive influence of respiration, and irritability, or shewn to constitute with irritability the principle of *life* in the *fœtal state*.

"9. The reflex function appears, in a word, to be the *complement* of the functions of the nervous system hitherto known.

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75. In this recapitulation it is singular that we set out with the same statement, and arrive at the same conclusion, but through two very different ways, though so nearly resembling each other are the *words*, that the addition of three letters *not* to the one, or the removal of them from the other, would make them nearly identical.

76. It may be asked why I have shown the correspondence of Prochaska with *Dr. Hall*, instead of any other writer on the subject (Müller, for instance), who has overlooked these things as much as the author of the Memoirs on the Nervous System. I have done it for the following reasons:—1st, Because Dr. Hall has given the fullest details of the facts and theory, and therefore the comparison was more *convenient*. 2ndly, Because he has stated clearly what others *had not* done, and what he himself *has* done, and therefore the comparison was more *easy*. 3dly, Because he requests the reader to search the opinions of others, compare them with his (Dr. Hall's) own, and judge for himself, and therefore the comparison was the more *just*. 4thly, Because he has given a history of the "opinions of former physiologists," from which the most important of those opinions are omitted, and therefore the comparison was the more *requisite*. And, lastly, Because he may perhaps find a reference to some work or author, the examination of which will assist him in the composition of the History of the Nervous System, which he is about to publish.

77. I can scarcely apologize for the length of this paper, as I hope the importance of the subject may be pleaded as a justification; but I must beg that its faults and errors may be excused or overlooked, for the sake of the intention. I have written it because it appears to me that we cannot be too strict in awarding to every individual the credit to which he may be entitled by the facts which his industry has accumulated, or the laws which his ingenuity has inferred from them; whilst, apart from this *suum cuique*, an imperfect history deprives the mind of the pleasure which it feels in tracing each principle from its origin to its end—from the first shadowing forth of the *idea*, to the latest detail added to the

system—from the mention of a few facts referrible to the same cause, to the announcement of a law so perfect that exceptions only prove its accuracy, and so important that its abrogation would be coincident with the cessation of our own existence. My hope is, that others better qualified may assume the task: if I have drawn the attention of such to the subject, I am satisfied; although my present conclusions are not altogether pleasing, for I am convinced that if *more* had been *read, less* would have been *discovered.*

CLINICAL OBSERVATIONS ON THE PRESENT TENDENCY TO PHLEBITIS; *Particularly in Young Women.* WITH SOME REMARKS ON ANASARCA.

By John Wilson, M.D.,
Physician to the Middlesex Hospital.

On the 9th of January we admitted three young women whose complaints began with pain in the forehead, which descended down the back, and affected the lower extremities, particularly the knees and ankles. At the time of admission we thought them to have been cases of rheumatism; but at the same time we observed that it was singular that all of them should have been similarly attacked. On examining them in bed, next day, previously to prescribing for them, we found that they complained greatly of pain in the knees and ankles, but without redness or swelling, yet very sensitive to the slightest touch, with more or less of pain up the legs and thighs. From that period to the end of the month (three weeks) we admitted three similar cases, and two others occurred in the wards. Of these eight cases, five had slight redness round the legs from the ankles up to the calf; this redness, with pain, had existed from a few days to a month, and generally the tenderness was the greatest up the inside of the leg and thigh to the groin. In some in whom the pain has been most severe, the swelling in the legs and thighs has been the greatest, but this was not always so: in some, the slightest touch with the tip of the finger at the groin, about the junction of the saphæna with the femoral vein, caused great distress. The seat of pain next in severity was down the inside of the thigh and leg, along the course of the saphæna major, or deeper along the femoral sheath. The redness or erythema on the legs was not, like that of erythema nodosum, in patches, but it was less red and more uniform, extending round the lower part of the leg, more like that of scarlatina, and as the redness subsided the cuticle desquamated; but even then the increased sensibility to the touch, round the leg, did not subside for some time after, and often returned again without the redness. In one, the calf was not red, but white, tense, shining, and much swollen, like phlegmasia dolens alba, with great pain extending up the inside of the thigh to the groin. In three, the cord-like feel of the vein was noticed, but others could not bear pressure, particularly along the femoral sheath, to ascertain the state of the veins.

Of these eight, all were young; only two were above twenty-two; all were unmarried; catamenia regular in all, but in four scanty; two had leucorrhœa; some had the appearance of good health; some were pale, and laboured under much debility; almost all had black eyes.

In the beginning of 1832 we had several cases of a like nature among young women, arising from unknown causes (idiopathic): it was then also noticed that almost, if not all, had black eyes. At that time we lost a case, when, on examination, the external, internal, and common iliac veins were found to be completely filled up with fibrinous and somewhat curdy-like substance, so as to have rendered these veins impervious to the circulation; the fluid pressed out seemed to be pus and blood mixed; the coats of these veins were much thickened, and the leg and thigh much swollen and tense. It may also be worthy of observation, that this case, which happened in February, we published in the Medical and Physical Journal for April, 1832, and at the same time also an account of a spotted fever which had been prevailing for some months before, and that periodical then made the first announcement of the Asiatic cholera in London. All the present cases occurred at the very time that a spotted fever prevailed still more generally, and its extent and severity have been great in all the London hospitals. It may also be noticed that while many of these cases were under treatment, phlebitis occurred in other wards of this hospital, which our col-

league, Mr. Arnott, lately stated at the Royal Med. and Chir. Society to have terminated fatally in five cases during four or five weeks in the surgical wards taken collectively; in four of these cases pus was found in the veins; in the other, the coats of the veins were thickened and adherent. These patients were all in separate wards, and one of the cases (Antry's) we attended, with our colleague, Mr. Tuson, after amputation of the leg. For some time after the operation all seemed to be going on well, when suddenly pleuro-pneumonic symptoms came on, he sank rapidly, and died 11th December.

Examination.—Great effusion of sero-purulent fluid into the right side of the chest, with much pleuritic inflammation; the costal pleura covered with a thick layer of lymph; purulent deposits in various parts of this lung, particularly about the posterior and inferior part, with engorgement; pus was found also in the veins of this lung: the left pleura and lung were healthy in structure, but the lung was gorged, excepting at the lower edge of the lower lobe, where there was a large emphysematous mass, the size of the fist. The pericardium was full of semi-purulent fluid. The femoral and external iliac veins of the stump were highly inflamed, and obstructed in parts by lymph and clots of blood, with pus in some places; this appearance extended as high as the internal iliac, beyond which no marks of inflammation were seen.

We have frequently observed the traces of inflammation in the veins to terminate at the angle of junction with other large veins, and marks of inflammation to be greater there than in the part immediately below it; hence the pain is often most severe in the groin, at the junction of the saphena with the femoral vein. In this case, though the vein was obstructed, there was no swelling of the stump.

But death may take place in the more early stages of phlebitis, when only obstruction by coagula or curdy-like deposits are found, without leaving traces of purulent deposits in such organs as the lungs or liver: but mechanical obstruction alone of the veins, where nothing more may appear to exist, is not sufficient to account for death, as the case, hereafter related, of the ossification of both the common iliac veins, may prove. Then in such cases the question may be asked, what is the cause of death? But perhaps it may not be so readily answered.

The following case lately occurred under us, making the ninth; it was that of a little girl, the only female that we have yet, this time, lost of phlebitis. We have had no cases on the men's side.

Maria Edwards, aged nine, admitted 31st January. Ill a week; began with sickness, vomiting, pain in the head, and all the limbs; said to have hurt the right knee, to which leeches had been applied; abdomen sore on pressure. Bowels open by medicine.

Feb. 1st.—Some swelling and much pain over all the right knee, but no marks of external injury there, with swelling up the inside of the thigh; tongue furred white; tenderness generally over the pelvic region; slight cough. Pulse small and weak; cannot lie on the back; lies on the right side, with head down and knees up; would not speak to us; the nurse said that she complained to her of soreness over all the body when touched, but most of pain in the right knee, shoulder, and arm. She continued in that state till the day before her death, when she became delirious; passed all under her, with some blood in the motions. Purpura hæmorrhagic spots appeared over various parts of the body, which had become yellow. She died on the fifth day after admission. We inferred that here we should find phlebitis with purulent deposits.

Examination, twenty-four hours after death.—The abdominal cavity contained a considerable quantity of straw-coloured fluid; spleen large and congested; kidneys both very large, and the surface of both covered with minute hæmorrhagic spots, and which also gave to the internal structure a spotted vascular appearance. Substance of the liver, generally, tinged with bile, as well as the inner surface of its veins; on the concave part, near to the surface, several small purulent abscesses were found; gall bladder very large, distended with bile, and the neighbouring parts were much more extensively tinged with it than usual. The pleuræ costales were entirely covered with the same purpura-like hæmorrhagic spots, as well as the pericardium, and also the thoracic surface of the diaphragm. The pleuræ pulmonales were partially covered with patches of lymph, with a considerable portion of serous effusion into both ca-

vities, but in a less degrce than into the abdomen. Many small purulent deposits in various parts of the lungs, but which were mostly confined to the small pulmonary apoplectic portions not far from the surface, some of which contained several of these small purulent cavities.

Brain.—The surface below the arachnoid was highly vascular, and in addition, had numerous hæmorrhagic points generally diffused over it, and the sections of the brain shewed similar points: the lateral ventricles contained no fluid. Three small purulent deposits were found in one kidney in the centre, near to a papillary portion, and close to a coagulum, which kidney having been injected, admitted of very little red size to pass into the minute vessels of its glandular structure, and had, after being saturated with spirit, a tallow-like appearancc, with minute red spots diffused over it and through its glandular structure, very similar in appearance to some of the injected diseased kidneys of those who die of dropsy with albuminous urine*. The vena cava inferior, and the external iliac on the right side, the only veins examined, shewed no traces of disease; but having found the result of phlebitis (purulent deposits) we probably might have found disease in the venous system, had not circumstances prevented a more extensive and minute examination.

We are indebted to Mr. Tuson for the two following cases; the results of phlebitis being shewn in the first by deposition of pus in the longitudinal sinus; in the second, by ossification of the deposition within the veins.

Mary Tillbrooke, aged 18, admitted on the 9th Decembcr last, with a cut on the back of the head, and slight concussion, owing to a fall from a window. She was discharged on the 19th, complaining only of a slight headache. She was readmitted on the 22d, when she complained of loss of sight, great pain in the chest, and much difficulty in breathing; pulse weak and irregular. On examining the back part of the head, the occipital bone was denudcd to the extent of an inch over the situation of the termination of the superior longitudinal sinus. The symptoms continued to increase, and she died in twenty-four hours after admission.

Examination.—A small quantity of fluid was found in each cavity of the chest. On cutting into the lungs several abscesses were discovered containing a glairy pus, which were circumscribed and near to the surface. In the liver several similar abscesses were found, one about the size of a pheasant's egg, encysted, and near to the obtuse margin of the right lobe. From these appearances we were led to look to the head for inflammation of the veins. When the dura mater was removed, the veins of the pia mater were seen in a distended state, but without traces of inflammation. There was serum beneath the arachnoid tunic. On examining the superior longitudinal sinus, its anterior part was found healthy; but about an inch before it terminates in the lateral sinus, it was found to be inflamed, and pus situated upon its serous coat; this inflammation extended beneath the seat of the injury, along a part of the lateral sinus of the left side. The veins of the right lateral sinus were healthy, as well as the substance of the brain.

A male subject was brought into the Little Windmill-street School, which had lost the right leg below the knee, the operation having been performed apparently some years previously, as there was great hardness of the cuticle covering the lower part of the knee. Mr. Tuson dissected the parts of inguinal hernia on the right side, and upon removing the integuments, he discovered a plexus of veins, which commenced at the vena saphena in the superficial fascia, pierced the muscles, ran round the abdomen, and ended in

* The kidney of Turner, who died two months ago of diabetes mellitus, was still less permeable to injection than an albuminous dropsy kidney, and the glandular structure more pale, forming the most striking contrast with an injected kidney of a case of spotted fever, a week ago, which, from its great degree of permeability, had almost the appearance of red sealing-wax itself, for half a line in depth over the whole exterior.

In this case of diabetes the kidney was in appearance, after injection, like that of a kidney in a case of dropsy with albuminous urine, both being but little permeable to injection; but in the former disease the body becomes emaciated and greatly reduced in bulk, with great increase of urine; in the latter disease the body becomes greatly increased in bulk, with diminished quantity of urine. It may also be noticed, that the lungs in this diabetic case were in appearance quite healthy, collapsed on the chest being opened, and free from tubercles; but he was carried off by chronic dysentery, and all the lining membrane of the rectum, and some of the colon, had the appearance of the bark of an oak-tree spotted with grey lichen, as nearly as we can describe it. It has so happened that we have had three cases of diabetes mellitus during the last twelve months, and two at the same time.

the emulgent vein. A similar appearance presented itself on the opposite side. On examining the deep-seated veins, the external iliacs, the termination of the internal iliacs, and the common iliacs, were discovered to be completely ossified—not the coats of the veins, but the whole internal adventitious deposit, so as to cause a complete obliteration of the veins already mentioned. Here it was presumed that, after amputation had been performed, phlebitis had occurred, and terminated by a deposit of fibrin in these veins, which by time had become ossified, the superficial plexus of veins returning the blood to the inferior cava. The preparation is in the museum of the Middlesex Hospital.

Mr. Tuson knows a man, now living, who has ossification of the saphena minor vein, which was formerly varicose.

Phlebitis may arise from various local injuries, as from bleeding, when it was formerly thought that the severe symptoms which sometimes followed arose from superficial twigs of nerves being cut in opening a vein. It may arise from various morbid substances being imbibed, and acting as irritants on the internal surface of the veins, by wounds, or after abrasion of the hands, or punctures, so common in dissection. Another often fatal form is, when it supervenes upon inflammation of the veins of the uterus after parturition, producing phlegmasia dolens, and one form of puerperal fever.

But phlebitis arising from unknown causes, called idiopathic, as all our women's cases are, may be fatal, and leave as marked results as in those who die from it after operations, as is here shewn by the first case, where the common external and internal iliac veins of the affected side were found impervious; and in the child's case, where the purulent deposits were found in various organs. But the following case will shew, that though it may not be fatal, yet it may leave irremedial results from permanently obstructed venous circulation.

In 1833, Eliza Tozer, aged 26, single, was admitted under us. Ill a fortnight; began with pain in the teeth, followed by pain in the bowels and back, with sickness; mouth sore from medicine.

Within a week, severe symptoms of phlebitis came on, though at the time she was using a gargle of chlorate of lime, for the salivation she suffered under when she came in. During the catamenia, and at the end of the week, the entire right leg and thigh were double the size of the left, with great tension and pain from the groin downwards, in the course of the femoral sheath. Had rigors; pulse 120, and small. An application of 32 leeches to the leg and thigh the same night, gave no relief; afterwards, as the right leg and thigh began to diminish in size and pain, the left leg and thigh became similarly affected as the right had been. At the end of a month both legs and thighs had returned somewhat to their natural shape and size; but the hips and abdomen, up to the navel, were marked by superficial vessels running in all directions, accompanied by pain and itching. At different periods she has been, and continues to be, an outpatient of the surgeon's, for swollen legs and varicose veins, with ulcerations.

A peculiarity of phlebitis is, its affecting the lower extremities alternately, as the last case shows; and another peculiarity is its progressing with and against the venous current in the same extremities, alternately, as the next case will show.

John Jackson came in, March last: three weeks before admission, trod upon an old nail, which ran by the side of his shoe into the middle of the right foot, an inch and a half deep, and caused bleeding for two hours afterwards: two days after, swelling began in the ankle, and then extended up the right leg to the thigh, with deep-seated pain. A fortnight after, the left thigh, and afterwards the leg, began to swell, with pain in the same direction as the right; afterwards the serotum became infiltrated, and abdomen enlarged. Has had a cough for four days before admission, with spitting and shortness of breath, so great that he could dress himself only with difficulty. Pulse strong and full. Never had dropsy nor palpitations.

After admission, the expectoration became streaked with blood; pain in the region of the kidneys, and the urine deeply tinged red with blood; he was cupped to ʒxvj. over the kidneys; afterwards three blisters were applied at different times, when he took large doses of squills, and the urine increased

in quantity, gradually became less tinged with blood, and ultimately clear. All the dropsy subsided, and he was discharged at the end of five weeks.

May this be looked upon as a case of phlebitis similar to some of the preceding ones, which proved fatal, and purulent deposits were found in various parts; and was its less degree of intensity, with the previous soundness of his constitution, that which prevented the like fatal termination?—for he had, subsequently to the injury of the foot, besides œdema, lesions of the lungs and kidneys.

The following is a case of what is commonly called inflammatory dropsy arising from cold; and it will be perceived how strikingly similar it is to the last case, arising from a wound in the foot.

Edward Ritson, aged 44, admitted 28th November last; ill a fortnight; began with a cold, caught while working on a railroad, with cough; difficulty in breathing. Swellings began in the legs and thighs, then in the scrotum, which is now the size of his head. The prepuce was punctured, to relieve the difficulty he had in making water. Lastly, abdomen and face became swollen; itching rash over the thighs for four days past; feet mottled blue and white; urine loaded with albumen. Has been a strong man hitherto.

December 8th.—Urine became coloured with blood; afterwards, for many days, he passed five or six quarts of urine, deeply coloured red with blood.

January 8th.—Swelling gone; urine only very slightly tinged brown with blood.

29th.—Discharged well.

These two last cases arose from different causes, the first from a wound, the second from cold, yet in what do they differ in their results? In both we have swelling of the legs, thighs, scrotum, and abdomen, with bloody urine and pulmonary embarrassment; in both, the œdema of the same character, tense and firm, approaching to what we are in the habit of calling brawny dropsy *, in contradistinction to the doughy dropsy, which is soft, and pits deeply, and is so common in obstructed cardiac circulation, and which generally begins in the lower extremities, as the ankles; whereas the inflammatory dropsy often begins in the face as early as in the inferior extremities—as the case of Miles, now under treatment, so well illustrates.

Admitted 29th January, aged 53. Ill a fortnight; began with a cold, followed by swelling of the face; three or four days after, swelling of legs began; and lastly, swelling of the abdomen. He was bled, took the saline powders, and in three weeks all the dropsy had gone. Urine showed no trace of blood nor albumen.

The two last cases but one came in with anasarca, followed by hæmaturia: was the lesion in their kidneys alike? We cannot say, having never seen a case like the first; nor do we recollect ever to have seen a case of inflammatory dropsy, in its early stage, end in death; but dropsy with purpura hæmorrhagica and hæmaturia, may have kidneys very little permeable to injection.

[To be continued.]

* In brawny dropsy the œdematous cellular tissue is firmer, more resisting, less elastic, and it is more difficult to fill up an indentation by making pressure on the immediate surrounding parts, than in doughy dropsy; also, an incision being made through the integuments after death, the parts cut recede from each other, gaping as in cutting into the blubber of the porpoise, and no fluid exudes for several minutes afterwards; so very different from an incision being made into a doughy case of dropsy, where the fluid follows the scalpel. The scrotum also is often infiltrated with a gelatinous-like fluid, which does not flow out on an incision being made; and on puncturing the scrotum in the living, when painfully distended, no relief is afforded, for none or scarcely any fluid exudes.

CLINICAL REPORTS.

By John Burne, M.D.

Physician to the Westminster Hospital.

Case I.—*Hydrophobia.—Remarkable Effect of Morphium administered endermically.*

George Cubit, age 17, a short, strong-built youth, a prisoner in the Penitentiary, Millbank, was taken ill on Friday, the 19th January, 1838, with chills creeping over the body, followed by quivering pains in the limbs and general indisposition; in which state he continued through the Saturday and Sunday. On Monday morning, when, as he was about to wash himself, his hands approached the water, he shuddered and shrunk back in alarm, being seized at the same moment with catching in the breath and constriction about

the throat. Later in the day, at dinner, he could not drink, spasms of the throat seizing him when he handed fluids to his mouth. These symptoms increased, the boy grew alarmed and apprehensive, and the disease was recognized by the resident medical officer, Mr. Wade, as hydrophobia.

Cubit had been in the Penitentiary twelve months, was by trade a tailor, and had conducted himself with propriety. It appears that as far back as September he became low-spirited and hypochondriac, full of apprehension, and could not endure to be alone at night, on which account Mr. Wade very humanely directed that he should be brought up into the wards of the Infirmary to sleep. About the 9th of October he was attacked with quotidian ague paroxysms, which were cured by quinine in the course of a week. The boy recollects having been bitten in the hip seven years ago by a large dog in a yard in Whitechapel, of which he can give no other particulars than that there was no reason to believe the dog was mad, and that no effects followed the bite. Two cats now alive were occasionally with him in his cell two months ago, but he is confident he was not bitten by either of them. He has complained to the attendants of pains about the right hip where he was bitten, and in his limbs, though the part presents no appearance, and when questioned relative to it he evades the subject.

I was called to see him at *half after ten o'clock* on *Monday night*, the first day of the hydrophobic symptoms, and the fourth of the boy's indisposition. He was sitting in bed, reclining backwards, his face rather swollen, and expressive of great anxiety and alarm. On presenting the candle to look at his tongue, he shrank from it with great emotion, had a momentary convulsive catch of the breath, and entreated me to put the light away. He complained of being giddy; of having a sense of fulness and swelling about the larynx and along the trachea: of dryness in the mouth and down the throat; of great thirst and desire to drink, but of inability to swallow, and of fear to make the attempt: he was hawking frequently and ineffectually, owing to the dryness of the throat; frequently he had strong convulsive inspirations suddenly arrested, then a considerable pause before expiration. His manner was subdued, he was very thankful to all around him for their attendance, and supplicated me to do something for his relief, saying, at the same time, that he could not swallow, and was gratified when I told him that I would prescribe a medicine which he could place upon his tongue, and suck. The tongue was white, foul, and clammy; the pulse 140, weak, the strokes distinct, though at times unequal; the skin moist, of natural temperature, and the bowels freely open.

> To take a quarter of a grain of Extract of Belladonna upon the tongue every half-hour. A narrow Blister to be applied along the upper part of the spine.

Soon after I had left the Penitentiary, Mr. White arrived, and by soothing and encouraging the boy, he induced him to drink some gruel and brandy. The night was passed with intervals of tranquillity, but without sleep. The first dose of belladonna affected the pulse in half an hour, causing it to flag and intermit; the second did the same, on which account it was not repeated more than three times in the night, and a fourth time about eight o'clock in the morning.

Tuesday, half-past nine A.M.—Mr. White and I saw him in consultation. The boy maintained the same position, had his throat exposed, that it might be free; he suffered the same throes of spasm in respiration and deglutition: was anxious to drink, but dare not, and could not; the pulse was not increased in frequency, being still 140, distinct and regular; the tongue was become brown and dry along the middle, clammy at the sides, and clammy sordes were forming upon the lips; the hands were blue, and below the natural temperature; the face dusky, and deathly pale. He said he should soon die, and often thought he was dying.

> To repeat the Belladonna as frequently as it can be borne, and an Enema of Mist. Sp. Vini Gall. ℥iv. to be administered every hour.

3 P.M.—Since the morning he has been constantly hawking, spitting, retching, and struggling, so that only one dose of the belladonna was taken into the mouth, and this not retained. Attempts were made to administer the enema, but they threw him into such desperate convulsions that they could not be persevered in.

He was now materially changed for the worse, and fast approaching dissolution, of which he was fully conscious. He was frequently eructating and retching, throwing up bile and ropy mucus. He was sweating profusely, and struggling with intense crampy spasms of the upper and lower extremities, calling and hollowing out, as the spasms seized him, for three or four persons to rub him, one to each extremity. The eyes were staring, the face was frequently and suddenly contorted, and the head twisted round to the right: at times the trunk was arched suddenly backwards, (opisthotonos) by violent spasms of the dorsal muscles; and spasms of the diaphragm interrupted the respiration almost to suffocation, so that he could speak only in broken words or sentences. He begged to have his throat rubbed, it was so painful from perpetual, distressing, ineffectual efforts to swallow. Often he desired gruel, which, when presented to him, he dare not put to his lips, but would dip his finger into it, and carry this to the roof of his mouth, afraid to approach the muscles of deglutition. He was giddy, recollected with difficulty, moaned distressingly, and altogether presented a picture of suffering and distress such as I never witnessed. The pulse could be felt, but not counted, owing to its rapidity, as well as to the spasmodic twitchings of the tendons and perpetual movements of the body.

In this state, Mr. White and I considered what could possibly be done to assuage the poor fellow's sufferings. Medicine could not be given by the mouth, not even put upon the tongue, nor by injection. I suggested the endermic use of morphium: accordingly, about *ten grains of the acetate* were sprinkled on the blistered surface along the spine, and we waited the effect. Scarcely had one minute elapsed when we observed the stare of the eyes and the dreadful alarm and anxiety of the countenance to diminish, then the violence of the spasm to abate, and the catchings in the respiration and the retching to subside; and to our astonishment this general amelioration progressed, till, in four minutes, the countenance had become placid and the respiration free; the retching had ceased, and the spasms vanished! Advantage of this repose was taken to give him some brandy and gruel, and afterwards some broth; of all of which he drank eagerly by a convulsive effort from his own hand, and said they warmed and comforted him. He then fell quietly back, as one fatigued and sleepy, lay down on the pillow on his right side, with his hand under his head and his knees drawn up in a most natural and comfortable position, free from pain and spasms. In this tranquil state we watched him for a quarter of an hour, and having shut out the light and directed the attendants to withdraw from his bed-side, in the hope that sleep might ensue, we retired; leaving instructions, that should the effect of the morphium pass away and the spasms recur, the morphium should be again administered, so as to keep him under its influence; also that a narrow blister should be applied in the region of the diaphragm, to facilitate the further use of the morphium, if requisite. So wonderful an effect from medicine I never saw. At 11 o'clock at night we visited him again. The influence produced by the morphium had continued, more or less, for three hours, when the spasms and other symptoms having recurred, the acetate of morphium was a second time applied, and a second time the symptoms yielded, though not so entirely as before, nor until nearly half an hour after the application. The influence of this second application had now passed away; the symptoms had returned, though in a mitigated form, especially the cramps of the extremities, which have scarcely troubled him since the first use of the morphium. The powers of life, however, are fast declining, the pulse is barely perceptible, and the face is growing sharp. We, nevertheless, did not think it right to abandon the case without another effort; Mr. White, therefore, raised the cuticle by passing a hot iron over wet silk laid upon the epigastrium, and to this fresh surface more morphium was applied, but in vain: the decline of the powers of life had probably rendered the body no longer susceptible. We left him about one o'clock, sinking, and he died in the course of two hours, having become outrageous and frantic some short time previous to dissolution. Death took place early on the sixth day of general indisposition, and in about forty-four hours after the supervention of the hydrophobic symptoms.

Sectio cadaveris, 36 hours after death.—The appearances on dissection

were of the usual description, and very limited. Brain and spinal cord: slight aqueous effusion underneath the arachnoid membrane and in the ventricles, and a larger quantity in the spinal sheath. The cut surfaces of the brain shewed numerous bloody points, and the membranes of the medulla spinalis were anormally vascular, without any pathological change in their organization. The pharynx was highly vascular, as was also the larynx and trachea; the lungs congested.

The other viscera presented no anormal appearance.

Case II.—*Hydrophobia.*

Thomas Chester, age 72, a watchman at the Chelsea Waterworks, was admitted into the Westminster Hospital on Friday, the 21st August, 1835, labouring under hydrophobia.

The history of this case during the period of deliteseence I found it very difficult to trace, owing to the distance of time since the bite, and the very contradictory accounts given by various individuals living in Chester's neighbourhood. Many knew the dog; one said he went mad and was shot; another, that he was drowned; and others, that he was not mad at all. The following particulars are the most correct I could obtain: — That Chester was bitten about fifteen months ago, by a dog not belonging to himself, in the middle and index fingers of the right hand; that the wounds healed, but the cicatrices remained tender, painful, and tumefied, for several weeks; that the dog remained in the neighbourhood several months, and was seen many times by Chester himself; that afterwards the dog went away from the neighbourhood with its owner; and that eventually the dog lost the use of the hind quarters, on which account he was drowned in the Thames. The evidence appeared clear as to the dog not having been rabid at any time.

On Sunday, the 16th August, 1835, Chester could not eat his supper as usual, nor could he on the Monday and Tuesday, and he complained of thirst, which he attributed to the heat of the weather. On Wednesday evening he first complained of nausea, and of the thirst having become very annoying. On Thursday morning he did not relish his breakfast, he drank but half his usual quantity of tea, and when pressed by his wife to take more, he answered that he could not drink it. At this time his breath was not affected, nor was there any sign of convulsion or agitation. After breakfast he went to bed, where he remained some hours, but did not drink any of the toast and water which was placed in a jug at his bedside. He took no dinner; and in reply to his wife's question why he had not drank the toast and water, he merely said "he could not." He went on duty as a watchman at five o'clock in the afternoon, but soon returned. At tea-time he still would not drink, though he sat quietly at the table while his wife took her tea, and manifested no inquietude at the fluids before him. His wife now went to a chemist, who gave her a powder of ten grains of scammony and calomel, which purged him violently during the night. This night was passed very restlessly; he was in and out of bed constantly. If he fell off to sleep he was disturbed by spasms and sense of suffocation. He complained, too, of itching in the hand and arm, where he was formerly bitten, the back of which he scratched very much.

It was on Friday morning at breakfast that the sight of fluids first seemed to trouble him; his wife observed that he turned away from the table, the sight of the tea inducing interruption of his breathing. At nine o'clock he was first visited by a medical man, Dr. Simpson, to whom he complained earnestly of great thirst, sickness, and retching, of frequent convulsive catchings in his breathing, and of inability to drink, which he could not account for. Dr. Simpson found the pulse weak, and varying from 48 to 56. On a cup of water being offered to him, he was seized with convulsive movements of the respiratory muscles, and a sense of suffocation; he persevered in attempts to drink, but could not succeed. The same symptoms recurred on a basin of water being presented to him, and splashing in it. He tried to suck water through a tobacco pipe, the basin being held out of his sight, yet the moment the water reached his mouth, he started back, shuddered, and became affected with spasms as before. He took three-quarters of a grain of acetate of morphium immediately, and again in a couple of hours, when steps were taken

to remove him to the hospital, where he arrived at half-after one o'clock. He was quite unconscious of the nature of his complaint, though he said once or twice that he was past recovery.

1½ P.M.—Complained of a stoppage in his breath; respiration interrupted by spasmodic catchings; the chest heaved frequently; frequently he sighed; his speech was interrupted, being unable to say many words without stopping to respire. He expressed great desire to eat and drink, but was afraid to swallow, thinking it would make his breath worse; he had great aversion to fluids, the sight of them increasing the trouble in his respiration; he would nevertheless hold a glass of water in his hand, look at it, then turn away, and remain some time before he could make up his mind to drink; this, however (after many attempts), he accomplished with much agitation.

The aspect of his face was anxious, the features rather sharp, though his manner was cheerful. The respirations 26, irregular, sometimes short, sometimes high, sometimes sighing and heaving; the pulse 50, irregular and unequal; the tongue red, and disposed to be dry; the skin perspirable; he could lie down, but preferred the semi-horizontal posture.

One minim of hydrocyanic acid on a small lump of sugar immediately; one grain of the extract of belladonna every hour; and a blister to the throat.

4½ P.M.—Respiration had become more embarrassed and convulsed: he gulped much, and made frequent efforts to vomit. The spasmodic catchings in the respiration were induced by a current of air on his face; he was growing restless, his spirits still good. The voice was becoming husky, the breath exhaled the febrile odour; the respirations 40; the pulse 60, weaker; skin clammy; purple flush on the cheeks; the belly was becoming flatulent; the epigastrium tender on heavy pressure; and he had a sense of constriction in the region of the diaphragm.

9 P.M.—Becoming excessively restless and violently delirious. Pulse 160; skin perspiring, warm, and not clammy; respiration more frequent, irregular, and interrupted; pupils dilated; eyes suffused; face flushed purplish red; frequent efforts to vomit: his spirits still good. On being asked if he was thirsty, and what he would like to drink, he replied, "a little drop of water;" but when it was presented to him, he begged to be excused, saying, "not just now, it will throw me into a sweat." It was no longer practicable to administer the belladonna by the mouth.

A Belladonna Plaster to the throat; an enema of Elaterium, gr. iii. immediately; and one also of Extract of Belladonna, gr. ii., every other hour.

10 P.M.—Talked constantly, deliriously, became boisterous and unruly, raved out "murder! police!" said he should be suffocated; got out of bed suddenly, walked boldly and vigorously, resisted all attempts to lead him back, and shewed strength, which it required four or five persons to control; his eyes were very wild, and moving quickly from side to side; the face blueish, and highly suffused and congested.

On being offered a cup of water, which he said he should like, he held it in his hand, looked at it askance, then turned away his head with horror and shuddering. He put his hand into a basin of water without hesitation, but took it out hastily.

11 P.M.—Strength much diminished within the last hour; he no longer raved; the pulse 160; utterance difficult; frequent suffocating cough; hiccup, and eructation of frothy coffee-coloured fluid; the respiration more embarrassed; inspiration short, laboured; expiration prolonged by convulsive cough; lower extremities cold to the ankles, while there was great heat of the trunk; the temples throbbed; the pupils were dilated, but contractile. He seems sensible to questions, but no longer answers, owing to the embarrassment of the respiration.

11¼ P.M.—His face has become livid; eyes glassy; extremities cold; the faec sharp, and features contracted. At twenty minutes after eleven o'clock he made a strong inspiratory effort, frothed at the mouth, and expired; being the second day of the hydrophobic symptoms, and the sixth day of general indisposition.

Sectio cadaveris 13½ hours after death.—Body muscular, and rather fat; the upper extremities flaccid, the lower rigid. The posterior parts of the trunk

and extremities, as also the whole head, the right side particularly, to which the head inclined, of deep purple, from gravitation of blood, hypostasis. The abdomen rather tympanitic.

In dissecting back the scalp, black blood flowed from the cut vessels, and the longitudinal sinus having been wounded in sawing through the occipital bone, black blood flowed in a stream, to the amount of a pound or upwards.

Brain and nerves.—The surface of the convolutions normal, both as to general aspect and vascularity. In the sections of the brain very numerous bloody points were seen in the medullary substance, the consistence of which was normal; the ventricles contained about three drachms of aqueous fluid; and fluid of the same kind, coloured with blood, was found in the hollow of the base of the cranium after the brain had been removed.

The pneumo-gastric nerves, examined from their origin in the medulla oblongata to the pulmonary plexus and to the stomach, presented no morbid appearance. The superior cervical ganglion of the sympathetic was supposed to be of a deeper reddish brown colour than usual; but its organization, as also that of the filaments proceeding from it, was healthy.

The pharynx, larynx, trachea, and anterior and posterior surfaces of the epiglottis, the velum palati, uvula, pillars of the fauces, and surface of the tongue were excessively vascular, giving a dark purple colour to their mucous membrane, without any change in the organization of this membrane, or any morbid secretion, as the product of inflammation: it was mere vascularity. The vascularity of the pharynx was not continued to the œsophagus, while the vascularity of the larynx and trachea extended along the minute ramifications of the bronchi. The lungs were excessively congested and not collapsed; the heart soft, flabby, and lacerable. The stomach, and whole tract of the alimentary canal, presented no abnormal appearance. The ganglia and plexuses of the organic nervous system of the abdomen healthy. The blood throughout the body black, thick, and not coagulated.

24, Lower Brook-street,
30th March, 1838.

REMARKS ON LENTICULAR GLAUCOMA.

By William Mackenzie, M.D.
Surgeon-Oculist to the Queen, for Scotland.

Professor Rosas, in his last work on Diseases of the Eye (*Lehre von den Augenkrankheiten*, Wien, 1834, p. 326), distinguishes three kinds of glaucoma, viz. glaucoma of the hyaloid, glaucoma of the retina, and glaucoma of the choroid. He makes no mention of lenticular glaucoma, which is much more common than any of the three which he particularizes. Indeed, vitreous, retinal, and choroidal glaucomata, are, to say the least of them, rare diseases.

Professor Jüngken, in his Treatise on Diseases of the Eye (*Lehre von den Augenkrankheiten*, Berlin, 1836, p. 565), states glaucoma to be a cloudiness of the vitreous body, caused by exudation, the product of chronic inflammation of the hyaloid membrane. He states, that the retina always suffers along with the vitreous body, and hence, he says, arises the concomitant appearance of amaurosis with glaucoma. He says, glaucoma is characterized generally by a grey-greenish sea-coloured cloudiness in the bottom of the eye, remote from the pupil, and appearing concave. He notices no other variety of the disease but one, and never hints at any other seat of the complaint than the vitreous body.

Reference might be made to the writings of other eminent oculists, to prove that the well-known appearances in glaucoma are not referred in general to the lens, but to the vitreous humour, retina, or choroid, and especially to the first of these three textures.

In a former paper on Glaucoma (Glasgow Medical Journal, 1830, vol. iii, p. 254), I did not express so confidently as I am now disposed to do, my opinion, that by far the greater number of glaucomata depend essentially on the state of the lens. On the contrary, I was more disposed at that time to consider glaucoma as in a great measure a reflection from the choroid coat, although even then I ascribed the greenish hue of the reflected light to the action of the amber-coloured lens.

In the second edition of my Practical Treatise on the Diseases of the Eye,

I stated (p. 827), that to the change in the lens to an amber or reddish-brown colour, especially towards its centre (this change of colour being in some cases so deep as considerably to impair the transparency of the lens), I was inclined chiefly to attribute the appearance of the eye in glaucoma. In confirmation of this view I stated, from actual observation, that if the lens were removed from a glaucomatous eye, or if it happened to sink, as it sometimes does, to the bottom of a dissolved vitreous humour, the glaucomatous appearance was lost. I mentioned that the lens, viewed *in sitû*, seems, in glaucoma, of a greenish, and sometimes of a deep sea-green, colour; but that if we extracted it, the greenness formerly seen in the eye was no longer to be discerned, while the extracted lens, on being viewed by transmitted light, presented a deep amber colour.

It is well known that various substances in nature present a different colour, according as they are seen by reflection or by refraction; and the glaucomatous lens appears to be one of them. Seen by reflection, it is green; seen by refraction, it is amber, or reddish-brown; exactly as the infusion of *lignum nephriticum*, if held between the light and the eye, appears of a golden or reddish colour, but if held from the light, so that the eye be between the light and the phial, it will appear of a blue colour (Boyle's Experiments and Considerations touching Colours, pp. 199, 216, London, 1670); or as the purpurate of ammonia, when viewed by transmitted light, is of a deep-red colour, while, by reflected light, the two broadest opposite faces of the crystals of that salt appear of a brilliant green (Phil. Trans. for 1818, p. 423).

I am inclined to think that there is never any very distinct glaucomatous appearance (that is to say, cloudiness of a greenish hue), except what is caused by the amber or reddish-brown colour of the central and posterior laminæ of the lens. In lenticular glaucoma, the lens has become, in a certain sense, dichromatic, being of a deep amber when allowed to transmit the light, but appearing green by reflected light; the green hue being probably the result of the absorption of the extreme prismatic rays of the light entering the eye, while the middle prismatic rays are but little affected.

All who begin to examine diseased eyes, find considerable difficulty in distinguishing lenticular glaucoma from lenticular cataract; but a little experience generally serves to make them acquainted, more accurately than any verbal description can, with the diagnostic appearances of these two diseases. No mere cataract is green; the cloudiness in glaucoma is considerably remote from the pupil; and though sometimes the cloudiness is limited, and surrounded by a lucid ring, while in other cases it extends almost across the breadth of the lens, yet it is always evident that the superficial laminæ of the lens are transparent in simple lenticular glaucoma. The greenish cloudy surface is always uniform, smooth, and as if polished, never streaked, spotted, or apparently rough, as is generally the case in lenticular cataract. The shadow thrown by the iris on the greenish cloudy surface is much broader than the shadow thrown upon a lenticular cataract.

The reddish-brown colour upon which lenticular glaucoma depends, affects only the internal and posterior laminæ, and fades away into an amber hue towards the surfaces, and especially the anterior surface and circumference of the lens. These, so long as the disease is one of simple lenticular glaucoma, have lost comparatively little of their natural transparency; but the reddish-brown part often presents, on making a section of the extracted lens, a peculiar dryness of substance, as well as a considerable degree of opacity.

After lenticular glaucoma has existed for a time, the surfaces of the lens may become coagulated and opaque, so as to constitute a complication of glaucoma with cataract. This sometimes occurs very suddenly.

When I have expressed my opinion, that the appearances in glaucoma depended on the state of the lens, and that the apparent depth of the greenish opacity was a deception, I have sometimes had difficulty in convincing those whom I addressed that this was the fact. They still conceived the cause to reside at the bottom of the eye. It is well known, however, that similar deceptions sometimes take place in other diseases. Thus, Panizza (*Sul Fungo Midollare dell' Occhio*, Pavia, 1826, p. 10), relates a case of fungus hæmatodes of the eye, in which the tumor was found touching the posterior surface of the lens, although,

before dissection, it appeared as if at the bottom of the eye. "The distance, then, of the spot," says Panizza, "was an optical illusion, owing to the presence of the lens; and, indeed, on again replacing the lens, the spot again appeared at a great distance, an illusory distance, which must have been increased by the aqueous humour and transparent cornea."

It is evident, from the silence of Rosas, Jüngken, and others, respecting the state of the lens in glaucoma (as well as from the explicit statement of the first of these authors, that the disease affects the vitreous body, retina, or choroid), that they do not consider the lens as particularly the seat of this disease, or its transparency to be in any way affected in glaucoma. A very simple experiment serves, however, to show that in this disease the transparency of the lens is always more or less implicated, and in many cases greatly lessened.

It was a notion communicated by Mr. Ramsden to Sir E. Home (Phil. Trans. for 1795, p. 3), that the difference in density between the continuous media in the eye was so very small, that refraction might take place without reflection. "This appears," says Sir Everard, "to be the state of the eye; for although we have two surfaces of the aqueous, two of the crystalline, and two of the vitreous humour, yet we have only one reflected image, and that being from the anterior surface of the cornea, there can be no surface to reflect it back, and dilute an image on the retina." This notion is incorrect; there is a reflection both from the anterior and the posterior surface of the crystalline body; a fact, the application of which to the diagnosis of eye-diseases we owe, I understand, to Professor Sanson, sen., of Paris. From the posterior surface of the crystalline, or perhaps rather from the concave surface of the posterior capsule, there is seen reflected a minute inverted image of the candle held in the hand of the observer. If the candle is moved to the right, the image is seen to shift to the left; if the candle is elevated, the inverted image is seen to descend; and *vicibus versis*. Besides the erect image from the cornea, there is a second erect image of the candle from the anterior surface of the crystalline body. It is not nearly so sharp as the inverted image from the posterior surface. For a very evident reason, it appears behind the inverted image, and being formed by the segment of a larger sphere it appears larger. It is even larger than the erect image from the cornea, being magnified by the aqueous humour, through which we see it.

As soon as I was made acquainted, by my friend Dr. Staberoh, of Berlin, with this ingenious method of examining the eye catoptrically, I hastened to apply it to the confirmation or refutation of the opinion I had long held, of the lenticular seat of glaucoma. I trust we shall ere long be favoured with the results of Dr. Staberoh's numerous and careful examinations of diseased eyes, in the manner suggested by Professor Sanson, sen. In the meantime I am satisfied of the truth of the following observations, which I have frequently repeated. The conditions under which they succeed best are, that the pupil be previously dilated by belladonna; the observer and patient placed in moderate daylight; the back of the patient turned towards the window; the patient seated so that the observer looks rather down into the eye than upwards; and a candle used which burns steadily, and does not blaze much.

On reading over the following particulars to Dr. Staberoh, he was kind enough to favour me with a few annotations, which I consider too valuable to be lost, and which I therefore subjoin under their respective heads:—

1. In incipient lenticular glaucoma, or what we may call the first degree of that disease, both the deep erect image, and the inverted one, are distinct.

[*While its outline remains pretty sharp, the deep erect image is rather larger in size, and brighter than in the healthy eye. It is also somewhat of a yellowish hue. With the increase of glaucoma the inverted image becomes larger, and more of a yellowish colour; its outline becomes sooner diffused than that of the deep erect image.*

In estimating the changes which are observed to occur in the appearances of the images reflected from the eye in its several diseased states, it is necessary to take into account two sources of these changes, viz. the state of the surfaces which form the images, and that of the

media through which we see them. Each of these causes must have an effect, more or less remarkable, in different cases of disease.—J. S.]

2. In mean cases, or what we may call the second degree of glaucoma, the inverted image is pretty distinct, when formed near the circumferential part of the crystalline; that is to say, if the candle is moved by the observer towards the right side of the patient, while the right eye is the subject of examination, the inverted image will be seen behind the nasal edge of the pupil. If the observer now brings the candle slowly in front of the eye, the inverted image, as it moves across the pupil, is seen to become less and less distinct, and in some cases is altogether extinguished; but as the candle approaches the patient's left side, it reappears behind the temporal edge of the pupil, being again formed by the circumferential portion of the posterior capsule. No such appearance as this is seen in lenticular cataract, a disease which always affects the superficial laminæ of the lens in such a way as to prevent the formation of the inverted image by any part of the posterior surface of the crystalline body. The extinction of the inverted image, when the candle is placed directly before the pupil, is evidently owing to loss of transparency in that portion of the lens, which, in lenticular glaucoma, suffers a peculiar degeneration, characterized by dryness of substance and a reddish-brown colour.

[*In moderately developed glaucoma, both images of the candle are represented by yellowish spots, or blazes, of a pretty bright appearance, following the motions of the candle in their corresponding directions.*—J. S.]

3. In complete lenticular glaucoma, or glaucoma of the third degree, the inverted image is no longer visible even at the edge of the lens.

4. The deep erect image is better seen in the second and third degrees of glaucoma than in the healthy eye. It is large and evident, but its outline is not sharp; so that it often appears like a diffused blaze. It is best seen when the eye is looked at downwards, and from one side. The fact that it is more distinct than in the healthy eye, is to be attributed to the reddish-brown part of the lens serving as a foil to the image.

[*In the far-advanced stage of glaucoma, both images disappear entirely; but I am not sure whether, in this case, there is no complication with cataract commencing.*—J. S.]

5. In lenticular cataract, no inverted image is visible; while, from the anterior capsule, there is merely a general reflection, but no distinct image.

[*In incipient lenticular cataract, the inverted image becomes indistinct, and its outline as if washed off. It is changed neither in colour nor in size. It is extinguished long before the cataract is fully developed. In capsulo-lenticular cataract, the inverted image fades much sooner than in mere lenticular cataract, and even when the capsule, or the peripheric substance of the lens, seems to be alone opaque, the image disappears much sooner than we should expect, from the apparently moderate degree of opacity.*—J. S.]

6. If the crystalline lens has been removed by operation, neither the inverted nor the deep erect image is visible.

The catoptrical examination of the eye confirms, in the most satisfactory manner, the doctrine that glaucoma is, in general, an affection of the crystalline lens. Concerning this disease, Rufus and Galen were right seventeen hundred years ago; distinguishing by the name of γλαυκώματα those internal opacities which they found to be incurable, while, on the more favourable, they bestowed the name of ὑπόχυματα. The former they believed to depend on a change of colour and consistence in the crystalline lens, an opinion from which the moderns have erroneously departed; while they attributed the latter to the accumulation of a new substance suffused between the iris and the crystalline—a notion which the moderns have successfully corrected.

Lenticular glaucoma may be considered as a nebula of the lens, while cataract may be compared to an albugo, or leucoma. Glaucoma, however, is in the centre, cataract is on the surface of the crystalline; and while the former is rarely, the latter is generally, unattended with amaurosis.

George Square, Glasgow,
March 30, 1838.

THE LEICESTERSHIRE TWINS.

To the Editor of the Medical Gazette.

Sir,

I briefly subjoin for your perusal, and if you think proper for your insertion in the Medical Gazette, a description of an anomalous and very unusual structure, which, for want of a better, may be designated "The Leicestershire Twins." I was called to see it on Sunday last, April 1st, in conjunction with Mr. Wright, of Mount Sorrel: we examined it together, and the following is the substance of our notes. The children were premature, about seven months old; they were in general well formed. From the midwife's statement, and from the congested state of the lower extremities, it appears that the feet and legs of one child, and one leg and foot of the other, presented, and were born first; then the breech and the other leg of one child bent on it; the heads were placed sideways in relation to each other, and were very much pressed together opposite their temporal bones. The children lay front to front; one side of each infant, united to the side opposite by a linea alba, formed the base; the other side of each, in the same way united, formed the uppermost surface. The point of junction may be described by a line extending from the third rib anteriorly at the angle where the cartilage would have joined the bone downwards on each side, and for the same width, to the umbilicus, which, as usual in seven months' fœtuses, was lower considerably than the centre of their longitudinal line. The antero-posterior breadth of the two thus united was at least double the breadth through the ribs or abdomen of either individual crossways. They lay like babes in the wood, encircled in the attitude of love; the arms were pressed a little upwards during the birth, but they soon resumed the posture in which perhaps they had been gradually moulded; one had both arms round the neck of his brother; the other had one arm round his brother's neck, and the other round the left loin. They were both males, and both born dead; from the state of the lungs, they had never breathed.

Their internal organization was of yet more importance. Each infant had a pair of lungs, a thymus gland, genito-urinary apparatus, alike, and perfect; but they had only one heart, one stomach, one spleen, one set of bowels, one liver, one anus, although the children were as separate at the sides of the abdomen as any children can be, and although each child had the ordinary antero-spinal depth in the abdominal cavity when they were ultimately separated from each other. They had no sternum; the ribs of one on each side arching forwards, and uniting on each side to the ribs of the other. The heart was considerably above its natural size, and perfectly well formed, but much higher than usual. It was placed at the point formed by the convergence of the two thoraces, and would have been felt, had the subject lived, by placing the finger at their point of junction superiorly a few lines below the summit of the ordinary sternum. It was evidently common property, part being in each; and, viewing the mass lying on its sides as one individual, the heart presented the same direction as in the perfect subject, lying obliquely, its basis posteriorly and superiorly, its apex anteriorly, and inclining outwards and downwards. There was a short common aorta, from which to each subject issued an aorta taking the usual course and distribution. They had but one cord, and but one placenta. Each of the umbilical veins arose by a branch from each subject, and had the ordinary termination. The hypogastric arteries, traced backwards, accompanied the umbilical veins for some distance within the abdomen, where, abruptly leaving them, they were seen to have a high insertion into the aorta; so that had the subjects lived, neither of them could have had a urachus. There was but one liver, filling the right hypochondrium of one fœtus, traversing the space of junction under the linea alba, also part of the left hypochondrium of the other; being, like the heart, partially in each subject, and like the heart also much above even the fœtal bulk. The liver was lobed as usual; Glisson's capsule and its contents, the gall-bladder and its appendages, were all distinctly traced. The abdominal cavity was unusually large, encroaching on the thorax. The ribs approximated so as to diminish the depth of the chest, and the diaphragmatic attachments were unusually high.

We cannot describe the abdominal viscera without separately designating

the children by some letter, say A and B. A had no stomach, no spleen, no duodenum, no jejunum, no ileum; his œsophagus was a *cul-de-sac*, terminating at the tendinous fibres of the diaphragm; A had, however, all the large intestines. The cæcum, with its processus vermiformis, began one inch above the level of the umbilicus, and lay a little above the left kidney of A. The colon then arose, and first crossing to the ordinary position of the cæcum, curved, and ascended to the right hypochondrium, arched along the empty epigastrium, descended and formed the rectum, which, distended with meconium, terminated in an imperforate anus. The sigmoid flexure of the colon communicated by a loop of intestine, about a span long, with the jejunum of B. B had a regularly formed œsophagus, a stomach one inch long, and half an inch wide, situated in its usual position, and with its usual attachments; it had, however, two pylori, and for two inches downwards two intestinal tubes, which, however, at this point united into one, forming the duodenum, which took its ordinary course. We now traced the jejunum, and could observe no peculiarity until we had advanced one yard downwards, where the jejunum divaricated at an acute angle; the loop already described abruptly crossing into the left umbilical region of A, and entering the cæcum, the main tube continuing its course for one yard and nearly two feet, attached to a short mesentery, very much convoluted, and therefore retaining the ordinary position of the small intestines. We traced the mass downwards to the anus, which was perforate. We cannot call any part of the intestines of B by any other term than small intestines, not merely on account of their size, which corresponded throughout — not merely on account of their position and structure, but still more from the absence of the fæcal odour in their contents, which, when compared with the intestines of A, would have appeared to any one satisfactory. It is right, however, to notice, that B had, about two inches below the origin of his intestinal communication with A, a distinct processus vermiformis; but there was no other visible alteration, except the gradually increasing density characterizing the ileum. B had no spleen. In both children the thyroid glands were unusually large. In A the urinary bladder was unusually distended. The children were in other respects, as far as examined, remarkably well made.

I have thought it my duty, sir, to forward this description on two accounts: I say nothing of the value of published facts in the frequent and unexpected illustration of what is unknown; still less am I actuated by the mountebank interest of mere novelty. The case strongly confirms the subsidiary character of the spleen in some way as an appendage to the stomach. A had no stomach, no spleen; B had a stomach and a spleen. The case also contains some of those peculiar facts which have given rise to the science of morphology. Our structures have a tendency to assume certain positions, and even in their aberrations may be proved some effort to obey their ordinary laws, and a frequent resemblance to aberrations already known. By examining the ascending scale of creation on the one hand, and the anomalies of man alone on the other, very much has been done towards the elucidation of dark points in human anatomy. I am partly logical in my observations, for the statement just made respecting the spleen may fairly be adduced as an illustration of my present observation. The position of the heart and of the liver, common to the two, was equally remarkable. Could the reader imagine the upper surface of the twins united to be the upper surface of a common subject, and presume the heart and liver to be of their usual form, he would have an idea of their exact position in the "Leicestershire twins." They were "quoad hæc," as far as these two organs and their appendages were concerned, but one creature: the organs were sufficiently bulky for this large existence, and were placed as though in one body. The stomach belonged to B alone. The liver and heart, or some subsidiary contrivance (as, for example, the extreme contractility of the arteries in the frog, which has no heart), are necessary for animal life. The stomach is not an indispensible requisite, as we see in the coral insects, who, as A must have done had he lived, vomit what they eat. The manner in which the large bowels curved in A, the manner in which the small bowels convoluted on the mesentery in B, each taking its usual form, position, and attachments, and leaving

in each child a place for the absent bowels unoccupied, are phenomena equally remarkable. One point of resemblance between the "Leicestershire twins" and the Siamese twins, is also worth notice. The junction in each began at the sternum, but was much more decided in our subject than in the monster that has attracted the attention of the entire thinking world. It is quite possible that some future day shall reveal a yet closer resemblance, by a comparison of the interior organization of both. Who can tell what important enteric, or vascular, or nervous connexion, may render them as mutually dependent on each other for life itself as the Leicestershire twins? In separating these two children we cut through the integuments, the linea alba, peritoneum, liver and heart, Glisson's capsule, we separated the diaphragms which joined in front under the heart; we cut through the intestinal loop uniting the cæcum of A to the jejunum of B; we then cut through the peritoneum, linea alba, and integuments connecting them on the other side, which, in our dissection, was the base. The vascular and nervous connexions were innumerable, but they were very small and anomalous.

Could the children have lived? You, sir, can judge of that question for yourself. The difficulty would have been in their birth. They appear to have died from pressure on the cord during the birth of the heads. Twins externally similar, but full-grown, are now preserved in Dr. Hunter's Museum, at Glasgow: it was necessary to separate them *in utero*, before they could be removed from the mother. Had our subjects been safely born, and at their full time, there is no very strong reason to doubt their viability. There is a fear lest the chyme would have passed through the connecting loop of A and B, into the large intestines, before the performance of chylification; but as it would have gone through one yard of the intestine, that fear is by no means conclusive. The position of a distinct processus vermiformis, one inch under the loop, seems to indicate, that although from every visible circumstance we must call the remainder of the tube jejunum and ileum, we may yet consider it highly probable that the tube below would have possessed the functions of the large intestines, and that, before the intestinal contents could enter either it or the large intestines of A, by the loop, the chyle may have been elaborated by lacteals following an early distribution. Had these twins lived, A would have required a slight operation for imperforate anus. The cul-de-sac of the rectum was in apposition with a well-formed and dark anal depression, and the fissure of the nates was perfect. Had they lived, they would certainly have been one of the most curious of living structures; but, like all other good things, one-half would not have been told us at once. In the absence of facts, the imagination may have ventured to intrude on the proper sphere of observation; but death alone, or rather death and the knife, would have been the only informants of "the truth, and the whole truth, and nothing but the truth."—Believe me to be, sir,

Yours most respectfully,

JAMES DEANE.

Swan Street, Loughborough,
April 5, 1838.

P.S.—I am happy to say that the mother is doing well.

POST MORTEM EXAMINATION OF AN OLD PUGILIST.

To the Editor of the Medical Gazette.

SIR,

THE narration of injuries that occasionally happen to the body through accident or disease, is always interesting to the medical practitioner; and it is with this impression that I am induced to forward the present communication. The subjoined account is one out of many that might be cited to shew the extreme value of pathological researches; for if a post-mortem inspection had not in this instance been procured, we shoud have remained in ignorance both of the ravages that disease had made in the economy, and also of the extent of those organic lesions that proved the immediate cause of death.

There is nothing remarkable in the following case, but the occurrence of an adventitious membrane under the outer covering of the brain, which was of very large size. Although morbid deposits of this kind are not frequently witnessed between the tunics, yet their

existence, to say the least, cannot be regarded as extraordinary, when we take into consideration the structure and function of the membranes themselves, and the violence of the inflammation to which at times they are liable. The membrane that was here found attached to the inner surface of the dura mater was the undoubted result of inflammation; and it is not at all improbable that its fsrmation originated in the diseased action that supervened to the meninges of the brain, upon one of those pugilistic contests in which the deceased had been so frequently engaged during a certain period of his life. No satisfactory history could be obtained as to the period when he was seized with paralysis; but in my opinion it might be partly if not wholly attributed to the pressure which this membrane had exerted upon the substance of the brain.

If you shall regard the detail of sufficient importance to occupy a place in the columns of your journal, you will oblige me by inserting it at your earliest convenience.—I am, sir,

Your obedient servant and
constant reader,
CHARLES THORNHILL.

Darlaston, Staffordshire,
March 27th, 1838.

Benjamin Baylis, æt. 43. of stout athletic make, had been affected with paralysis of the right side for upwards of two years, and through indigent circumstances had become an inmate of the poor-house belonging to the parish of Wednesbury. During an earlier period of his life he had been a noted prize-fighter, and had often proved a match for some of the best pugilists of his day. Some years ago he contended for two hours on Sutton Coldfield with a person named Griffiths, who at a subsequent period was elected governor of Shrewsbury gaol, and was lately reputed to be enjoying the comforts of affluence. The paralysis came on while he was residing in London, but I have failed in ascertaining the exact period of its attack. About two years previous to his death, however, he was removed to his native parish, since which time he remained in tolerably good health; and notwithstanding his lameness he was frequently seen to traverse the streets by the aid of a crutch, with the palsied arm hanging useless by his side.

On the evening of December 23d, 1835, he had gone into the town on some trifling errand; and while crossing one of the principal streets he had been knocked down by a cart that was passing rapidly along. He was immediately conveyed back to the workhouse, where he was attended by the parish surgeon, who bled him largely from the arm: but he never seemed to rally from the moment of the accident, and he died after the lapse of about thirty-six hours.

As there was evidently some obscurity about the nature of the case, the body was opened at the request of the parish officers. The examination was conducted by Mr. Lees, of Wednesbury, assisted by Mr. Wm. Underhill, and myself; and the following are the notes I took on the occasion.

The body presented a well-built appearance, and there were no external bruises, excepting a discoloration and abrasion upon the outer surface of the right arm.

The dura mater was firmly adherent to the calvarium; and on making a small incision through it, from five to six ounces of dark fluid blood issued forth. On the inner portion of the dura mater of the left side, and attached to it, was an adventitious membrane apparently of long standing, and covering nearly the whole of the corresponding hemisphere of the brain. It was of equal thickness with the fibrous membrane to which it adhered, but of rather softer structure. It was of mottled appearance, and greatly resembled in character the morbid adhesions that are formed in inflammatory affections of the plura. The tunica arachnoides was firmer than usual, and the vessels of the pia mater were turgid with blood. The left hemisphere seemed smaller than the right, and looked as though it had been compressed by the false membrane. The substance of the brain was firm, and studded with numerous bloody points. The left ventricle was distended by serous fluid to the amount of two ounces, and the arachnoid constituting its floor was considerably thickened, though softened in texture. The corpus striatum of the same side was nearly absorbed, but its remains furnished a yellowish gelatinous appearance, without any distinct reference to original structure. The optic thalamus was reduced below the natural size, and the tænia semicir-

cularis had entirely disappeared. The fluid occupying the anterior cornu of the left lateral ventricle was of a thick yellow gelatinous kind, while that portion which was found in the posterior descending corner was of the usual serous character.

No morbid appearances were discovered in the right ventricle. On making incisions through the substance of the cerebellum it was softer on the left than on the right side.

The longitudinal section through the integuments of the trunk displayed a complete mass of fatty substance. On examining the interior of the thorax, several ounces of fluid blood were discovered; and the seventh, eighth, ninth, and tenth ribs of the right side were fractured. These latter were not only detached at about an inch from their heads, but they penetrated to some distance within the chest. The posterior portion of the middle lobe of the right lung was evidently abraded by them in two or three places; the diaphragm also was pierced, and bore marks of considerable injury. The heart was of natural size, healthy, and surrounded with fat; and a large quantity of sebaceous matter was also found imbedded beneath the sternum.

The peritoneal covering of the bowels was healthy; but from four to five pints of coagulated blood were collected in its cavity. The omentum was thickened in places, and so completely studded with fat, as almost to resemble the head of a small cauliflower. The outer coat of the stomach was highly vascular, and particularly thickened at the pyloric orifice; but the mucous lining was in all respects healthy. The remaining part of the alimentary tube was natural, though greatly distended with gas. The spleen was of ordinary size. On removing the liver for the greater convenience of inspection, the outer cavity was found to be burst in several places, and a large wound was discovered in the under surface of the right lobe, running from the right extremity towards the lobulus cordatus. This wound was between three and four inches in length, and about an inch and a half in depth, and had evidently been made by the ragged surface of one of the fractured ribs. The pelvic viscera were without marks of lesion.

RECOLLECTIONS OF CHOLERA:

ITS NATURE AND TREATMENT.

By W. Griffin, M.D. Limerick.

No. X.

Tartrate of Antimony and Ipecacuanha.

I shall have to mention these medicines again when I come to speak of emetics, but they are now to be considered with reference to properties influencing the system in a very different manner. No one who has witnessed the effect of emetic tartar in inflammatory diseases, when used in contra-stimulant doses, can be surprised it should occur to the physician, in many countries, to try its influence over cholera. It was, indeed, at a very early period, employed in India, and the practice was followed up by several practitioners during the late epidemic, in Russia, Prussia, Germany, and France. Almost all who have adopted the treatment have applauded it highly; and we can only account for its not having ever been fairly or sufficiently tested, in England, on the supposition that the repeated administration of a medicine capable of inverting the action of the stomach so violently, and of depressing the strength so alarmingly, was rather a bold measure in a disease the most obvious symptoms of which were disturbed action of the stomach, and general prostration of the vital powers. Such timidity would have been highly inexcusable, if, instead of the opinions of the several advocates in its favour, we had tables of the numerical results in the practice of each. When a man meets with three, four, or five consecutive cases, terminating fatally under any treatment, he must be very hardy if he proceeds, supported merely by general opinions; but, on the other hand, if he abandons a remedy under the same circumstances, while he has accredited tables before him proving that whatever the loss may be in limited trials, it cannot exceed 16 in the 100 in the mass, he is never likely to attain success or confidence with any treatment whatsoever.

The effect of large doses of emetic tartar ipecacuanha, frequently repeated, in cholera, was found to be the very opposite to emetic after the first two or three doses—as in the case of inflamma-

tions. Whenever the interval between the fits of vomiting was most prolonged, or the symptom soonest ceased altogether, the recovery was more probable and speedier; wherever the interval was short, or the emetic property of the tartrate or ipecacuanha was long perceptible, it was more doubtful, and the remedy less likely to succeed. Dr. Thompson, of Madras, found the latter very successful, given in a dose of only ten grains, with five every half hour after until the vomiting subsided. The German physicians gave them in doses much larger, and more consonant to the views entertained of their manner of effecting a cure. Their example was followed in Paris. M. Recamier gave the tartrate of antimony in the dose of two or three grains; ipecacuan, in a quantity varying from fifteen grains to twenty or more, and repeated it at intervals. The vomiting at first increased, but soon ceased altogether, and with it the intestinal discharges; a gentle warmth followed, the skin was covered by sweat, the cramps ceased, and tranquillity of the system was restored. What the proportion of cures, upon the whole, which could be obtained in any stage of cholera, by this treatment, we cannot learn. In England, although the trials, so far as we have any published accounts of them, were very few, they appeared, in one or two instances at least, to have been more accurately made. Mr. Steward, who after vomiting with the tartrate, gave it in two or three grain doses every hour, with one, one and a half, or two grains of opium, states that he had 17 recoveries and 4 deaths in twenty-one cases, or about 19 deaths in the 100, which, if they were all primary cases, was not far short of the success with calomel, and if several were in collapse, may be said to exceed it. Mr. Steward states that before he had adopted this plan he had 17 deaths in twenty-six cases! An anonymous contributor to Dr. Ryan's Medical Journal*, who seems to have been a very accurate observer, states that he gave this medicine in doses of three grains every two hours in the dry state, until the vomiting subsided, which was always soon, and usually after the third dose. If the vomiting continued he sometimes combined it with a grain of opium. The effect of it, however, uncombined, was generally to check the vomiting, not to increase it, and he remarked that the recovery was always speedier in proportion as the interval between the fits was prolonged. The most accurate report which I have met of the treatment by tartrate of antimony is that published by Mr. Langford, of Manchester. He administered it in much smaller doses—half a grain frequently repeated, with toast and water, or whey *ad libitum*, prohibiting all heat and friction. The following is the statement of results in 94 cases*:—

1st class.—Cases with the skin and tongue warm, and pulse tolerable. 28 in number: all recovered.

2d class.—Cases with skin and tongue cool, or icy cold, with feeble pulse. 36 in number: 25 recovered, and 11 died.

3d class.—Cases pulseless. 30 in number: 11 recovered, and 19 died.

By throwing the two first classes together, a comparison may be drawn between the tartrate of antimony and calomel treatment. They amount to 64 cases, out of which died 11, or at the rate of about 17 in 100; the greatest mortality under the calomel treatment in Limerick being 16 in 100.

In the 3d class, or cases of collapse, there were 19 deaths in 30, or nearly two out of three; that is, about three recoveries in ten, which exceeds the amount of recoveries from this state when calomel was used, and is equal to the amount obtained when the treatment was limited to very mild cordials and diluents.

If this report was borne out by larger trials of the remedy, I have no hesitation in saying it would be preferable to the calomel treatment, as it would be unattended by the occasional unpleasant results which follow the employment of the latter medicine. It requires, however, to be tested still further, and in future comparisons it would be necessary to ascertain previously whether the mortality under the calomel treatment should be rated at 16 in the 100—the extreme amount in any of the reports—or should be fixed at a proportion between that and four—the least amount in any of the reports. As I cannot determine this question myself, I have always, in comparing the calomel with other remedies, quoted the mortality at

* Vol. II. p. 528.

* Johnson's Medico-Chirurgical Review, p. 146. No. 39, Jan. 1834.

the extreme: to err, if I do so, at least upon the safe side.

Mr. Olier, of the New Bailey prison, praises the effect of emetic tartar, given according to the views of Dr. Reisch, of Berlin, and says he has witnessed many patients, after a pulseless, hopeless state of suspended animation, continuing for hours, restored to life, *without the risk of consecutive fever*. Mr. Olier was probably not at that time aware that such wonderful recoveries from collapse would have equally taken place under very various treatment, or under no treatment at all. Indeed, Mr. Langford candidly acknowledges he should not expect success from it, "in the most malignant and fatal stage of spasmodic cholera;" meaning, I suppose, the stage of pulseless collapse. There is only one point of importance in Mr. Olier's statement, if he be correct, which is, the freedom in recoveries under that plan, from risk of consecutive fever. Any treatment during collapse, or previous to it, which could secure this protection, would form a most valuable addition to our means of cure.

Sulphate of copper has been employed with the same views as induced medical men to use the tartrate of antimony and ipecacuan. I should not be surprised at its effecting very numerous recoveries; but I have seen no reports which could tend to suggest even an opinion on the subject.

Astringents.

The serous diarrhœa in cholera, and the rapid sinking which accompanies it, has engaged the attention of every medical practitioner. No one, indeed, could well witness fifteen or twenty cases of the disease, without being convinced that, although not an essential cause, the bowel complaint is a symptom which contributes more than any other to run the patient into collapse: hence astringents were very early employed as a means, if not of counteracting all the phenomena of the malady, of at least suppressing or checking the one which was most formidable, and so gaining time for other measures. Opium has been a most popular medicine in this view in all countries, and is one of the few whose reputation, in some degree, outlived the universal discredit which fell upon almost every eastern remedy during the continuance of the epidemic in England. I cannot believe, however, that its astringent property is more than a mere adjuvant in the cures which it effects, since its success in suppressing the alvine discharges is not always the measure of its influence in allaying the other symptoms of the malady. I have no doubt its essentially curative power is sedative or contra-stimulant, like calomel and tartrate of antimony, along with which remedies it has been already considered.

The success with which the subacetate of lead has been given in several forms of diarrhœa, led to its employment for cholera in most places, but it does not seem to have made any permanent character even with those who gave it a considerable trial. Perhaps this may arise from the unfortunate propensity in the profession to decide upon the merits of remedies by their application to all stages of the malady; so that by their frequent failures in the stage of collapse, which constitute nearly half the cases one is called to, any credit they have acquired in the cure of the previous stage is obliterated. The late Dr. Mackintosh states that it was tried in Edinburgh without any good effect, and other medical men speak indifferently of it. Perhaps the most extensive trial it has had, was in the practice of Dupuytren, at the Hôtel Dieu. He gave a dessert spoonful of a solution of the subacetate of lead in an infusion of mint with syrup every hour, and a cup of decoction of poppy-heads every two hours, employing at the same time a lavement of the decoction every third hour, and dry friction. Of 64 patients placed under this treatment, he found 30 dead in the morning, and eventually abandoned it altogether. I have myself never given it by the stomach; but, on the second irruption of the cholera at Limerick, it was almost the only astringent we made use of by lavement. It was given very extensively in this way, in the proportion of half a drachm at a time, and, as far as the suppression of the diarrhœa, which was all we looked for from it, with very general success.

Professor Graves, of Dublin, has lately, in a communication to the Editor of the Medical Gazette, recommended it most strongly to the consideration of the profession, given in combination with very minute doses of opium. If it proved, on the average, in extensive trials, as successful as calomel has been found pre-

vious to collapse, it would, I think, be a much preferable medicine; but I fear, from the ill success of Dupuytren, who used it in combination with decoction of poppy-heads, which may be supposed tantamount to the minute doses of opium, and from Professor Graves's experience of it not having extended through the whole period of cholera, that however influential in controling the progress of many severe cases, it would scarcely attain an equal amount of cures with calomel, if equally well tested. This, however, requires to be determined by future experience, and I know of no remedy which deserves more to have its capabilities accurately investigated.

In collapse, it is almost needless to say, it offers as little hope as any other remedy. If Professor Graves's recommendation of subacetate of lead was meant to apply to its use in that stage, I have no hesitation in saying he would find his confidence disappointed in more extensive trials. I have already shewn that the collapse of cholera, though universally fatal if abandoned to nature, will occasionally do well, no matter what the previous treatment has been, if only closely watched and judiciously managed in the consecutive fever, into which six or seven out of ten spontaneously struggle from the alaid stage. There is no man who witnesses these wonderful recoveries, especially the revivals from collapse, but is tempted to connect the event with some remedy which he has been administering. Hence it is, our periodicals teem with illustrations of extraordinary recoveries, under the supposed influence of almost every known remedy, and which, if the conjectures regarding the means were as true as the facts, would make it very inexplicable why any one should die of cholera. It would appear, indeed, as many have supposed, that in collapse the nervous system is nearly insensible to medicaments. Enormous doses are taken without the slightest perceptible effect, and the revival eventually takes place without relation to them. Perhaps pure stimulants may be more or less an exception, as very powerful ones seem sometimes to exhaust the feeble power which still exists in that state, and milder ones, I have sometimes thought, accelerated the period of reaction. Camphor dissolved in æther, as given by Recamier, in Paris, or in spirits of wine, as administered in this country, appears to have some such influence; and we might, perhaps, infer as much of mild stimulants, from Magendie's statement, that of nearly 600 cases which were treated by him, all except 38 who died on the frame on which they were brought previous to being put to bed, had the heat of the body and the circulation restored. His treatment consisted chiefly in the administration of weak punch, acetate of ammonia, &c. with friction, and dry heat externally.

In the primary stage of cholera, I am disposed to think the acetate of lead may be found a very valuable remedy: whether it would effect as great a proportion of recoveries as calomel or opium, or even as tartar emetic, or ipecacuan, is yet matter of conjecture; but I believe, like opium, its success would be more certain in proportion to its early administration. During an unexpected visitation of cholera in Limerick, within the last two months, it was very generally employed by medical men, exactly in the form suggested by Professor Graves, without any eventual success. It for the most part succeeded in suppressing the alvine discharges, but did not prevent the consecutive fever, of which they almost all died. The cases were, it is true, very neglected ones, few coming under treatment until the pulse was imperceptible. It is singular, that although the disease was of a more protracted type than in the former epidemic, it was on the whole vastly more fatal, two-thirds at the least of all who were attacked having died.

Other astringents have been used with advantage in cholera, but I suspect them to be much inferior to the acetate of lead. Dr. Elliotson used sulphate of copper in the dose of half a grain every half hour until the diarrhœa abated, and speaks favourably, though not confidently of it. Dr. Craigie employed nitrate of silver in grain doses. Strychnine has been used by others, and among them by the late Dr. Mackintosh, of Edinburgh. He gave it at first in the dose of 1-12th of a grain, gradually increased to a grain every hour or two, and conceived that, besides checking the discharges, it had the effect of shortening the collapse, and rendering the reaction more permanent.

On the whole, I should say, astringents, from whatever class they may be selected, and whether given by the

mouth, or in the form of lavement, should form an essential part of the cure of cholera. But none of them have yet been proved capable of absolutely controling or suppressing the disease to an extent that one could confidently trust to in general practice.

Purgatives.

It may seem a mockery of all treatment to enter seriously into any discussion on the value of purgative medicines as a remedy in cholera, after having so unequivocally admitted the advantage, and, indeed, the necessity, of administering astringents. But we are yet far from understanding the principles of therapeutics so perfectly as to venture to reject evidence or statements in favour of particular medicines, merely because they are opposed to received systems or doctrines, or cannot be reconciled to our knowledge of other well-attested facts. We cannot, for instance, deny the effect of strong solutions of caustic in subduing violent inflammations of the tunica conjunctiva; or cayenne pepper in relieving inflammatory sore throat; or spirits of turpentine in effecting the cure of burns; although aware that a very opposite mode of treatment, and one strictly consistent with principles applying to inflammatory action, wherever situated, is also generally successful. If we suppose the whole tract of the intestinal tube to be in a state of extreme irritation, or even of inflammatory action in cholera, and relievable by general and local blood-letting, and gum-water, it does not necessarily follow that a powerful irritant applied directly to the diseased membrane, as in the case of the eye or throat, would not be equally successful. We, nevertheless, justly require stronger and more numerous facts in proof of the efficacy of medicines whose ordinary actions are opposed to the general principles of treatment in any similar disease, than we do in support of such as fall in with long-established opinions. In this respect, I apprehend, much as we have heard of the effect of purgatives, and especiaially of croton oil, in cholera, they must be assigned a very inferior place among the list of remedies, if, indeed, they can properly be allowed to rank among them at all. Mr. Tagart, Inspector-General of Military Hospitals, praises croton oil highly. A Dr. Ward recommends the use of it chiefly in young subjects, and some contributors to our medical periodicals give cases of extraordinary recoveries from collapse, through its means. Dr. Douglas, of Dieppe, says that it does mischief while there exists either vomiting or purging; but when these have ceased, and there is but a glimmering of life, its effects are often wonderful. He gives some remarkable cases of almost immediate reaction brought on by it in very desperate cases, and one of death, occurring when it was given in the early stage, during the continuance of vomiting and purging*: he looked upon it merely as a *dernier resort*. I do not know whether we are to look upon the successful administration of sulphate of soda in small doses, frequently repeated, in this disease, as attributable to its purgative property, or to some ill-understood remedial influence possessed by it, in common with other salines, which do not seem to cure by increasing, but rather by allaying and altering, the peristaltic action of the bowels. The physicians in the Isle of France were in the habit of giving it in the dose of a drachm every hour, until the dejections become yellow, and, it is stated, with considerable success. It was also tried by Recamier, in the Hôtel-Dieu, among many other remedies, but I do not know with what result. For my own part, if I was bound to receive without hesitation the testimonies in its favour, I should class the remedy among the simple salines, whose influence, if any at all, is of a nature we do not understand, rather than admit that severe purgatives, and especially saline purgatives, which are known indisputably to produce cholera, could also be the fittest medicines to effect its cure. I have seen rather an amusing letter from a surgeon in one of the medical periodicals, who, from having witnessed beneficial effects, as he conceived, from the purgative effect of large doses of calomel in a few cases, warmly advocates the general use of purgatives. It is sufficiently ridiculous to see such confident inferences deduced from so few cases,

* The late Dr. Mackintosh says, "in taking a retrospective view of the cases in Drummond Street Hospital, I cannot but condemn the practice I resorted to, more particularly with relation to the exhibition of *purgatives*. I cannot state the fact too strongly, that they are dangerous remedies."

especially when the only two he cites died; but the most amusing circumstance is, all his observations seem grounded on the belief that it is an extremely difficult matter to procure an evacuation from the bowels in cholera! It never seems to have struck his mind, that the usual effect of large doses of calomel in cholera is not only not purgative, but the allaying of both vomiting and purging.

When we recollect what extraordinary recoveries occasionally take place in cholera, under every possible treatment, provided only the patients are well managed through the consecutive fever, we cannot attach any importance to the few cases, however imposing, which are produced in proof of the benefit of purgatives in the disease.

[To be continued.]

RESULTS OF INFLAMMATION.

To the Editor of the Medical Gazette.

Sir,

As your correspondent, "Medicus," in your number for March 3d, ult., seems to be halting, not between two, but between several, opinions on the subject of inflammation and its products, I take the liberty of throwing out a few hints for his consideration and guidance.

A little knowledge is always a dangerous thing, and I greatly fear that, unless the questions connected with this subject be more carefully, as well as more deeply agitated, it is much more than probable that "the result of the inquiry may prove that we know just as much, and no more, than was known half a century ago." It is not enough for a man to sit in his easy chair and commit his loose notions negligently to paper; it is not enough to be satisfied with believing so and so has been lately proved, as if it were a matter of no moment whether it had or not. He who would agitate these questions profitably, must be more industrious than your correspondent, Medicus; he must look to the authorities himself—he must make himself thoroughly master of all the facts whence the conclusions are drawn—he must reject unscrupulously, without any regard to the high name of a particular author, every inference which is not strictly borne out by the evidence of the facts adduced. On the other hand, while he is thus cautious against being imposed upon by deceitful fallacies, he must be equally on his guard against the no less surely fatal error of neglecting those conclusions which are really sound, however much they may be at variance with his preconceived opinions. It is rare that authors may not be trusted as to simple facts: as much, however, cannot be said of their inferences. As far as my experience goes, those authors are least to be trusted in their conclusions who have been much engaged with the microscope, and long employed in poring over excessively minute dissections: they are too often, as Napoleon found to be the case with some of his savans, like the objects they have to deal with, "*infiniment petits.*"

This caution to your correspondent is not altogether uncalled for, since he has not only misstated Gendrin's opinion on the formation of pus, but has also failed to detect the most glaring fault in the whole work of that author. Gendrin's facts demonstate, with great distinctness and accuracy, the gradual and successive deposition of all the constituent parts of pus; he sees the red globules gradually lose their colour as they approach the surface of a wound, and at length slowly emerge; he sees that the addition of these colourless globules to the matters previously effused constitutes pus, and yet he attributes the formation of this substance to the operation of some chemical change occurring after the effusion has taken place, while no attempt whatever is made to show in what the change consists! Is it not clear, that if those matters constitute pus, upon any new chemical change really taking place, *the new product would not be pus, but something else?* It is curious to observe how Gendrin persists in this blunder, and labours to show that blood may become pus by remaining at rest in the tissues, by means of facts which prove, that however long the blood may remain at rest, no pus is formed unless inflammation be established; in which case, of course, the pus is formed, and must undoubtedly be formed whether the blood were there or not: he does not show the change of pus into blood, but the addition of pus to blood. If Medicus be desirous of knowing in what inflammation actually consists, and on what its products de-

pend, let him, with the caution I have just shown to be so necessary, examine the condition of the capillaries — the nature of the blood's current through them—to what cause that kind of current is owing — what variations take place in it—which variation it is that invariably precedes, and, in fact, constitutes the commencement of inflammation—the nature of the current and condition of the capillaries during inflammation—and lastly, the nature of the absorbing function, and how it is affected by the occurrence of inflammation.

If Medicus will undertake this investigation, which, judging from his observations, the unsettled state of his mind renders him well qualified to enter upon, as being open to conviction, he will find a sufficient number of well-ascertained facts has been accumulated within the last half century, upon each and every one of the above-mentioned points, to set them all at rest; or if this inquiry be too serious an affair, and he merely wishes to ascertain whether pus be a secretion or not, let him only look to the evidence on the affirmative side of the question: here it is, Mr. Hunter says, that at a certain period of inflammation, "either a new set of vessels is formed or a new disposition of the old takes place, which I call glandular; and the effect, or pus, a secretion." Is this true, in fact, or purely imaginary? It will give no one much trouble to answer this question.

I have the honour to be, sir,
Your most obedient servant,
CHIRURGUS.

March 15, 1838.

MEDICAL GAZETTE.

Saturday, April 14, 1838

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

MEDICAL EMIGRATION.

WE lately offered some observations to our readers on the eager competition existing in our profession, which seems to be daily on the increase, and which threatens to reduce the remuneration of the practitioner of physic to the level of the wages of an artisan. Although the popular fancies as to the reward of medical exertions, commonly veil the unpleasing truth from the young aspirant before he has entered the profession, yet, once within the sanctuary, it is too glaring to be concealed. One of the most obvious effects of this late-born wisdom is the desire of emigration, which seizes whole troops of our younger brethren; yet, vast as are the British dominions, illimitable as appear to be those colonies within which, according to the old Spanish boast, the sun never sets, the field is still too scanty for the labourers. Considering this natural tendency to expatriation, we must regret that it is so difficult to obtain any accurate account of the relative advantages of the countries to which the medical speculator is likely to betake himself. Could such accounts be obtained, it might turn out that the dusky reality bore as much resemblance to the bright description by which the young student was led astray, as the hardships of a campaign do to the promises of a recruiting serjeant. Really impartial statements, however, are of such rare occurrence, that we must generally be content with those that have a certain twist, and endeavour, like unbiassed judges, to get at the truth amid the conflicting sophistry of interested advocates.

Thus, for example, the account of the constitution of the medical department in the Honourable Company's service, which we extracted from the Indian Journal of Medical Science *, contained much useful information, though evidently drawn up by one who had determined to paint things of any colour but *couleur de rose*. The young surgeon whose cross-grained destiny the writer set forth—the hero of his tale, if we may use the expression—is placed under

* MED. GAZ. vol. xx. p. 323 and 364.

circumstances much more unfavourable than are at all likely actually to occur. He is supposed to come out to India with borrowed money; his passage and outfit being alike paid for in this manner, to the tune at least of 200*l.* On arriving in Hindostan he must buy a regulation tent, a horse, saddle and bridle, a table, chairs, cooking utensils, and his uniform, amounting to 150*l.* more, which he is again obliged to borrow; and as money in that country partakes of the general luxuriance of tropical vegetation, he pays 12 per cent. for the use of this sum, or, indeed, 18 per cent., including life-insurance.

Then, even the modest retinue of an assistant-surgeon amounts to a considerable number of individuals. He must have a sweeper, bearers, a *kidmutgar*, or waiting-servant, a washerman, water-carrier, cook, scullion, *syce*, or ostler, and a grass-cutter. No servants-of-all-work in India! The upshot of the matter is, that while these and other necessary expenses amount to 30*l.* a month, his pay is only 23*l.*; so that during the five years which are supposed to elapse before he obtains the medical charge of a regiment, and a consequent addition to his income, his debt is steadily on the increase. He is now able to pay the interest of what he owes, but cannot do any thing towards lessening the principal, till he becomes a full surgeon, after the lapse of ten years more. Meantime, the hero of the tale is supposed to marry, and he takes this step on ultra-Malthusian principles, in order to increase, not the Anglo-Indian population, but his own practice. "It is deemed essentially necessary that a medical man should be married, to be chosen as a family practitioner; and he therefore complies ordinarily with what he finds to be the universal wish."

The Malthusian project, however, turns out ill, as the wife seems to bring neither pagodas nor practice, but only children; so that while he is debited with wife and little ones to the amount of 150 rupees a month, not an *ana* is set down on the other side of the book for dowry or fee. Still, however, if he is a surgeon at a full batta station, in spite of his devouring wife and children, he pays off his debt in four years and eight months; and at the age of 45 is even with the world, and has an income of about 777*l.* a year.

The author of this gloomy history protests that he has given a favourable turn to matters; that he might have supposed his surgeon to have bad health, with all its contingent expenses, and diminution of salary; or to be obliged to send his wife and family to Europe, which would make another loan necessary to defray their passage. In such cases he must continue in debt till he reaches the rank of superintending-surgeon.

It would be easy, of course, to make a more favourable set of suppositions, and in doing so, to be nearer the mark. We might imagine, for instance, a man going out who possessed sufficient capital not merely to pay his passage and provide himself with cotton shirts and Russia duck trowsers, &c., but even to buy the horse, saddle, bridle, and so forth, when he landed; nay more, we might be so romantic as to suppose that his wife had a small dowry, and that his self-devotion in going up to the altar of Hymen in compliance with "the universal wish," was rewarded with a practice of at least £80 a year. In such a case his debts would be trifling, and his situation comparatively comfortable. Still we must candidly confess, that while the condition of the Indian surgeon, as gathered from the author's own premises, does not seem so bad as he represents it, on the other hand, it is still farther from being so

brilliant as ordinary belief would make it. Yet, as men contrive to live even in London, though the streets, contrary to rustic belief, are not paved with gold, so we have no doubt that clever surgeons flourish in India, and average ones pick up a subsistence, though the visions of lacks and crores of rupees, with which our forefathers regaled themselves, have long since vanished. But if emigration to India is of such doubtful prospect, what shall we say of less golden countries—of Africa, Australasia, or Spain?

Of the Western coast of Africa the less that is said the better; no statistical writer, we believe, has yet tried to distinguish the bones of medical men in that vast charnel-house, or to give the minute fraction representing the chance of a surgeon's return. At the Cape of Good Hope, on the contrary, as well as in Australasia, the climate is healthy; and those practitioners who find that the genial sky is in one way unfavourable to their views, may relinquish their original profession, and take to farming instead of physic.

Spain has of late years been the hope of many of the younger members of our profession; and an interesting little work now before us*, though chiefly devoted to details more strictly surgical, is not void of instruction for practitioners who emigrate on speculation.

We must observe, however, that as the paper in the Indian Journal was an attack in the guise of an account, so here we have a defence in a similar garb. In the former case, the assistant-surgeon has nearly 300*l.* a year; *but* his cottons and woollens, his single-poled tent, and the gay coat upon his back, have been paid for with *æs alienum;* the occupier of a double-poled tent, the full surgeon, has nearly 800*l.* a year; *but* his wife and children (who ought to have brought him practice, yet do not) eat up his income, and, unless a very lucky dog, he cannot pay off his debts until he is a superintending surgeon.

In Spain, on the other hand, the connexion between the medical officers and pay of any kind seems to have been slight or none; single and double-poled tents were equally rare; "*but*," says Mr. Alcock, "it has not been a service, however, of unmixed evil, as some of our friends would have the world believe."

This we can readily believe, for unmixed evil is almost as rare as unalloyed good. Among other advantages, the medical survivor of the Spanish campaigns would come back inured to any and every hardship. No mattrass, though stuffed with straw (tick-penetrating straw), can be hard to him who has slept upon bare stones; and even the aluminous compound of our London bakers is better than the Vittoria bread, which was "often made of unsound flour, of the worst kind, imperfectly kneaded and baked, forming a mass of black and heavy dough, calculated to puzzle the digestion of an ostrich, incapable of affording nourishment, and well adapted, on the other hand, to produce disease*."

If the doctors as well as the common men ate this noisome mixture, it will partly account for the mortality among them.

"Many of our number were carried from the hospitals to their quarters, to rave, in the sharp access of their delirium, of blackened feet rotting from the living flesh, of the screams of the dying

* Notes on the Medical History and Statistics of the British Legion of Spain; comprising the results of gun-shot wounds, in relation to important Questions in Surgery. By Rutherford Alcock, K.T.S. &c., Deputy Inspector-General of Hospitals, with the auxiliary forces in Portugal and Spain. London, 1838.

* Alcock, p. 14.

still struggling among the dead, until they themselves were added to the list. Such scenes had they daily witnessed, and their faithful picture and piercing shrieks, more than once, in spite of the stoicism of habit, made me shudder*."

Your comfortable but pitied Indian, on the other hand, when he returns to England, or as he phrases it, to Europe, is somewhat staggered at the scanty simplicity of our social arrangements. His pay will, in this country, afford him neither *syce*, nor attentive *kidmutgar ;* too happy if he can secure the aid of a stunted foot-boy, and is not reduced to the encyclopedic services of a lodging-house drudge!

Mr. Alcock praises the zeal and courage of the numerous medical staff who entered the service, regardless of taunts and sneers at home, and discharged their duties faithfully, during a period abounding in dangers and privations.

Now if we take it for granted that the medical staff entered the service with a full consciousness of what they were about to endure, it will be impossible to deny them the honours of martyrdom—the praise due to those who sacrifice their lives for the benefit of the human race. But if we suppose their zeal, though ardent, to have been somewhat less than this, and that they expected to come back, and that with their pay in their pockets, we cannot but deplore the want of information current on these subjects, and the recruiting illusions by which surgeons as well as soldiers are taken in. The time, however, for taunts and sneers is over; and the most callous epigrammatist would refrain from his japes, when he heard that of *seventeen* medical officers at Vittoria, *eleven* died within four months!

* Ibid. p. 2.

PHYSICAL SOCIETY, GUY'S HOSPITAL.

March 24, 1838.

Dr. Hughes in the Chair.

The minutes of last meeting having been read and confirmed, Mr. Brereton rose to call attention to that part of his paper in which he had treated of

Lesions of the Brain in Connexion with Derangements of the Intellect, Delirium, &c.

and mooted the inquiry whether certain defects of the mind could be traced to coincide uniformly with alterations in some part of the cineritious structure. In lunatics, paralysis to some extent often co-existed ; and it was important to remember this, in order to avoid hastily attributing the lunacy to the same cause which produced the paralysis.

Mr. Solly said he had attempted, in his work, to explain how far the surface of the brain was associated with the intellectual functions. Abercrombie and other writers had laid it down as a rule, that arachnitis occasions delirium, and that if the inflammation go on, the intellect becomes lost. This result, in his (Mr. S.'s) opinion, was not referrible to the inflammation of the membrane merely, but to a continuity of it to the adjacent surface of the brain. The subject was not, however, sufficiently examined, and he was inclined to believe that alteration in colour was often the only visible symptom of inflammation or other derangement in the grey matter, and was frequently overlooked in examinations.

Dr. Allen, of High Beach, said he had been physician for twenty years to a lunatic asylum at York. He was an advocate of, and formerly lectured upon, phrenology. In all cases where he had the opportunity, he examined the dead, and had *always* found his prognosis verified by the morbid appearance. In cases of that gradual decay of the mind, which was usually associated with masturbation, he had found softening of the cerebellum to exist.

Mr. Molloy, in reference to a case in the Dreadnought, quoted by Dr. M. Hall, at the last meeting, stated, that he had since visited the patient, in company with Mr. Edge, and had found that complete paralysis did not exist; for motion and sensation, to a limited degree, remained. In another case which had also been quoted, although there was complete paralysis of motion, yet sensation was not destroyed.

Mr. Hilton said that his dissections did not coincide with those of Mr. Grain-

ger. Mr. Grainger had described both the anterior and posterior roots of the spinal nerves to arise from the *middle lateral* column of the spinal marrow, and not at all either from the anterior or posterior columns. Mr. Hilton wished to reserve a minute statement of his own observations for another occasion, and would at present content himself with merely saying that he had traced both to the anterior and posterior columns. With regard to the excito-motory experiments of Dr. Hall and Mr. Grainger, there existed, he thought, one source of error—viz. that the sympathetic system of nerves had never been removed from the sphere of operation; and it was therefore difficult to say how much of the results obtained by these gentlemen might be assigned to these nerves. For the last six years, he (Mr. Hilton) had been in the habit of describing, in his public demonstrations of the nervous system, at this school, the various muscles of the body, and the integument immediately covering those muscles, as being supplied by the same set of nervous filaments. So that when an irritation was applied to the surface, by means of those nerves, it immediately excited the muscles connected with that part of the skin to contract. This was done instantaneously, and for an appreciable moment of time before the exercise of volition.

He thought that pathology generally bore out the views of Foville, who connected the thalami with the upper, and the corpora striata with the lower, extremities of the opposite side; and the difficulty which appeared occasionally to occur, seemed to him to be often explicable, as follows:—The nervous power is conveyed from both these bodies to the respective limbs downwards, along the crura cerebri. Now if the lesion existed in the thalamus, or in the crus cerebri below it, the chain of connexion between the corpus striatum and the lower extremity was cut off as effectually as if the lesion had been seated in that body itself.

Dr. Whiting said, there were abundant facts in pathology and in physiology to show that muscular contractions could occur independent of volition — as in tetanus, sneezing, coughing, &c. But many paralysed cases had fallen under his observation, in which the excito-motory influence, as described by Dr. Hall, could not be made to operate.

Dr. Marshall Hall said that one positive fact was of more value than a thousand negative ones, and that the latter by no means invalidated the general rule. The failure of excito-motory influence in these negative cases, might, in some instances, be accounted for by the circumstance of some disease or failure in the nerve between the spine and the irritated surface. Several years ago he had noted the fact, which he could not then explain, that if a frog be decapitated, and its spinal cord be afterwards divided in the middle of the back, upon irritating the posterior extremity it became immediately convulsed. Not so in the toad, treated in a like manner. He now knew, by the aid of anatomy, that in the latter the cauda equina commences very high in the back, and was divided in the experiment In the former, the spinal cord was cut through.

Mr. Key supported the view taken by Dr. Hall, and quoted cases in which paralysed limbs moved powerfully when touched or irritated.

Mr. Chevers, Mr. Solly, Mr. Randall, Mr. Roderick, and other gentlemen, also took part in the discussion; the remainder of which was principally occupied by an examination of the connexion between developments of the brain, spinal marrow, and parts of the nervous system, with various functions of intellect, instinct, &c. The lesions associated with aberrations of mind also came briefly under notice.

The thanks of the Society were unanimously given to Mr. Brereton for his excellent paper, and the Society adjourned to Saturday, April 7th, when Mr. Gorham read "Some Observations connected with the Respiratory Apparatus of the Infant, in Health and in Disease," Mr. Aston Key in the chair.

EMPLOYMENT OF CHILDREN IN FACTORIES.

To the Editor of the Medical Gazette.

Sir,

As the Factory Act has undergone an inquiry, and some important alterations have been already publicly discussed, it becomes every member of the profession who is conversant with the operation of the present act, to give his candid opinion upon those provisions which immediately fall under the province of the medical profession, and more particularly on that clause which entitles a surgeon to grant certificates of age to persons working in the mills. Being one of the specially appointed surgeons to grant certificates of age for the division of Heywood, I have ample opportunities of witnessing the good and bad effects of its operation, which I beg, through the medium of your excellent journal, to offer to the public. The act provides that no person under 18 shall have power to work in mills, without obtaining a certificate, stating the person either to be of at least 9 years of age, or

exceeding 13; so that the surgeon has to determine — 1st, Whether the person is above 9 years of age, and under 13; and if so, a certificate to that effect is given, which intitles a child to work eight hours a day, and attend a school during two hours. 2dly, Whether the person is above 13 and under 18; and if so, the certificate permits twelve hours' work during the day. The wages consequent upon obtaining these respective certificates are very different: the one with twelve hours' labour generally receives double the wages to another who works eight hours. Persons applying for these certificates are often the parents of the children, and dependent on them, so that it becomes a matter of great interest to obtain a certificate to entitle the applicant to twelve hours' labour. The consequence is, that the most barefaced lies are often told, in order to obtain a certificate stating the child to be above 13; and were it not for our knowledge of the strength and appearance of persons usually at that age, we should often be the dupes of designing and interested parents. When the late Mr. Rickards was Factory Inspector, he never attempted to strictly fulfil the instructions of the act, but contented himself with allowing a person physically efficient the power of work, well knowing the impracticability of working the machinery of the act to the letter. This, however, has not been the case with Mr. Horner, the present Factory Inspector, whose vigilance and perseverance have been particularly exemplified on this head, by trying every plan that could be devised to fulfil every word specified in the act; in the execution of which duty he has to contend with great difficulties, and more particularly in regard to the age, which will be seen by the repeated change in regulations addressed to surgeons, for their better guidance in granting certificates of age, medical men, in return, have been beset with similar difficulties, in not being able to give any regulation a fair trial before new ones have been issued.

The first rules which Mr. Horner made, after coming into office, respecting granting certificates of age, were these:—"The declaration which the surgeon makes in his certificate has no reference to the actual age of the person named; it states that, in his opinion, the person who has appeared before him is of the ordinary strength and appearance of the particular age which he inserts. The surgeon must form this from the physical condition of the person, without regard to the actual age*."

* Circular Letter to Surgeons, Sept. 28, 1836; paragraph 2d.

From this paragraph, Mr. Editor, you would suppose Mr. Horner would have occasion to make few alterations, as the subject of inquiry was a voluntary act of his professional judgment and skill. After this system had been followed by the various certificating surgeons, it was found necessary to enjoin height of body as an element in the inquiry, and the height of four feet three inches and a half was given as the minimum height of children of 13 years of age. For various obvious reasons height did not long continue an element of inquiry, for the progressive growth of mankind is very uncertain at different ages, and will differ very much in different families; and if height be taken as a guide for bodily strength, appearance, and capability of enduring factory labour, it would be very exceptionable, and would preclude every Lilliputian, however physically efficient, the chance of gaining a livelihood from factory employment. Actual age was the thing sought for, and baptismal registers being a sure test of age, it was deemed by the law officers that they should be taken as a reasonable substitute for medical certificates; and the consequence has been, that however physically efficient the applicant might be, surgeons were incapacitated from granting a certificate, except he could prove, or had good reason to believe, the person above the age he had to specify, whatever opinion he might have as to their strength and appearance. This plan was open to much fraud, abuse, and expense, and is not as yet practised, except where the surgeon has refused the applicant in default of physical strength and appearance.

The last and least exceptionable plan of knowing the age of a child is by dental eruption, which is become a very feasible guide as a test of age: the only exception to its being an accepted and undeviable rule is, that age entitles the person to work, whether a martyr to phthisis, scrofula, or any of those constitutional disorders which ought to preclude factory employment, however limited to hours, or eligible in years.

With these difficulties the specially appointed surgeon has to contend, which for the present I will leave to your impartial consideration, and I will now venture upon a few practical observations upon the health and fitness of persons in factory districts, and more particularly on those placed in my immediate neighbourhood. Much has been undeservedly said about the tyranny, hardships, and privations, which people working in mills are subjected to. It is needless to say that the present act has worked a complete metamorphosis; and even if these evils did exist, the operatives are at present so inde-

pendent of their employers, that if such existed at the present day, they would not be long tolerated. The work is for the most part easy, very simple, and without any of those despotic exercises which have been adduced; and though the hours of labour are long, they are not attended with much bodily exertion. The time is for the most part passed over so peaceably and pleasantly, it is difficult to get a child to any other work after being employed in a mill. Length of time has been commented upon by our metropolitan brethren as being conducive to varicose veins, hernia, phthisis, scrofula, &c., and a long list of diseases which are said to exist in manufacturing districts, but which, in my opinion, do not exceed the same number in agricultural districts. The exhalents of operatives are generally relaxed in factories, and sudden atmospherical vicissitudes materially affect them, causing inflammatory fever, gastro-pulmonary disorders, &c., which are by far the most common disorders incidental to a manufacturing district. Where the factory operatives reside in a country situation, and the rooms of the mill are large, the children are generally healthy, and possessing as good physical appearance as others in the same family engaged in agricultural pursuits. Where the operatives dwell in large towns, and in narrow ill-ventilated streets, small rooms, and ill-drained districts, they suffer more from these causes rather than the confinement in the mills. Generally the larger the mills are, the better the health of the operatives, the more regular their habits of industry and precept, and the more constant the employment and prosperity of the place.

The only points which require national interference are the crowded streets, small rooms in their dwellings, and want of draining and removing unnecessary nuisances from about their habitations; and these are generally caused by persons who, with their industry and economy, save sufficient to erect a few houses, which are done with as little expense as possible in their construction, to occupy little space to the injury and inconvenience of the inhabitants. In these houses from two to three families sometimes live, to save rent and fuel, which form a chief out-go in their expenditure. Another and very serious evil, existing amongst operatives, is the practice of drinking, which often deprives them of their necessary rest, and undermines their general health. This evil is often counteracted by the formation of Mechanics' Institutions, circulating libraries, and the establishment of religious and moral instruction. The wages consequent upon factory employment are very different, according to the kind of work, and manner in which it is done: for instance, a power-loom weaver must be very active to earn twelve shillings a week; while a mule-spinner, in the same time, can earn thirty shillings. In most of the mills, this difference in wages will cause a proportionate means of expenditure; the one with twelve shillings a week cannot furnish and support a house in the same manner as the other with his thirty shillings, nor has he the same power to educate and keep his children from work so long as the other; the consequence is, that the man with his small weekly wages must want, or he must obtain his children employment. In three-fourths of the mills the eight-hours' system is not attended to, on account of the difficulty in forcing the children to school, and the danger in being fined in not strictly adhering to the necessary regulations; so that the child cannot enter the mill to work twelve hours under these circumstances till it hath attained its 13th year. It is impossible for the man with his little income to keep his children without work till they have attained that age; the result is, that the surgeon must suffer by the parents of these children obtaining false ages, in order to obtain their children work, by substituting an older child for one which is younger, in order to give a false certificate. In several instances the applicant has been the oldest of five or six children, and the parent's earnings not more than sixteen or seventeen shillings a week; and in many of these cases I have been compelled, through the present act and its regulations, to refuse a certificate. When refused, the methods mentioned are had recourse to, and when every stratagem fails, the child is generally worked unknown either to the master or surgeon. Where the eight-hours' clause is worked, which enables a child to enter the mills at nine years of age, the parents and children are gainers, by being enabled to bring up a child in a manner suitable to its destined sphere of life; but the necessary schooling restricted by the act is often evaded, and when attended to, not productive of mental edification, for want of competent schoolmasters. From these facts, it will be seen that the general factory community of operatives are not able to keep their children till they arrive at 13 years of age without the children being destitute of clothing and necessary food; and sooner than undergo these domestic privations, some stratagem is used to gain the child employment in the mills.

The question which now devolves is to show that it is remediable—1st, by restricting the moving power in all mills; 2ndly, by making the eight-hours' clause practica-

ble with less trouble; 3rdly, by allowing all persons in the enjoyment of good health the privilege of full hours' labour after completing their 11 years of age, and to be under the periodical *surveillance* of specially appointed medical men, who may be vested with full power to turn out any child or person from the mills who is not in the enjoyment of good health. Such regulations would relieve the master from the fines which are from time to time imposed upon him, as well as the specially appointed surgeon the unpleasant duty of refusing children a certificate of age for work when physically deserving, and the operative from starving his child, or working it without a certificate of age, and under the liability of being fined; whilst it would divest the mills now employed of those who are the martyrs of phthisis, scrofula, &c., and who can be employed through the present act when they have attained the necessary age.

Should these observations meet your approbation, an insertion in your widely circulated journal would be deemed a favour by, sir,

Your very obedient servant,

JESSE LEACH, M.R.C.S.

Heywood, Lancashire, March 17th, 1838.

MEDICAL CLUBS.

To the Editor of the Medical Gazette.

SIR,

PERMIT me, through the medium of your excellent journal, to invite the attention of the profession to the disreputable and delusive system of "medical clubs." I find, in the advertisement for surgeons to the Bradfield Union, the following condition:—"No medical gentleman will be appointed unless he agrees to establish a medical club in every district which he undertakes;" which means, literally, "We (the Somerset House potentates) will entrap you (surgeons) into the formation of medical clubs, which will enable us to carry out our avowed intention of dispensing with district surgeons, and thereby to effect a saving to the rate-payers at your expense." "Medical clubs" have been formed in this town, and in several of the surrounding villages; and in order that I might inform myself of their tendency, both with reference to patient and practitioner, I have closely watched their operation. Now, when I state the terms of these clubs, I think, sir, it would be difficult to imagine a position more humiliating than that of a surgeon who can set such a price upon his services.

The rate of contribution to these clubs varies from a farthing to a penny per week each person: in consideration of which, the surgeon promises to furnish necessary medicines and attendance. Now, it so happens that several of these clubs are located in villages varying from three to seven miles from the surgeon's residence. Can any one, therefore, assert that he is enabled to fulfil his contract without incurring a loss? And if he fails to do so, is he not morally guilty of trifling with his patient's life? It is curious that, whilst petitions are pouring in upon the legislature, some of them praying for revision of the medical portion of the poor-law, others demanding a means of suppressing quackery, whilst a third party are denouncing our corporate bodies for their apathy in their welfare—I say it is remarkable that, whilst urging the reform of these grievances, many of our members are establishing a system of "quackery" of the lowest grade—a system, as I shall presently show, far more injurious and fatal to the profession, and at the same time more delusive to the poor, than any thing emanating from the Poor-law Commissioners. I inclose you a copy of the rules of the "Banbury Medical Club," under which you will perceive that the surgeon is bartering his drugs and services at a remuneration scarcely exceeding that which he receives for the pauper; and as some of these clubs are even lower in the scale than the one alluded to, we may safely assert that in some instances the remuneration is less than that named. Now, after a consideration of these facts, can we wonder at the lethargy of our Halls or Colleges, or can we feel surprise at the treatment we have experienced from the Poor-law Commissioners? Is it to be supposed that any liberality towards the profession will proceed from the legislature or commissioners, whilst many of our established surgeons are setting forth their ability and inclination to supply medical relief to the independent artizan and labourer at the rate of a farthing per week? I have heard surgeons assign two reasons for their attachment to these "clubs." The established practitioner says, "If I refuse the office, some other person will accept it, and by walking over my beat, he will be likely to deprive me of my private practice." But I should say, if a young beginner, and a stranger too, could succeed in doing this, the affinity between patient and surgeon must be weak indeed; they must be connected by a very fragile tie. The aspirant for practice says, "I take the club as a stepping-stone." But is he aware of the prejudice of the public in favour of the "family doctor?" Is he acquainted with the aversion evinced by patients to new surgeons, more especially towards the junior members? I would,

moreover, remind him that an imprudent competition will never serve his cause, nor will the cheapness of his drugs or services be any recommendation in the eyes of the public.

And now I would ask, how can a surgeon, attached to these degrading clubs, regard the oath taken by him at the College? And further, does he suppose that we shall continue to be respected by other liberal professions, if we permit ourselves to be made the tools of club committees, who assume to themselves the right of dictating to us the terms on which we shall attend our poorer patients? If "medical clubs" should give satisfaction to the poor, they will extend gradation to the next class of patients, with, perhaps, the substitution of pence for farthings; and if we look at the hundreds of surgeons in the manufacturing and other large towns, whose patients are almost exclusively composed of small tradesmen and the better class of artizans, is it not certain that if you compel them to practise under this fraudulent system, you will bring down irretrievable ruin on this deserving portion of our profession? Every man has a right to be charitable, to give his services, and, if he think proper, his medicines likewise; but he has no right to establish a competition which, whilst it cannot benefit any one, may prove the ruin of many. If a surgeon says he can physic his patients at a farthing per week, his less fortunate neighbour will be expected to do the same; it therefore behoves those of the profession on whom fortune has smiled, so far from fostering, to resist so iniquitous a system; they may, perhaps (for a time), lose a few worthless patients, but by steering an independent course, they will ultimately triumph over their neighbouring delinquents; they will enhance their reputation in public estimation, and merit the respect and admiration of the profession.

THETA.

Banbury, March 24th, 1838.

SUPERNUMERARY KIDNEY.

A SAILOR, aged 30, died of typhus fever in an hospital at St. Petersburgh. On post-mortem examination, his left kidney was found to be of extraordinary size; but its situation, colour, and consistence, as well as the insertion of its vessels and nerves, were regular. The right kidney, which was also healthy, was situated opposite to the left one, on the lumbar portion of the diaphragm, and the quadratus muscle. Its ureter, which was smaller than that of the left kidney, passed down in the regular way as far as the bifurcation of the aorta. There a third kidney was found, situated on the right iliac artery, and the psoas muscle. It was larger than the right kidney, and of an oval shape; its two extremities were a little flattened or dilated; the anterior surface was convex, the posterior one flat.

The ureter of the right kidney passed through a longitudinal furrow in the supernumerary kidney, and joining the ureter of the latter (which was larger than usual) entered the bladder at the ordinary spot.

The supernumerary kidney received three arteries; one came off directly from the aorta, another from the right iliac, and the third from the hypogastric. One of its veins entered the vena cava ascendens, the other the right iliac. Its nerves were derived from the inferior mesenteric plexus, and interlaced with those of the right spermatic plexus, and the renal plexus.

The structure of the kidney in question was, in every respect, regular. The weight of the three kidneys was as follows:—

Right kidney more than 8 ounces.
Left kidney ..... 3½ ounces.
Supernumerary kidney 5 ounces.

The left kidney was 4½ inches long, and nearly three broad. The right was 3½ inches long, and nearly 3 broad. The supernumerary one was 3½ inches long, and more than 2½ broad. The right and left kidneys were provided with a suprarenal gland; the third one had none. The bladder and the rest of the genito-urinary organs were normal.—*Gazette des Hôpitaux*, Feb. 20th, 1838.

PARISIAN HOSPITALS.

THE budget of the hospitals and asylums (*hospices*) of Paris, for the year 1836, amounted to eighteen millions of francs (720,000*l*.) Of this the town gave, as its ordinary contribution, 5,300,000 francs (212,000*l*.) The tax upon the theatres produced 660,000 francs (26,400*l*.) The rest was furnished by freehold property, the interest of money and stock, and legacies. The average daily cost of a patient in the hospitals, is 1 fr. 67 centimes (16 pence); but it varies extremely in different ones. At the Hôtel-Dieu the average is 1 fr. 62 c. (15½ pence); at St. Louis, 2 fr. (19 pence); at the Cochin Hospital, 1 fr. 90 c. (18 pence); at the Clinique, 2 fr. 75 c. (two shillings and twopence). In 1816 [1836?], 81,996 patients were admitted into the hospitals; and 13,820 persons were maintained in asylums. The mean mortality of the hospitals is 1 in 11. —*Gazette des Hôpitaux*, March 1, 1838.

DR. M. HALL AND PROCHASKA.

To the Editor of the Medical Gazette

SIR,

You are mistaken in supposing that Dr. M. Hall has made no mention of Prochaska, he having referred to that physiologist in one of his published lectures.

Your obedient servant,

JUSTITIA.

April 9, 1838.

[Mr. George, in the valuable paper begun in our last, and concluded in our present number, stated that Dr. M. Hall, in his History of the Opinions of former Physiologists, had not mentioned Prochaska. We echoed the statement, and now take leave to repeat it, believing it to be perfectly correct. It is notorious that Dr. M. Hall, in the hundred-and-one papers he has written in exposition of *his own* (?) views, has completely given the go-by to Prochaska, who it appears had long before promulgated precisely similar doctrines. What he once quoted Prochaska for, was to illustrate a point connected with anatomy, in which he (Dr. M. H.) wished to controvert an opinion of Sir Charles Bell; and the circumstance is only valuable as affording unanswerable proof of the work having been known to him.—ED. GAZ.]

INSTRUMENT FOR EXTRACTING CATARACT.

To the Editor of the Medical Gazette.

SIR,

MR. MIDDLEMORE, in his communication in the last week's GAZETTE, asks if any of your readers could suggest an instrument better adapted for the purpose than the one he describes, for extracting an opaque capsule through the sclerotica. I may mention that the instrument invented by Sir James Earle (An Account of a New Mode of Operation for the Removal of the Opacity in the Eye called Cataract, London, 1801), and which is essentially the same as Mr. Middlemore's, was found by Sir James to answer very well. Sir James gives a figure of his instrument, and also of a director to facilitate its introduction into the eye, when that is necessary.—Yours,

* * * * *

April 9, 1838.

Sir James Earle's description of his instrument is the following:—"It consists of a small spear-pointed lancet, of a proper breadth, which introduces a pair of fine forceps into the globe of the eye; and when sufficiently inserted, the sharp or spear-point, by means of a spring, is withdrawn, leaving the forceps behind; with these the cataract may be gently seized, made to quit its connexions, and be brought away through the opening; and thus is completed the whole of the operation."—p. 32, 33.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *Apr.* 10, 1838.

Abscess	1	Hooping Cough	8
Age and Debility	39	Inflammation	20
Apoplexy	5	Bowels & Stomach	3
Asthma	7	Brain	7
Cancer	3	Lungs and Pleura	14
Childbirth	5	Insanity	2
Consumption	43	Liver, diseased	2
Convulsions	18	Measles	4
Croup	2	Mortification	3
Dentition or Teething	2	Paralysis	3
Dropsy	9	Small-pox	7
Dropsy in the Brain	6	Spasms	1
Dropsy in the Chest	2	Tumor	1
Fever	21	Unknown Causes	94
Fever, Typhus	9		
Gout	1	Casualties	3
Heart, diseased	2		

Decrease of Burials, as compared with the preceding week . . . 23

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

March.	THERMOMETER.		BAROMETER.	
Thursday . 29	from 27	to 59	30·44	to 30 48
Friday . . 30	42	59	30·41	30·25
Saturday . 31	35	51	30.15	30·08
April.				
Sunday . . 1	21	43	30·08	30·10
Monday . . 2	*13	47	29·93	29·87
Tuesday . . 3	22	43	29·87	29 89
Wednesday 4	35	56	29·89	29.90

Winds very variable.

Except the 31st ult. and afternoon of the 2d and 3d instant, generally clear. A little rain on the evening of the 3d.

* The low state of the temperature on the morning of the 2d is worthy of particular remark.

CHARLES HENRY ADAMS.

NOTICES.

Communications from the following gentlemen have been received, and are preparing for publication:—Dr. Thompson, Mr. Jeffreys, Mr. Saunders, Mr. Grant Calder, Mr. Taylor, Dr. Wilson (of Middlesex Hospital), Dr. Wilson, R.N., Dr. W. Griffin, Dr. D. Griffin, Mr. Mayo, Dr. Fergusson, Dr. Ward, Mr. Sandall, "K.," "A Retired Physician," "Philomathes."

We cannot insert Mr. Holloway's letter.

Mr. Harvey's paper is under consideration.

Dr. C. Orpen's letter, for the *extra limites*, has been received.

We shall be glad to receive the promised continuation of Dr. Burne's papers.

ERRATUM.—In Mr. George's paper in our last number, after line 1, p. 41, read the three lines at the top of the following page.

WILSON & SON, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 21, 1838.

LECTURE

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine,

By Southwood Smith, M.D.

Physician to the London Fever Hospital.

Lecture XI.

Relation of Epilepsy to Forensic Medicine—Epochs of Life peculiarly subject to Epilepsy—Circumstances under which a fatal Epileptic Paroxysm may occur—Seats of Irritation inducing the Paroxysm—Congestion of the Brain and its Membranes—Morbid Deposition of Bony Matter—Enlargement of the Glandulæ Pacchioni—Tumors of the Brain and its Membranes—Mechanical Violence—Sources of Irritation distant from the Brain—Diseases in the Spinal Canal—Inflammation of the Spinal Cord—from External Violence—Effusion of Serum between the Dura Mater and the Vertebral Canal—Extravasation of Blood.

Relation of epilepsy to forensic medicine.—Violent convulsions, attended with loss of consciousness, recurring at intervals often with great regularity, sometimes after the paroxysm is over, leaving no trace of its existence on body or mind; yet while the fit is on, producing effects which terrify and appal every beholder: a disease distinguished by characters so obvious and peculiar it would seem impossible to overlook, mistake, or confound with the effects of poison, or with any conceivable results of external violence. But epilepsy, though it be usually slow in destroying life, and be very seldom fatal in the first attack, is yet frequently fatal in some one attack; and every now and then it is fatal in the very first attack. Some one of the many states of the system on which epilepsy depends, may be slowly and insidiously developed, giving no indication of its presence until the fatal paroxysm supervene quite suddenly; or such a state of the system may be rapidly developed, a person falling suddenly from a state of apparent health into deep sopor with convulsions, from which condition he never recovers. Hence, fatal epilepsy may be a very acute or a very chronic affection, and in either case it may occasion death almost as instantaneously as apoplexy. In its ordinary forms, epilepsy can neither be overlooked nor mistaken; but this is the very reason why its less common forms deserve, in relation to forensic medicine, a special study.

Epochs of life peculiarly subject to epilepsy.—There is no age exempt from epilepsy. It may occur in the infant at the mother's breast, and in the old man of seventy; still there are certain epochs of life connected with essential changes in the development of the frame, at which the disease is peculiarly apt to occur. When the food of the infant is changed from the exquisitely elaborated milk of the mother, to the coarse and irritating aliment which is almost invariably substituted for it, the irritation of the alimentary canal excites irritation in the central masses of the nervous system, and induces epilepsy. The pressure of the tooth against the gum in the first dentition; the like pressure in the second dentition; the change that takes place in the constitution of the female on the first appearance of the catamenia; the difficulties attendant on the establishment of that secretion in a regular and efficient manner; the important changes that take place in both sexes at the age of puberty; the changes that still go on during the period of adolescence; the exertions and the excesses of manhood, act powerfully either as predisposing or as exciting causes of epilepsy, and render these several epochs

of life peculiarly subject to this malady. Nevertheless, the susceptibility to the disease does not cease at the age of manhood. After the lapse of years morbid changes take place in the structure of the arterial and venous systems; slight causes are then capable of disturbing the nice balance of the circulation; a disturbing agent which might have been easily resisted when the frame was vigorous, may produce irreparable mischief in the relative weakness of declining age; and hence a fatal paroxysm, perhaps the very first attack of epilepsy, may occur at sixty, seventy, or even a much later period.

Circumstances under which a fatal epileptic seizure may occur. — Now when epilepsy comes on at these comparatively unusual periods, and in the rare cases in which it proves fatal in the first attack, the very mode in which the paroxysm is likely to occur, may induce, or may give a colour to circumstances which have induced, a suspicion of poisoning or of violence. It is so common for the first attacks of epilepsy to occur in the night and during sleep, that the disease is sometimes established a considerable time before its existence is known or suspected; the seizure is at length discovered by accident, or by the increasing severity of the malady, in consequence of which the recurrence of the paroxysm is no longer restricted to the state of sleep. A person retires to bed in his usual health; he is found the next morning dead in bed; the cause of his death may be an attack of epilepsy which has now supervened for the first time. Or after some slight and disregarded indisposition, while dressing himself, in the act of putting his hands into cold water; or while at dinner, in the act of putting a morsel into his mouth; or while sitting quietly in a state of repose, soon after having taken a hearty meal, a person suddenly loses his recollection, falls convulsed to the ground, and remains in a state of profound sopor, from which he never recovers. This happens under circumstances which give rise to a suspicion of poisoning or of violence. You are called to investigate the case. The idea of epilepsy does not occur to you. Nothing can be more unlike the usual mode in which epilepsy attacks and kills; the repeated and regular paroxysms, so characteristic of this disease, cannot but have made the friends and connexions of the sufferer from it well aware of his malady. It is, then, highly important in a judicial point of view, that you should bear in mind that these unusual circumstances are, nevertheless, the very circumstances under which the most formidable and hopeless epileptic seizures do occur. Moreover, a fatal epileptic seizure may happen to a person at a distance from his home and friends: when no one is near who can give any account of him; in a lonely situation, where he may receive from the fall, external injury itself sufficient to account for death. Under such circumstances it may require very careful consideration to discover the true nature of the case.

Seats of the nervous irritation which induces the paroxysm. Congestion of the brain and its membranes.—That epilepsy is the result of nervous irritation is proved by the whole concourse and character of the symptoms, as well as by the mode in which the paroxysms approach and subside; and inspection after death often demonstrates the seat and source of that irritation. In some cases nothing can be detected but cerebral congestion; and the indication of this may vary from a very slight to an extreme degree of vascularity. Often, just as in apoplexy, the blood-vessels of the membranes, or of the substance of the brain, are numerous, enlarged, and turgid, giving in a high degree the appearance of purple mottle or marbling, as you see represented in the drawings before you.

Morbid deposition of bony matter.—In the epileptic there is often a remarkable tendency to a morbid deposition of bony matter. The whole calvaria is sometimes singularly solid and heavy. You may find it three or four times the natural thickness, the capacity of the skull being thus very materially diminished; and there can be no question that the morbid action which produces this induration, solidification, and thickening of the bony parietes of the brain, is communicated, in a greater or less degree, to the brain itself. Occasionally the whole exterior of the calvaria presents an irregular surface, like that of embossed work; at other times the deposition of bony matter takes place principally in the situation of the sutures; spiculæ of bone shoot out in different places, chiefly from the inner table; the greater part of the internal surface becomes irregular: in some places there are convex prominences, and in others corresponding depressions; while the natural prominences and depressions are increased, and all are rendered scabrous, sharp, and rough. Sometimes the deposition of bone is confined to the membranes of the brain, and more especially to the dura mater and its productions, as the falx, of which you see in the specimens before you striking examples.

Enlargement of the glandulæ Pacchioni.—Sometimes, connected with epilepsy, there is obstruction to the due return of the blood, by an exuberant growth of the glandulæ Pacchioni, which compress the longitudinal sinus, or choke its channel,

and which thus give rise to congestion of the veins.

Tumors of the brain and its membranes.—There is a close connexion between epilepsy and the formation of tumors of various kinds in different parts of the brain and its membranes. "In passing through one of the wards of the hospital (says Dr. Bright), I was attracted to the bed of a patient by a kind of sobbing sound, with a half snoring noise. I found her strongly convulsed, lying on her back, with her face turned to the left side. She was violently agitated, like a child in convulsions; her head drawn back in twitches; her eyes turned to the right side; her legs and arms, more particularly the latter, stretched with convulsive catches In the space of an hour she expired. She had been night-nurse in the ward about four months; had made frequent complaints of headache; about a month before her death, was found one day at the sink, where she had been for water, quite motionless; but she soon recovered, and was always an efficient nurse. About a week before the fatal attack, she said she felt giddy; and three days after this sensation of giddiness, on rising from her chair to pour out some water, she fell senseless to the ground, where she was found struggling, with her head under the grate; against the bars of which she had struck her forehead, which afterwards occasioned a black-eye. The next morning she was seized with another fit, and from this period till her death she fell into a succession of fits, being perfectly sensible between the attacks, but confined to her bed.

"On inspection of the body after death, there was found ecchymosis round the right eye, a bruise on the right side of the forehead, with ecchymosis beneath the scalp, of the size of a half crown, and some marks on the hairy scalp about the same part, which appeared the result of some remedial applications formerly used; the left foot drawn inwards and extended, as from convulsion; the right foot in the same way, but less complete. On removing the calvaria, the dura mater appeared more flaccid than usual, and on the anterior part of the right side a number of bleeding vessels were seen with small drops of blood, where they had been divided from the calvaria. This appearance was not presented at any other part, though the whole was rather vascular. Raising the dura mater on the left side, the arachnoid had a considerable quantity of serum beneath it. On the right side the same was observed; but after raising a very small portion, about one-fourth of an inch from the division of the hemispheres, the dura mater was firmly glued to the arachnoid, and this again to the substance of the brain. This adhesion extended over a space about the size of a crown-piece or more; and on using a little force to draw up the dura mater, fungous granulations of the size of peas came into view, apparently arising from the arachnoid of the dura mater; for it was not difficult, with the back of the scalpel, to detach them from the fibrous dura mater, while at the same time the arachnoid covering the brain was entire. Such was the state of those granulations first raised; but as we proceeded further, the fungous growths were more firmly attached to the brain, which tore up with the granulations. None of this disease, however, went more than one-fourth of an inch deep; and in the centre, on cutting towards the dura mater from below upwards, the medullary matter, for the depth of half an inch and about the same extent, was quite softened down, and infiltrated with serous fluid, which seemed to divide the fibres and produce a little aqueous softening, without any defined cyst or margin. On taking off a slice of the brain, the bloody points were very numerous, and the cortical substance was every where vascular. The ventricles contained too much serum, which was very limpid; and the vessels on the lining of the posterior cornu were of unusual size: in the plexus were a few small vesicular bodies. The cortical part of the corpora striata exhibited a most unusual red mottled appearance. In the basis a considerable quantity of serum had accumulated."

Mechanical violence.—Epilepsy is often induced by external violence, terminating in organic disease of the brain. In this case epileptic seizures take place more or less severely, for a shorter or longer period; and at length, when the health is completely broken up, the patient expires in a violent fit of convulsions. However sudden the death, or however obscure the previous history of the case, you will often be able to detect its true nature, and to trace it to its true source—mechanical injury. A man, 55 years of age, had injured his skull when a boy, in consequence of a fall on his his head from a height of about forty feet: several fragments of bone had been removed. There were observable on the skull two depressions, each large enough to admit the point of a finger. Subsequent to the exfoliation of bone, he had been subject to epileptic fits, at first frequently—three, four, five, six, in the day; but they gradually diminished in frequency, and only occurred occasionally at irregular periods. About a month before his last illness he had two fits in one day. He now became dropsical, and fell into coma, with convulsions, in a violent

paroxysm of which he died. On the exterior of the skull two depressions were to be remarked, in which the external table of the bone appeared to have been lost. On raising the calvaria, a distinct membrane was left behind, attached to the bone, about half the usual thickness and strength of the dura mater, but rough and woolly in its texture; this was more or less spread over the whole calvaria, but was particularly strong near to the places where the skull had been injured. On stripping this membrane from the skull, the bone was found to be rough and porous, and in two places corresponding to the situation of the external injury there was evidence that the mischief had extended to the inner plate, but that it had been repaired without leaving much inequality on the internal surface. The dura mater was adherent to the arachnoid over the greater part of its extent. There was a small plate of bone in the anterior part of the falciform process. The arachnoid was remarkably thick and opaque, and the contiguous surfaces on the internal sides of the hemispheres adhered so firmly, that they could with difficulty be separated on their anterior parts. The arachnoid and pia mater were detached in one large, firm, opaque sheet, from the right hemisphere, but when it was attempted to do the same from the left, it was soon found that on the whole lateral portions of the hemisphere, particularly on the middle and anterior lobes, the adhesion of the membrane to the cineritious substance was so complete that all the tops of the convolutions tore away with it; and the cineritious matter was completely removed, the portion of brain immediately under being rather harder and more brittle than the rest. The convolutions at this part were very shallow, and seemed to adhere together, the whole of that portion of the hemisphere thus bearing a very unhealthy aspect. The substance of the brain was not unhealthy, but in the ventricles there was a slight accumulation of clear fluid; and on the posterior part of each choroid plexus was a round mass of bony hardness. The lungs did not collapse when the chest was opened, being slightly emphysematous, and loaded with œdematous effusion, which poured out in streams from the large bronchial tubes when the lung was squeezed. The liver was pretty natural, but slightly inclining to granulation; the spleen small, and its external surface rough with slight cartilaginous deposits; the intestines tolerably healthy. The kidneys presented a most illustrative specimen of the hard granulated change which those organs undergo.

Sources of irritation distant from the brain.—But you must bear in mind that the cause of epilepsy is not always seated in the head. You may find the brain, its membranes, and its bony parietes, perfectly healthy; the fatal epileptic seizure may have been induced by irritation in some distant organ. In a person predisposed to epilepsy, a hearty meal of indigestible food is capable of producing a violent epileptic seizure, which may come on in five minutes after the stomach has become thus loaded, and may prove rapidly fatal. In this case you will discover the cause of death in the quantity of indigestible articles present in the stomach. The presence of morbid matters in the intestines may produce a like fatal seizure, and more especially the presence of worms, which not unfrequently occasion fatal paroxysms of epilepsy both in children and adults. The number of worms which are sometimes found in these cases is astonishing. In a girl, seven years old, eleven lumbrici had made their way through the coats of the intestine into the general cavity of the peritoneum, and the coats of the stomach were also perforated with holes, in some of which other worms were sticking. Pressure upon the trunk of a nerve is capable of producing fatal epilepsy. A stout young woman became suddenly liable to epilepsy; for the space of twenty months she suffered from repeated fits; at last she died suddenly, comatose. On the examination of the body, particular attention was directed to one of the lower extremities, because the fits had always begun with acute pain in a particular part of the thigh, and here a bony tumor was found as big as a nut, pressing upon a branch of the sciatic nerve. Nothing is more common than epileptic seizures in children from the irritation of teething, and in females from uterine irritation, the accession of the catamenia sometimes regularly inducing the disease. The recollection of these circumstances will often save you from serious error, and guide you to an acute detection of the true nature of an obscure and doubtful case.

Diseases in the spinal canal.—But after the most diligent search, in a fatal seizure which you have every reason to believe has been really epileptic, you may be able to find no appreciable disease, either in the bones of the skull, or in the membranes of the brain, or in the brain itself, nor in any one of the thoracic or abdominal viscera. Where, then, are you to look for the cause of death, the seat of the fatal disease? In the vertebral canal and the spinal cord. Distinguished physiologists, accurate and acute observers, whose hours have been spent at the bed-side of the sick, believe that all convulsive diseases have their true and proper seat in the spinal cord, and

that when some morbid condition of the brain is the manifest cause of epilepsy, the convulsive paroxysm is induced indirectly by the transmission of the irritation to the spinal cord. "All convulsive diseases," says Dr. Marshall Hall, "are affections of the true spinal marrow. The cerebrum is the seat of the mind, neither sentient itself nor the originator of motions in itself. The true spinal marrow, on the contrary, is the term of certain excitements and the source of certain motions—the centre, in a word, of a peculiar series of excito-motory phenomena, physiological and pathological. Unlike the cerebrum, it induces, if stimulated, convulsive movements of the organs appropriated to ingestion and egestion, and of the limbs. Diseases within the cerebrum, by irritating excitor nerves or the medulla oblongata, induce convulsions or epilepsy. Diseases within the spinal canal may prove the source of convulsions or epilepsy still more immediately." Certain it is that diseases within the spinal canal are the frequent causes of sudden death—causes which have been very much overlooked, and which, in reference to forensic medicine, deserve particular attention.

A person is seized suddenly with coma, accompanied by violent convulsions, and followed rapidly by death. You are able to learn nothing of the previous history of the case. On the contrary, the symptoms have been accurately observed; there has been pain more or less severe, extending along the spine, perhaps increased by motion, and little felt except on motion, attended with some degree of pain and rigidity of the limbs, and in the worst cases with tetanic contractions of the muscles of the back and neck, and transient disturbance of the respiration. On a careful inspection of the brain and of all the viscera, you are able to detect not the slightest trace of disease in any organ. Now, lay open the vertebral canal; you will find the spinal cord in different parts of its course involved in inflammation, the cord itself and its membranes vascular, thickened, and covered by coagulable lymph, or by a layer of pus. Here, then, is an adequate cause of death, and a natural cause of sudden death with convulsions.

Inflammation of the spinal cord.—A boy, 7 years of age, had been slightly indisposed for two days with headache and feverishness, for which he took purgative medicine, and on the morning of the third day seemed almost well. About two o'clock in the afternoon of that day he was seized with severe and general convulsions, and remained confused, incoherent, and partially comatose. He recovered in some degree from the coma; but on the third day from the convulsive attack he was seized suddenly with another violent paroxysm, in which he expired. Every part of the brain was found in the most healthy state; but on its removal a considerable quantity of fluid flowed from the spinal canal; and on laying open the canal, there was still a good deal found between the cord and the external membrane. The cord was healthy at the cervical portion; but in the upper part of the dorsal region it was remarkably softened and broken down. This appearance extended for several inches, but varied in degree. At one place a complete separation took place, in attempting to raise the cord, the part falling down into a soft diffluent pulp through its whole diameter. From the middle of the dorsal portion it was quite firm and healthy. The inner membrane of the cord was dark-coloured, highly vascular, and shewed evident marks of inflammation at the part corresponding with the softened portion of the cord.

Many analogous cases are on record, in which persons previously in apparent health have died in the course of three days with violent convulsions, without disease of the brain, from acute inflammation of the spinal cord—inflammation detected in some part of its course, and accompanied with softening of its substance. It is true that organic disease of the spinal cord is very frequently a chronic affection, attended with symptoms which manifestly point to the seat, and which clearly denote the nature of the disorganization that is going on; but the thorough knowledge of the acute diseases to which the spinal cord is subject, will sometimes enable you to make an accurate discrimination, and to form a clear and correct judgment, of the true nature of a case which would otherwise be involved in impenetrable obscurity.

From external violence.—It is important, in connexion with this subject, to keep in view the strictly analogous diseases of the spinal cord produced by external violence. A waggoner, sitting upon the shafts of his cart, was thrown off by a sudden jerk, and pitched upon the back part of his neck and shoulder. He complained of nothing except stiffness in the back part of his neck; he could move all his limbs with freedom. In a few days he was seized with general convulsions and locked jaw. After a few hours he was seized with a singular convulsive movement in the jaw, which continued in a state of violent and incessant motion for about five minutes. This was followed by maniacal delirium, and he then sunk into a state resembling typhus fever, in which he died. On dissection a great quantity of purulent mat-

ter was found within the spinal canal, which had dropped down to the lower part of it. It appeared to have been formed about the last cervical and first dorsal vertebræ, and at that place the intervertebral cartilage was destroyed, so that the pus had escaped outwards among the muscles.

A man having suffered an injury of the spine by a fall, ten months after the accident had prickling of the left lower extremity, which ceased after a month, leaving it paralytic. The right lower extremity became affected in the same manner, and some projection of the vertebræ at the affected part was then first observed. He had afterwards convulsive motions of the limbs, and died about a month after the appearance of the palsy. On dissection there was found false membrane of an inch in extent, with purulent matter betwixt the bone and the membranes of the cord at the affected part; and the portion of the cord included in the diseased membrane was entirely diffluent, like cream. Above and below this portion the cord was of a natural consistence, but highly injected, and of a deep red colour, especially below the softened part, where the redness was intense.

The cases hitherto recorded of undefined suppuration and abscess of the cord have been of a chronic nature, attended with paralysis.

Effusion of serum between the dura mater and the vertebral canal.—Sometimes death takes place very rapidly, the patient having been seized with violent convulsions and coma. On inspection no disease is discoverable in the brain, but a quantity of fluid is found effused between the dura mater and the canal of the vertebræ. A child, aged twelve months, after appearing to be in much pain, lost the use of the inferior extremities, and died in three days. The spinal canal was found full of bloody serum. A strong healthy child, two years old, after having been oppressed and feverish for two days, was seized with violent convulsions. The first fit continued about an hour, and left her comatose, with distortion of the eyes. She had not recovered out of this state when she had another attack of convulsion, about two hours after the first. The second fit left her in a state of coma, from which she never recovered. She took food or medicine when they were put into her mouth, but shewed no other appearance of sensibility. The eye was completely insensible, and the pulse very frequent. She had afterwards several slight attacks of convulsion, and one more severe, a short time before death, which happened thirty-three hours after the first attack.

No disease could be detected in the brain except some appearance of increased vascularity, and slight effusion under the arachnoid. The brain and cerebellum being removed, there was a copious discharge of bloody fluid from the spinal canal. The canal being laid open, there was found a copious deposition of colourless fluid, of a gelatinous appearance, betwixt the canal of the vertebræ and the dura mater of the cord. It was most abundant in the cervical and upper part of the dorsal regions. Between the membranes of the cord there remained a small quantity of the bloody fluid which had flowed into the cavity of the cranium. The substance of the cord at the upper part seemed softer than natural, and very easily torn. All the viscera of the thorax and abdomen were perfectly healthy.

Extravasation of blood.—A person may be seized suddenly with a fit of convulsions, and may die as rapidly as from an apoplectic seizure. There may be no disease of the brain, nor of any viscus; but on examining the spinal cord you may find in it a regular apoplectic cell, containing a clot of blood mixed with the broken-down matter of the cord. It may be a case of spinal apoplexy. A child, seven days old, after slight apparent indisposition, was seized on the third day with convulsion, which recurred at various intervals, sometimes in the form of tonic spasm of the whole body, and sometimes of violent convulsive agitation. On the fourth day the convulsion continued, and he died in the afternoon. No disease could be detected in the brain. In the spinal canal there was a long and firm coagulum of blood lying between the bones and the membranes of the cord on the posterior part, and extending the whole length of the cervical portion.

A young lady, aged 14, had headache and pain in the back, with a tendency to sickness on sitting up. At the end of a week there was a sudden and violent aggravation of this pain, followed by general convulsions, which were fatal in six hours. The spinal canal was found filled with extravasated blood in the lumbar region, which had been the seat of the pain. The brain and all the other viscera were sound.

A miller, in lifting a heavy sack, suddenly lost the use of his lower extremities, and died in fifteen days. Extravasated blood was found mixed with sanious matter in the spinal canal. The membranes were inflamed, and the nerves of the cauda equina appeared rotten, as if they had been long macerated in putrid water.

From external violence.—Spinal apoplexy producing sudden death, may be the con-

sequence of external violence. A man received a violent blow on the three inferior lumbar vertebræ, by a log of wood which fell upon him, and he died in four hours. Extravasated blood was found in the spinal canal, but the vertebræ were entire, and the cord was healthy.

A boy, aged 14, received a violent jerk of his neck, by a cord which was thrown over his head as he was swinging forward in a swing. He felt no bad effect at the time, but after some time became inactive and weak in the limbs, with stiffness of the neck, and difficulty in moving his head. Nine months after the accident the weakness of his limbs increased to paraplegia, and soon after he had paralysis of the arms, with retention of urine. He had been a short time in this state when he was seized with violent pain in the spine; he then had difficult and quick breathing, which was first observed during sleep, but afterwards continued while he was awake; and he died after suffering severely from it for two days. His death happened about ten months from the injury, and a few days after the violent attack of pain in the spine. A large quantity of extravasated blood was found in the spinal canal, betwixt the bone and the theca vertebralis. It was partly coagulated and partly fluid, and appeared to have come from the upper part of the canal, about the second or third cervical vertebra.

The spinal cord may become indurated, and changed into a substance of the consistence of cartilage; new formations, as fleshy or albuminous growths, tubercles, hydatids, and depositions of bony matter, may compress it, or a portion of it may be destroyed, so that, for the space of two or even four inches, it may be entirely wanting; but these affections, which are uncommon, when they do occur are generally of a chronic nature, and not likely to be attended with circumstances which call for judicial investigation. With this, then, I conclude an account of sudden death from natural causes, as far as those causes are connected with the nervous system. In relation to all the diseases which have hitherto come under our consideration, I cannot too strongly urge upon you the study of the Pathological and Practical Researches on Diseases of the Brain and Spinal Cord, by Dr. Abercrombie, and the second and third volumes of Medical Reports, by Dr. Bright; works which contain an inexhaustible fund of information, and which are easily accessible to you all. There remain to be considered certain diseases of the thoracic and abdominal viscera, which destroy life even more rapidly than apoplexy, and which assume a peculiar interest in relation to Forensic Medicine.

CLINICAL OBSERVATIONS

ON THE PRESENT

TENDENCY TO PHLEBITIS;

Particularly in Young Women.

WITH SOME REMARKS ON ANASARCA.

By John Wilson, M.D.,

Physician to the Middlesex Hospital.

[Concluded from page 100.]

Two months ago we had a case of dropsy in a man, with orthopnœa, purpura hæmorrhagica over the legs and thighs, urine with a coffee-ground like deposit, and albuminous, all of long duration. There was found great hypertrophy of the heart, with much valvular obstruction; red and grey hepatization of the lungs; kidneys small, pale, not granular, and almost impermeable to injection.

In the child's case, (Edwards) the kidneys were large and congested generally, and had black spots externally and internally, with fluid in both the thorax and abdomen. Before death, purpura hæmorrhagica appeared, and she passed all under her, so that we did not see the urine; but the fæces were mixed with blood. Here, again, the kidneys were but little pervious to injection.

Dr. Bright has given a plate of a dark chocolate-coloured kidney, from being gorged with blood, of a man who died at the end of a month, of sudden anasarca, and who, from the time of admission till his death, three weeks, had hæmaturia; and he died suffocated by œdema of the epiglottis, from inflammation; but here the kidney was not injected.

May not inflammatory dropsy, or sudden anasarca, arise from cold and moisture applied externally, and thus, by determining the blood internally, cause congestion or inflammation of the kidneys; as we so easily admit it may do in various other internal organs, as the lungs, serous and mucous membranes, spleen, liver, and encephalon?

For the kidneys in hæmaturia, like the lungs in hæmoptysis, shew signs evident enough of inflammation or deranged circulation affecting the functions of secretion in the kidneys, and respiration in the lungs.

Yet, no doubt, one of the causes of dropsy is dissipation, particularly excess in drinking, which gradually induces

disease of the liver, lungs, and heart, as well as the kidney; and then on exposure to cold and wet, anasarca may appear for the first time,

Also, after the first attack of inflammation of the kidneys, when hæmaturia has ceased, and the health appears to be restored, yet some interstitial deposit may remain; then, perhaps, afterwards, from irregularity in living, or a repetition of another inflammatory attack, the kidneys may assume the granulated structure, or deposition into the cortical part, and greatly diminished permeability, accompanied with albuminous urine, so well described by Dr. Bright, and in which state they seem to be so little influenced by medicine. Hence the importance of ascertaining, if possible, how the incipient disease of the kidney is first induced, which afterwards leads to those irremedial results; and what may be the best remedial agents at its first, and then possibly curable, stage.

May not the kidney, like the lung, become inflamed, and terminate, though rarely, in abscess?

Again, may not the lung be attacked with inflammation producing pneumonia, which may end in resolution with expectoration of blood and muco-purulent secretion? and may not the kidney be attacked with inflammation producing nephritis and anasarca, which may end in resolution with hæmaturia and albuminous urine? Or should death be the result in both cases, in the lung we may find some parts hepatized from deposition, and almost impervious to the circulation of air and blood; also, we may find the kidney, from deposition, but little permeable to the circulation.

Again, may not the lung be affected with phthisis, accompanied by muco-purulent expectoration, with or without blood; and after death, tubercles, hard and soft, may be found, and some forming softened masses in walled cavities, and only a small portion of the lung be left permeable to air and circulation? And may not the kidney, in another state, produce albuminous urine with or without blood, when after death granulations may be found in the kidney, rendering it but little permeable to the circulation?

Thus these two double sets of purifying organs of the blood, the lungs and the kidneys, cease to be able, from their diminished permeability, to continue to render the blood capable of sustaining life; by the formation of tubercles in the one organ, the lungs—and by the formation of granulations in the other organ, the kidneys: but tubercle is a name given to a small, round, consistent, adventitious body, found in the lung; and granulation is a name given to a small, round, consistent, adventitious body, found in the kidney.

Thus, we may have muco-purulent excretion, with or without blood, in tuberculated lungs, incurable: and we may have albuminous excretion, with or without blood, in granulated kidneys, incurable.

Again, may we not have muco-purulent excretion from the lungs, as in bronchitis, curable? and may we not have albuminous excretion from the kidneys, as in scarlatina, curable? as in the following case:—

George Grant, aged seven, came in November last, said to have been ill five weeks; began with redness over all the body, which became as red as scarlet, with a very bad sore throat, cough, trembling and shaking, and very heavy for sleep.

Complained, on admission, of pain in the knees and head, which prevented him from sleeping; knees, legs, and ankles, swollen; urine scanty, muddy, and albuminous.

The œdema of the legs gradually subsided, and the cuticle desquamated. Afterwards he had otorrhœa, and was discharged well at the end of five weeks.

Was the anasarca here the direct consequence of the inflammation of the sub-cutaneous cellular tissue at the time of the cutaneous inflammation; or was it a result after the inflammation had subsided, but had left the cellular tissue in an altered state, less favourable to circulation through the minute capillaries of its arteries, veins, and lymphatics?—or was it caused by the (at least temporary) disordered state of the kidneys?* Or may the effect of scarlatina here rather be considered, like the first effect of inflammatory anasarca arising from cold, as acting upon the skin, suppressing the natural action of the minute capillaries of the cutaneous sur-

* We have now one of the nurses, attacked with erysipelas of the face and part of the scalp, who passed urine like muddy coffee, and loaded with albumen; afterwards it became more thick, and the colour of pea soup, and still loaded with albumen: when the cuticle desquamated, the urine became pale, clear, and free from albumen.

face; and this perverting, as it were, the double function of the skin (that is, its pulmonary and renal-like functions, if we may call them such), by preventing the atmosphere from producing its full influence over the surface of the entire body, and also by preventing the excretion of what constitutes the perspiration in the healthy state; and thus surcharging the kidneys with the elimination from the circulation of that which would otherwise have been excreted by the skin.

That the skin and the kidney can excrete the same morbid production, the following case will show:—

About a year ago we had a case of diabetes mellitus (Bowers): his perspiration, caused by a hot air bath, or sudatorium, was collected by a sponge and water, and afterwards evaporated; and a light-brown deposit remained, which was sweet, and like that obtained by the evaporation of his urine. When he was discharged, he was stronger and better in health than he had been eighteen months previously.

We do not know that it has yet been shown that the skin imbibes any of the atmospheric air, though very possibly it may; and if it should prove so, the skin would then appear to be able to perform, in part, under certain conditions, the double function of the kidneys and lungs, and aid in explaining many of what are now called sympathies.

The skin is allowed to be a secreting, excreting, and absorbing organ, though the pores through which these functions may be affected be not visible; yet it is now known with what facility different gases will pass through various membranes, even through a bladder; how much more easy the transit may be through a membrane like that of the epidermis; and if perspiration, in the form of vapour or fluid, pass through the skin, and, on the body being immersed in an aqueous fluid, part of that fluid be absorbed, how much easier, therefore, may not the body, when immersed in the surrounding atmosphere, imbibe through the skin a fluid so much more rare than an aqueous one.

In beginning the subject of phlebitis, we had no intention of connecting it with anasarca, but we have been gradually led to make the preceding observations, and shall now conclude with some general remarks on the eight cases of idiopathic phlebitis.

It appears that this phlebitis may give origin, on the one hand, to pain and swelling of the extremities, with or without redness; and on the other hand, when not accompanied with swelling of the limbs, to symptoms like those of rheumatism, with pain in the joints, but without swelling or redness of the joints, for the most part; though in many the lower parts of the legs were red, and very painful at the same time; but, sooner or later, the pain extended up the inside of the legs and thighs to the groin, where it usually was the greatest, and sometimes accompanied with red lines, but much oftener without any, and with or without swelling of the legs or thighs.

The two young women who were attacked by phlebitis at the same time, about eight weeks ago, when in the hospital for slight complaints, are still both confined to bed; but latterly they have been up occasionally. These two were in the same ward, attacked at the same time, and have had almost entirely uniform symptoms and variations at the same time; both have had diarrhœa occasionally; both have suffered, at the same time, nearly the same severity of pain in the lower extremities; and when these became relieved, both were seized at the same time with a pain and sense of weight at the præcordia, and great pain below the clavicles, extending over all the arms; but the most pain was down the inside of the arms, with red streaks. The pain extended to all the fingers, but in both cases the little fingers became exquisitely sensitive, with a faint blush, but they could not bear the slightest touch. There was then a uniform line, about one-sixth of an inch in breadth, sometimes paler and at other times redder than the skin, running in a spiral direction from the little finger over the back of the arm, to the inside of the elbow; and even yet they shrink on an attempt being made to touch the fingers. One has now a small tumor under the axilla, very painful till it opened. The other has much pain, and some swelling, from the clavicle to the breast, with blue veins, and compares the pain in the fingers of the same side to be like as if the nails were being pared off with a knife*.

Dr. Cape, physician to the Westmin-

* Since these observations were made, a pustular eruption appeared about the mouth, with otorrhœa purulenta of the right ear, which were followed by permanent relief; and she is now free from all pain, and convalescent, as well

ster Lying-in Hospital, has just given us a short statement of a case he saw on the day of her death:—

M. Byard, aged 24, an out-patient, had been confined of her third child 2d March, which died. Fourteen days after delivery he saw the mother for the first time, who had been ill seven days of puerperal fever, and insensible for thirty-six hours. Not much tympanitis; purulent deposits on the palmar face of the last phalanges of seven fingers and under four toes; also on the soles of each foot, and one on the right heel, with a red blush on the right ankle.

Two relapsed cases have been re-admitted. One had first the left calf painful and swollen, like phlegmasia dolens. When readmitted the left leg continued well, but the right leg was affected with pain and swelling, though not so tense, white, and shining, as the left had been. When she recovered, the left calf, and the right leg up to the calf, continued enlarged.

The other case has had four relapses, and after each relapse the right leg only has been affected, and always similarly, with pain, redness, and vesications. The pain was severe up to the groin. As her general health was good, the treatment was entirely external.

These pains may vary in situation and degree, and be such as to render the patients unable to bear the change of position. Sometimes they can only lie on the back, from the tenderness over the hips; and often they cannot bear the bed-clothes to touch the legs, when a cradle is used. Yet, at the same time that the sufferings are so great, the constitutional disturbance, in cases not fatal, may only be slight, and the effects of remedies doubtful.

This time we have not adopted what is commonly called active treatment, having previously done so without our confidence in such treatment having been increased.

The case of Tozer shews that phlebitis may even begin when the patient is under salivation, and the catamenia present—two conditions looked upon as most favourable to induce recovery from an attack of phlebitis; and in this case thirty-two leeches were applied at once, but without relief. So, in the present cases, mercury has not been used internally; but the strong mercurial ointment has been applied with a camel's hair brush along the course of the pain in both the legs and arms. In some, fomentations have been used. Half a dozen leeches or so, when applied to the groin, gave more relief, for a time, than when applied to any other part of the lower extremities. Internally, medicines were used generally, with a view to improve the constitutional state. Carbonate of iron seemed to be what the patients, particularly those of a pale countenance, attributed the most benefit to.

For the two following cases, which have occurred so recently, we are obliged to our colleague, Dr. Watson. In both inflammation of the veins was found. We were present at the examinations.

Elizabeth Johnson, aged 22, single; admitted 10th March, with much pain of the left leg and thigh, and swelling, which is shining, but does not pit on pressure. On the surface are several vessels; countenance anxious; complains of pain in the chest, but chiefly about the precordia; lips blueish. Catamenia were present three weeks ago, when she had pain in the same leg, but which did not inconvenience her sufficiently to make her lay up until the 8th, when she had a severe rigor, and profuse perspiration night and day.

On the evening of admission she was suddenly seized with faintness and cold perspirations, when she complained of great distress and weight at the precordia; this occurred on attempting to turn on her side. In less than ten minutes she expired. The arms, hands, face, and surface of the chest, were cold before the last few struggles of life, and the heart had ceased to beat before the respiration had ceased.

Examination 40 hours after death.—The femoral vein was found inflamed, and containing coagula of blood, which adhered in part to the red and thickened side of the vein, with abrasions of the lining membrane of the vein. The redness and thickening extended upwards

as the other, who was relieved after the purulent discharge from the right axilla.

Was the purulent or peccant matter in these cases discharged from the circulation externally, after such long constitutional struggles, and life preserved? as in some of the other cases ending in death, the pus was deposited internally in the lungs and other viscera.

Should we chance ever to have a case similar to these two, which have been so strikingly similar in themselves, we shall avail ourselves of this indication of nature, and introduce a seton to aid her efforts.

as far as the junction of the left iliac vein with the cava; and the appearance of the inflamed vein contrasted remarkably with the healthy aspect of the cava itself, and of the right iliac. The vein was not traced downwards beyond the upper third of the thigh; so far it was diseased, and contained dark blood more or less coagulated. The veins proceeding from the inflamed part, as far as they were examined, contained coagulated blood; but the origin of the inflammation was not traced to any particular vein or organ. The uterus was vividly red, from congestion of blood at its fundus, but otherwise it was natural. The right lung adhered to the walls of the chest in part; some slight emphysema in both lungs; no tubercles. The heart contained dark jellied blood, with some firmer white coagula in its several cavities, but was in structure healthy. Brain healthy. Some slight abrasions in the inside of the labia.

Hannah Randal, aged 20, single, came in 3rd January, in a state of emaciation and anæmia, with obstinate purging. Three weeks after admission, general swelling, pitting, and shining of the legs began, but with very little pain. Towards the last, delirium came on, with involuntary evacuations. She died the 16th March.

Examination 24 hours after death.—Lungs healthy, excepting one dark spot, looking somewhat like pulmonary apoplexy, but not solid, in which was a pulmonary artery, obstructed by a firm plug of fibrin adhering to its sides. Between the left lobe of the liver, diaphragm, and spleen, was a large sac containing about 4 ounces of pus, but capable of containing a pint. The walls of this cavity were formed by the thickened peritoneal covering of the liver, diaphragm, and spleen. The stomach was immediately beneath it, but did not communicate with it. Two small holes were found in the side of the sac, which ran into the left lobe of the liver. Both femoral veins up the thighs, the one to within two inches, and the other up to the common iliac, were inflamed, thickened, and irregular in calibre, containing more or less firm coagula adhering to the sides in many places, but chiefly over the valves. No pus was found in the coagula.

In this case possibly, the abscess, by its communicating with the substance of the liver, might there deposit pus, which, after having been taken into the general circulation, at this time of an epidemic tendency to phlebitis, might have contributed to the results found in the veins.

Lastly, man appears to be sustained, or at least nourished, by the vital current of the blood ramifying through the entire body, which blood is continually undergoing changes, and which changes become marked in the blood itself: also we find that the excretion from it, through the agency of the kidneys, is continually, during the twenty-four hours, undergoing changes in colour and smell, as well as in variety and proportions of its chemical qualities.

Again, the constitution of man is dependent on the food from which his circulation is recruited, and on the various excretions by which it is purified, all of which are continually varying. It is also subject to a variation from another and no less important a cause—the atmosphere—which envelops the entire body, penetrates the lungs, diffusing life to the system, through its influence on the blood.

The atmospheric variations, as regard temperature and weight, we can accurately estimate; but the atmosphere is continually undergoing other changes, which we cannot appreciate but by its effects on the entire system, and which the Italians, from the visibly marked effects it produces on some who may be exposed to this sort of altered atmosphere, so common about Rome and other parts of Italy in autumn, and though they cannot appreciate the chemical changes of the atmosphere then and there, yet they have not been deterred, on seeing its pernicious influence, from giving a descriptive name to that state of atmosphere—"malaria." No one doubts the atmospheric influence over bodily pain, particularly rheumatic; but what quality of atmosphere produces this increase or diminution of pain? On this subject we ought not to despair of becoming more enlightened, for the investigations of science are now actively and successfully directed to the phenomena of electricity, which probably may be found to exercise an influence over the powers of life itself, of which we have yet no idea.

May there not be at certain times a peculiar state of the atmosphere, which we may call malaria, acting through the organs of respiration on the blood

of some constitutions exposed to its influence, at the same time that these constitutions are undergoing in the circulation such changes, by assimilation and excretion, as before described. Then such may be the state of the malaria, and of the circulation at the same time, that they together, by their combination, may produce results which may act like a poison on the general circulation; the intensity of which varying as the malaria and the state of the constitution conjointly.

To this non-appreciable atmospheric state, malaria, acting upon constitutions more or less disposed to be affected injuriously by it, may we not attribute the cholera, spotted fever, influenza, and phlebitis, which have appeared in London at different times, some together, or alternating with one another, since the year 1832?

OBSERVATIONS
UPON
VENTILATION AND WARMTH;

COMPRISING STRICTURES ON DR. ARNOTT'S WORK UPON THESE SUBJECTS.

By Julius Jeffreys, Esq.

[Continued from page 56.]

It is with much pleasure I leave the moral questions I felt bound to enlarge upon in my last paper, for those which are of a scientific and practical character. If I shall have appeared to my readers to have occupied an undue space upon the former, I desire to apologize. They can scarcely have failed to perceive throughout the last paper that which was rightfully the case, and which, therefore, there was no desire to hide, namely, that personal and private motives had much influence in the dictation of it. If there be but one reader who, perceiving this, shall allow a suspicion of sinister motives to affect the attention he might otherwise be disposed to grant to them, to satisfy the doubts of that one reader I shall be happy to state the whole motive causes of these my papers. The following are facts, the exact correctness of which many friends can well testify:—

Ventilation and warmth have been objects of my attention for many years. Improved methods of providing for them in public and private buildings have long since been the results of this attention. That previous to the invention of the Respirator I was about to carry them into operation, is a fact; and it is equally certain that, on account of Dr. Arnott's having subsequently taken up the subject, I allowed my own plans, though in progress, to lie by; and that, from the time of his taking up the subject to that of the appearance of his work, I often replied to friends inquiring what I was doing in regard to these plans, that I had determined not to appear at all as a competitor with the author before the public; and even after the appearance of the author's work, I will candidly admit that, unless roused by his reflections upon myself and my labours, I might too probably have allowed the pressure of my occupations, and a desire to avoid discussion, to predominate over that which is unquestionably a public duty, namely, to oppose with whatever of argument or experience one may possess, doctrines and plans the prevalence of which one believes would, *in the end*, prove highly injurious to the health of one's countrymen. But while I admit that the motive force has been the author's statements regarding myself and my labours, I will yield to no one in a heartfelt desire to perform my duty as a member of the community; and so far from secular motives predominating, I may, I trust, safely affirm that the inducement is not to be found which would persuade me to give countenance to the author's arguments and plans, holding, as I do, opinions directly opposed to them, and which are not the opinions of a novice.

Whatever other objects there may have been in securing the proprietorship of my plans connected with warmth and ventilation, and the improvement of evaporating apparatus, one motive, at least, has been to offer a consistent opposition to the doctrines regarding patents generally, and patents held by medical men in particular, set forth by the author. I affirm, therefore, that, apart from any pecuniary motive (whatever may be its suspected amount), I have claimed the patent right of my plans upon principle.

That even one individual reader should, on this account, allow the notion of private motives to affect the

attention he would otherwise favour me with, would be a source of regret to me, not on my own account, who am confident that the cause of truth, as in morals so in philosophy, will ultimately prevail, on whichever side it may appear to lie, but because it would pain me that my own affairs should be even a passive cause of inconvenience to him, if, prejudiced on their account, he should, upon subjects of great importance to every man, refuse attention to arguments and plans which, if true, must partake of the value of these subjects.

In conclusion, I have to express a regret that it should have been necessary for me to take up my reader's time with the personal matter of my last paper; but I would beg to remind him that, in explanations of that nature, the time of the public and of the legislature is oftentimes largely occupied—in cases, too, where the offence is by no means so well grounded; and that, so far from refusing attention to such an explanation, he might well look down upon the individual who, in these islands, would submit in silence to a despotism, either in philosophy or in politics, or who should be backward, either from apathy or timidity, in defending the interests of those concerned in his labours.

The subject of my last paper, ventilating and warming by the aid of recovered animal heat, is the subject also of the present; and a digression which was then called for, regarding the author's thermometer stove, it is needful to renew. In favour of that instrument the utmost was said that I felt able with sincerity. But while, with much pleasure, I admit the author to have selected the best of the cubical hot-air stoves, and while the experience of every good chemist must confirm the advantage of attending, as he has done, to the casing of a fire with brick, and the commanding of the draught (points which no one in any measure qualified to be an improver of stoves could neglect), and while I would also desire to do the fullest justice to his thermometer regulator, I feel it right to acquit myself before my reader of advocating the use of hot-air stoves of any kind, and least of all of such as are cubical; and this, not only because they fail to *enforce* a free ventilation of an apartment, leaving this duty to the deceptive feelings of the inmates, who are almost sure to accustom themselves by degrees to do without the window drawn down, or door set a-jar, the feelings, where the air introduced is colder than that of the room, always tending to a detrimental exclusion of it further and still further; it is not for this reason only, though alone an abundant one, that I consider these stoves very defective: there are several other reasons which it would be out of place to discuss at length elsewhere than under their proper head of ventilating and warming by the aid of combustion. It is true, that of the three kinds of apparatus which (contrary, as my friends well know, to my settled purpose) the author's writings have drawn me on to lay before the public, one will prove to be a hot-air stove, while the two others are a hot-water stove, and an open fire, with self-acting regulators of two kinds for the former. But this hot-air stove will only be recommended for places where there is no chimney at hand. Though I believe it to be less oppressed with defects than other air-stoves, being an air-stove I cannot recommend it generally. In saying this, I have to sacrifice the feelings of an inventor; for its power of discharging its draught into a drain where there is no chimney, and its compactness, in comparison with the duty it can perform, are properties which render it attractive, and are the grounds upon which it will be recommended. Whatever experience I have had is so decidedly in favour of water as a medium for the transfer of heat, and is so well confirmed by the authority of the most experienced, that I feel surprised that the author should not have adopted it; nay, that he should have left it, after having made trial of it, even in the imperfect form of the hot-water stove he describes in p. 41 of his work; and for reasons which I cannot but think very insufficient. By a little perseverance in its use, he could hardly have failed of falling upon a method of employing it, more efficient than the "water-clad stove" which he had to lay aside, or than that described at the close of the work.

To return to my subject. Many years ago, desirous of recovering the heat from vapours issuing copiously, and at a high temperature, from large evaporating vessels in a salt-petre refinery, in the East Indies, a process

presented itself to me by which this promised to be well effected, and which I was disposed to name the heat-transferring counter-current process; but in that dry and hot climate, blowing fresh air over the surface of the liquors by a large ventilating pendulum-pump, was substituted as less expensive.

I was not, at the time, aware that the above principle had been attended to by any other person; but I subsequently found, that in some distilleries, and more recently in steam engines, it had been employed; and the author appears, many years ago, to have described it. Though I did not erect the apparatus in India, I brought with me the plan from that country, in the year 1835; and, as my friends are aware, was about to employ it in a manufactory in which it promised to be especially advantageous. Negotiations were entered into for the works in a midland county, but not completed, on account of the large size of the property. At that very time, the distress which the inhaling of cold air occasioned to an invalid with whom I was walking, led to the idea of the respirator. With the mind busied upon the above heat-transferring process by counter currents (the double current process of the author), I committed the error of allowing myself to turn to it, as the principle upon which to construct a respirator; there was, therefore, presented to my thoughts an instrument, precisely similar in principle to that which the author, owing to the same oversight, proposes as an improvement on the respirator. Maturer reflection rendered manifest what I ought to have perceived at once, that that was not the principle upon which any efficient respirator could be constructed. The proper principle, and any instrument acting upon it, having never been developed, some excuse might perhaps be found for my mistaken view at first: but with the respirator already constructed before him upon the proper principle, it is a matter for surprise that the author should have turned aside from it, not for a short time only, but to the length of a detailed recommendation in point, of so faulty an instrument as that described in the following passage:—Speaking of the material of the respirator, which the author calls wire gauze, he says (p. 71), "We may remark, with respect to the wire gauze, that the temperature of the air inspired through it is very unequal, being much higher at the commencement of the respiration than towards the end, where the metal has already given up nearly all its heat. But a perfect uniformity of temperature is obtainable by using a suitable modification of the double current apparatus, of which I have spoken above. For this purpose is required a mouth-piece, from which one or more tubes of thin metal, or other substance, may carry the hot breath to the atmosphere, and to which another larger tube, surrounding the first, may bring pure air from the atmosphere, the currents being kept quite distinct by simple valves in the tubes. The warmth of the departing air will thus be given to the coming air, and the degree of warmth retained will depend on the length of the tubes, or on the action of a valve placed over a lateral opening in the outer tube, near the mouth-piece, and made obedient to a thermometer*. Thus, also, will uniformity of temperature be secured. The apparatus may be made to resemble a cane held to the mouth, or a Turk's pipe, or may bend round the neck, and be concealed in a cravat."

The reader will probably perceive at once, the mistake the author has committed in supposing that his mouth-piece would act upon this double current principle. The very existence of this principle requires that the two currents should move simultaneously. Its essence consists in having a colder fluid moving contiguous to a warmer one in an opposite direction, and at the same time, so that in every point throughout their course heat may be continually darting across, from the latter to the former, through the thin intervening metal. It is only so long as there is an uninterrupted current travelling in, while the warm current is going out, that one end of the metallic tubes can remain as much warmer than the other as the principle requires. If the heat-conducting influence of the colder fluid be suspended for a moment, heat will rush, not across the metal as before, but along its substance lengthways, to establish an equilibrium of temperature between its two ends; and if the conducting influence be suspended a longer time, there being no current coming in during all the time the warmer current is

* A thermometer!

going out, the rise in temperature of what ought to be the cold end of the tube, will disqualify it for taking heat from the outgoing air, long before that air has parted with all the warmth that might otherwise be recovered from it. Furthermore, if the outgoing heat-supplying current ceased, before the entry of the incoming current commenced, the latter, as to the supply of warmth, would be in an unfortunate position. Little heat could it get from the metal, which had little to give, and there would be no current on the other side of the metal, rich in warmth, travelling out at the same time, according to the conditions of the principle, to meet its necessity with the expected supplies of heat. Now this extreme case is that exactly of the author's attempt to make a respiring instrument act upon the double-current principle. He has failed to reflect, that, before this could be done, it would be needful to alter the structure of our respiring organs themselves. In order that the two operations of breathing in and breathing out should form a double-current process, they would have to be simultaneous; not distinct and alternate, as they really are. Our organs act upon an alternate current, and not, of course, upon a double current prinple: our breathing out is all over, before our breathing in commences. For us, therefore, a double current instrument would be totally out of plaee. The only way in which it would be possible to make an instrument of this kind act at all, would be to desert the double-current principle, and to give its tubes such capacity as to contain at least one whole outgoing and one whole incoming breath at onee, if not two of each; that is, to have a bulk of from 50 to 100 cubie inches, besides that of the materials. It would have to be from four to eight feet long, and nearly two inehes thick, and would then act very imperfeetly, since the new principle on which such an instrument would act, would allow it, at most, only one-half of the heat, and in practiee it would not realize one-fourth.

If we had two wind-pipes—one, for instance, from the right lung, going to the nose, and the other from the left lung to the mouth, and had the power of expanding the right side of the chest while we contracted the left, and *vice versâ*, then we might draw air in by the nose during all the time we were breathing out by the mouth, and in by the mouth all the time we were breathing out by the nose. If such were the conformation of our organs, then, and then only, would the author's mouth-piece answer at all; for then, having double wind-pipes and distinct lungs, we might have a double-current respiration, and a "double-current air-warming mouth-pieee" would henee suit us. But eonstituted as mankind is, with an alternate current respiration, the only instrument suited for them must be one which, like the Respirator, is eonstructed upon the alternate eurrent principle.

Since the double-current instrument of the author is upon principle manifestly inapplicable to the purpose intended, it is scarcely necessary to detain the reader by pointing out the great inconvenience of any instrument of that kind, even if of one-fourth part of the size that would have any effect. In order to get over this difficulty, and place it out of sight, the author has to make it flexible; and for this end he actually hints that it might be made of "other substance," and not metal! What non-metallic substance is there, he may be asked, which would act at all? It is to be regretted that the author, in his attempt to support what he imagines an improved respirator, should thus hastily make a statement of which the tendency must be to mislead the public. He concludes this proposal by saying—"Bceause twice as much heat issues with the breath as is wanted again, a person will not lose the advantage of the apparatus by detaching his mouth from it occasionally, to take his part in an ordinary conversation."—Page 72.

The meaning of this passage it would not be easy for any one to explain. In practice it is found very difficult, even with an instrument of the most suitable kind, to catch as much of the heat of the breath as is in many eases desirable; but snpposing all could be eaught by his instrument, instead of less than a fourth part, however bulkily made, how could this benefit the breathing while the instrument was removed? During the shortest act of conversation, several inspirations of eold air would take place, which, on account of the sudden removal, would be especially trying, granting for a moment that the instrument had a proper warming power; and this is the instrument offered to the public as an improvement on the res-

pirator, to obviate a trifling and unavoidable irregularity of temperature! The straining at a gnat, and swallowing a camel, is surely realized by the person who objects to one instrument on account of an inequality so trifling now as never to be complained of; and then proposes another instrument which, every time it was removed for conversation, would throw suddenly the full force of the cold air upon the lungs of the invalid.

By the following passage the author would appear to consider the respirator to be a simple modification of muffles tied over the face; so small a one that he would retain for it their common-place name "fear nothings" (Art. 84, p. 69.) "This process for transferring heat from impure air about to be dismissed into the atmosphere, to pure air about to be applied to use, will by many readers be deemed quite new; and yet, in a less perfect form, it has been a popular practice in Europe from time immemorial. When a person going out of his house into the cold air of winter, ties a bulky woollen handkerchief, called "fear-nothing," around his neck and face, or holds any such porous mass over the mouth, that he may breathe through it, he is really applying the same principle. His warm breath going out through the handkerchief or other mass, warms it, and then the cold pure air drawn towards the mouth through the heated mass, absorbs a great part of the retained heat, and enters the chest of the individual much less cold than the air of the atmosphere around. Lately, a useful modification of this simple process has been presented to public notice by a medical practitioner returned from India. He substitutes for the bulky handkerchief, or texture of common thread, a less bulky texture, yet more heat-absorbing, of metallic thread or wire; in fact, folds of fine wire gauze or pierced plates of metal, which when fixed in a light frame, form a compact mouth-piece, fitting closely round the lips, and leaving free passage for the air between the numerous apertures. The folds or plates are heated by the warm breath going out, and then give up the heat so acquired to the pure air going in. The contrivance may be called the fear-nothing of metal; and it has the advantages of being less bulky, of retaining less of the impure air in it, and of allowing speech to be heard through it, almost as if there were no interruption." Were it true that the "bulky woollen cloth" acted in the manner stated by the author, namely, by conducting off heat from the impure breath going out, and giving this heat to the fresh air entering in, it is manifest that their action ought to bear a proportion to the conducting power of the materials of which they are made. Those fibrous coverings the fibres of which are known to have the quickest conducting power, ought to be those which acted best, while such as have a very feeble power of abstracting heat, ought to be the feeblest in power. So far is this from being the case, in regard to the several kinds of clothy textures used before the mouth, that we find the woollen articles called "comforters," or "fear nothings," although composed of wool, the very worst conductor of heat, to have the most warming power. Next to these are cotton handkerchiefs, next silk, and last of all linen. So feeble is the action of linen, that if it be made of a texture open enough to be readily breathed through, it will have no warming power which is appreciable. But in this series the fibre of linen has the best heat-transferring power, and in the series upwards from silk to cotton, and from cotton to wool, we have a decreasing power; the fibre of wool being almost the feeblest conductor of heat of any substance in nature. Of all these mouth-coverings, we find that—namely, the woollen—to act best, in the way of saving warmth, which is the worst as a tranferrer of heat; and that — namely, linen — to act worst, which is the best transferrer of heat of any such substances. It is therefore manifest that these articles do not at all act upon the heat-conducting principle; and in endeavouring to make out the respirator to be a simple modification of them, the author has mistaken their action. A little reflection will suffice to explain the principle upon which these wrappers do really act. In the first place, as to a bulky woollen handkerchief: woollen fibre is so feeble a heat-giver and heat-receiver, that a piece of wool may be burnt to a cinder close to the fingers, which may be holding another part not perceptibly warmed. It is not possible, then, that the fibre itself of the woollen cloth should receive any heat worth naming from the outgoing breath, or give any to the incoming

breath. The warmth which is felt, whatever it amounts to, is not derived from heat conducted off from the impure outgoing air by *the woollen fibre itself;* it is not from heat thus stored up *in the fibre itself*, that the incoming air can receive any heat, but from a volume of hot impure air lodged among the fibres of the wool, throughout the interstices of the cloth, which remains there as a portion of the old breath last discharged, and which enters again with the new air. Hence, to produce any considerable warming sensation, these cloths must be very bulky. The impure breath spreads about in them in all directions, as the person is breathing out; much of it remains lodged in the cloth, and, mixing with the fresh air drawn in, it warms it in a small degree. When a linen cloth is employed, little or no effect is produced. The reason is, that though a better conductor of heat than woollen fibre, the conducting power of *linen* even is far too feeble to give it any efficacy on the principle of conduction, while its closer texture and greater density than that of the woollen cloth, leave much smaller interstices for the lodgment of any of the hot breath, so that there is little to mix itself with the air drawn in.

It has then, I think, been made very obvious, that in the case of all the fibrous coverings of the mouth, their action does not depend upon the principle the author would apply to them; that the share, indeed, which this principle has in their action, really amounts to nothing. The linen, which, if that were the operating principle, would unquestionably prove the warmest substance to breathe through, proving in practice the very opposite; while the woollen, which according to that principle ought to have the least effect, proving to have the most. The truth is, that the heat-conducting power of any of these fibrous substances is so feeble, and their capacity for heat so small, that they could do nothing effectively upon the principle of receiving and giving out heat. It is so obvious that the warming power possessed by any of these cloths depends upon their porosity and bulk alone, that it appears surprising the author should have sought for any other than their manifest quality of porosity and bulk, which give lodgment to some of the hot air of the breath.

Had the principle of these things and the respirator been one and the same—had the invention of the latter required no new principle to be developed, but merely a change of materials, the necessity for which would have been obvious with the principle in our sight—we could not have remained up to this time without an instrument, needed as much centuries ago as at present, since its action is to give ease to a class of sufferers so numerous, that vast sums are annually collected by empirics through the sale to them of deleterious drugs, known to aggravate in the end, though for the time suspending their symptoms. So far from pointing to the invention of the respirator, the bulky woollen cloth, or "fear-nothing," was the champion and representative of antagonist articles, and kept our minds among simple sheep in the folds, and at the shearing-house; whereas they ought to have been penetrating below the pastural surface, to find in the earth's bowels the quickest conducting of minerals. The truth is, that for the clothing of our persons and of our beds, our minds have been so familiar with the use of what the vulgar call "warm" articles, and the chemist "slow conductors" of heat, that the chemist even has not, any more than the former, thought of putting so "cold" or "quick conducting" a thing as metal near the body to keep it warm; and still less over the mouth, where (all in a like manner with the same notion of "warm things") experience was shewing the worst of conductors to be here also the best of coverings, until the development of the very opposite principle of the respirator led to the use of metal.

The subject of the employment of recovered animal heat in ventilating and warming is not yet concluded; but as there appears to be an expectation, on the part of some of the readers of these papers, that the third division—ventilating and warming by the aid of combustion—should be early entered on, the conclusion of the present division will be postponed for the consideration of the former*.

* Three weeks will elapse before leisure will be afforded me for drawing up the next paper, which will therefore be published on the 12th of May.

AN ACCOUNT OF SOME RECENT EFFECTS OF MESMERISM*.

To the Editor of the Medical Gazette.

SIR,

I HAVE lately availed myself of three or four opportunities of witnessing the singular "mesmeric" effects which Dr. Elliotson has obtained upon some patients in the North London Hospital. What I witnessed on these occasions was so strange, that I have written it out to enable myself to comprehend the facts by placing them in a philosophical order; and as the narrative, so drawn out, of these observations (which numerous spectators present on the same and other occasions have had similar means of making with myself) may interest your readers generally, with Dr. Elliotson's permission I send it to you.

I must premise, that many of the effects which I have to mention are such as I should have given no credit to, unless I had seen them repeatedly produced (so that they were evidently reproduceable at pleasure), not by one or two only, and by the initiated, but by several, including myself, indifferent spectators, and that under such a variety of circumstances as entirely to do away with the possibility of deception. Different persons arrive at conviction upon points of questionable probability in different ways: some are most influenced by authority; with myself, I will frankly admit, in the present instance, authority went for nothing. The facts which had been narrated to me appeared perfectly incredible till I saw that they were certain.

The observations which I shall communicate touch the following subjects:—the conditions of the body and of the mind during mesmeric sleep;—the natural states to which mesmeric sleep is allied;—the mode of exciting the mesmeric phenomena, and the nature of the principle involved in their production;—the conditions which render the body susceptible of the mesmeric influence, the further application of the same, and its capability of use and abuse.

I. In an essay entitled "Powers of the Roots of the Nerves in Health and in Disease; and on Magnetic Sleep," I explained generally the points of difference which I had ascertained to exist between mesmeric sleep and true sleep; and recommended the use of the term *trance*, to denote the former peculiar condition of the system. The *mesmeric trance* may exist in two degrees: one analogous to sleep, the other analogous to somnambulism. Dr. Elliotson, on the occasions to which I advert, exhibited three patients, girls, of ages between thirteen and seventeen, who displayed in succession these two states, alternating with each other, or with the rational waking state, at the pleasure of the operator. These two states may be called *mesmeric coma*, and *mesmeric sleep-walking*.

Mesmeric coma is a state of profound insensibility, in which the pulse is slightly quickened, the breathing gentle and natural, the pupil as ordinarily, but turned up or rolling away, when the eyelids are opened, contracting on exposure to light. The insensibility is so profound as not to be disturbed by a strong electric shock, or by the sustained action of a magneto-galvanic battery, each so intense as to be intolerable to most persons. It should be mentioned that the muscles were convulsed under the influence of the galvanic discharge, while it appeared to be unfelt by the patient. The lesser stimuli of pinching or pricking the skin, or pulling the hair, produce no effect whatever. It is not to be doubted that any surgical operation might be performed upon a person in this state without her feeling it.

In *mesmeric sleep-walking*, the patient is to a certain degree awake to external objects, and is able to support herself, and to walk about, but with hesitation and insecurity: she sees, but the eyes remain fixed for distant vision, so that objects close at hand appear double: she hears, but has no perception of the direction of sound, so that she does not turn her head to one addressing her standing by her side; if the voice is a familiar one, she recognizes it, and is anxious to know where the person is, and looks in search of the person, but looks straight before her only. The insensibility to touch is as great as in the mesmeric coma: neither pinching the skin, nor pulling the hair, nor touching the sur-

* We have ventured to substitute this for Mr. Mayo's paper on Slow Pulse, previously received, as of more *immediate* interest. We shall make room for the other as soon as possible.—ED. GAZ.

face of the eye, nor the electric or galvanic shock, provoke sensation any more than in the former state.

Whether the muscular sense, and taste, and smell, are suspended, is not yet ascertained. Dr. Elliotson inclines to think that the former is preserved; Mr. Wheatstone is of opinion that, together with taste and smell, the muscular sense is temporarily extinguished. Citric acid, and soda, which Mr. Wheatstone placed on the tongue successively, seemed to excite no sensation. Snuff does not produce sneezing. But one of these little patients, having two different weights placed in her hands, lifted more readily, and said that it was easier to lift the lighter than the heavier.

The extent to which reflection and volition are awake in this state, appears to differ in different individuals. Two of the three young patients talk fluently, with great readiness of apprehension and reply, and some humour, in the state of somnambulism; the other, who is younger, and when awake of a more quiet and gentle disposition, hardly answers more than yes or no. The difference probably has to do with two causes: one natural character, the other the passing or permanent tendency to ordinary somnambulism and fits of delirium.

One of the three patients shewn by Dr. Elliotson, after being thrown into the mesmeric coma, spontaneously and suddenly awakes into the state of somnambulism, with an appearance of pleasure and delight at her returned consciousness, and instantly begins addressing observations to persons or objects around her. One of the two others requires to be artificially, and by a peculiar manipulation, aroused from the state of coma into that of somnambulism. The period in which one or both of the others would remain in the state of coma seems to be indefinite. Dr. Elliotson mentioned that one had been kept in this state twenty hours.

II. With the state denoted by the term "mesmeric coma" I know no spontaneous disorder of the system that has been proved to be identical; but it is highly probable that in some other trances, and in some hysteric fits, insensibility as profound exists.

The state of mesmeric somnambulism is most likely the same with one, perhaps with two, sorts of mental disturbance that are well known to medical observers. The first is the state of mind that alternates with the natural state in double consciousness.

As the reader may not be familiar with this fitful alienation of the mind, I will introduce the details of a case communicated to me by Dr. G. Barlow, that will bring to the recollection of the spectators of Dr. Elliotson's cases one of the latter that resembles it in many features.

"This young lady has two distinct states of existence. During the time that the fit is on her, which lasts sometimes for a few hours, sometimes for three days, she is occasionally wild, merry, and mischievous; occasionally appears in pain, and rolls about in uneasiness; but in general seems so much herself that a stranger coming into the room would not remark any thing extraordinary. She amuses herself with reading or working; plays on the piano *better than at other times;* knows every body, remembers every thing, and converses rationally, and makes very acute observations upon what she has seen, heard, or read. The fit leaves her suddenly, and she then forgets every thing that has passed during it, and imagines that she has been asleep, and sometimes that she has dreamed of any circumstance that has made a vivid impression; for example, during the period of these fits she had been reading some of Miss Edgeworth's Tales, and had in the morning been reading one to her mother; she went for a few minutes to the window, and suddenly exclaimed, "mamma, I am quite well, my headache is quite gone," returned to the table, and taking up the open volume which she had been reading five minutes before, said, "what book is this?" turned over the leaves, looked at the frontispiece, and replaced it upon the table. Seven or eight hours after, when the fit returned, she asked for the book, went on at the very paragraph where she had left off, and remembered every circumstance of the narrative: and so it is always, as she reads one set of books during one state, and another during the other. She would seem to be fully conscious of her state, for she said one day, "mamma, this is a novel, but I may safely read it; it will not hurt my morals, for when I am well I shall not remember a word of it."

The other state to which mesmeric somnambulism bears an affinity is ordinary somnambulism.

III. The conditions ordinarily used to throw a waking person susceptible of this influence into the mesmeric coma, are, either to hold the hand steadily extended towards the head (the forehead or back of the head indifferently), or to move the hand pointed towards the patient slowly downwards, repeating this in a series of similar movements: the hand is held at the distance of a few inches from the patient; the nearer, without touching or exciting sensation, seemingly the better: the distance at which the effect can be produced, and how much gesture is absolutely necessary in addition to a certain degree of mere proximity, and how much mental attention, are points which have not been ascertained. It did not appear to me that it made any difference in the time required to throw a waking patient into the mesmeric coma, whether the hand was presented to the forehead or to the back of the head; whether the operator was seen by her, or acting unseen by, and acting unknown to her.

When the state of coma supervenes, the iris contracts, as it does when one drops asleep; but the contraction is transient, and goes away suddenly. It may be seen coming on for three or four seconds, while the coma is supervening; the moment that is complete, the pupil becomes natural again, and maintains the same degree of contraction or dilatation as under the same light in the waking state.

From the state of coma, it has been mentioned that one, if not two, of Dr. Elliotson's patients, spontaneously, in about a minute, wakes into the mesmeric somnambulism. In the others, or in one of them certainly, this change does not take place spontaneously. To produce it a manipulation is used, which is equally effective with all three. If with both thumbs the eyebrows are once or twice stongly rubbed from the nose outwards, the little patient opens her eyes, and is at once in the state of somnambulism above described, and can hear and speak. A similar manipulation used on the forehead, or the cheek, or the chin, is totally useless, and the patient is left in the same state of coma as before.

The same manipulation, continued longer, restores the patient to perfect waking and the natural condition of the mind and body.

When she is in the state of somnambulism, the least motion directed towards the patient by any spectator, not only when near, but when several feet off, instantly produces the state of coma. *This* effect takes place with more certainty, if the motion is in front of the patient. Not only on moving the hand downwards, but on bowing, or on raising the foot before the patient, with a direction towards her, she drops into total unconsciousness.

The coma is produced with equal certainty if a thick screen of board is interposed between the operator and the patient, as if he is standing immediately before and seen by her; but the effect does not then ensue as soon as otherwise.

One mode of inducing the state of coma upon somnambulism is to blow upon the patient's eyelids. What makes this effect the more singular, is, that when the patient has thus been rendered comatose, blowing upon the eyelids awakens her into somnambulism; so the patient's sleep-walking senses may be blown out, and afterwards blown in again.

Of the instances already given of the production of mesmeric phenomena, some are quite incompatible with the notion that they are excited by impressions made upon the mind, through the common avenues of sensation. The following, on the one hand, help to put that alternative supposition quite aside, and on the other, determine some of the conditions necessary for the operation of the mesmeric influence.

Yet, in preparing to narrate them gravely, one is half disposed to smile sympathetically with the smile of incredulity which the narrative must excite. One imagines the reader to say, that what I have already described was known to, and better stated in the words of Aldiborontiphoscophornio,

> "And yet, 'tis not definitively sleep,
> Rather a kind of doze,—a waking slumber,
> That sheds a stupefaction o'er the senses."

But what will we quote in good-humoured ridicule of the following incredible facts?—

When Dr. Elliotson's little patients are in the state of mesmeric somnambulism, if you place your hand near their hands or feet, and move your fingers, the fingers of the hands, or feet of the patients, likewise begin to move. If, then, or after having simply held your hand near the hand or foot of one of the somnambulists, you draw your hand slowly away, the hand or foot of the patient follows your hand. After the

hand or foot has followed your hand a short distance, the patient drops from the state of somnambulism into the state of coma. That is to say, the effect thus produced is two-fold. The influence of your hand is first to drag the hand or foot of the somnambulist after it; secondly (like every other movement directed at the somnambulist), to determine coma. And these results ensue equally, whether you are before the patient and seen by her, or at her side; or out of the sphere of vision, or behind her, a thick wooden screen being interposed.

The most striking instances are these:—Mr. Wood, of University College, acting under Dr. Elliotson's directions, stands behind the patient, and then moves, as you suggest to him, one hand in a direction from her towards the right or left, pulling her, as it seems, by immaterial links; her hand is now raised from her side in the same direction; then her person inclines to the same side; then she drops unconscious. Or Mr. Wood raises both his hands, standing behind her, and drawing her by the same immaterial force upwards, which she obeys; and half raising her arms, and raising herself upon her toes, she falls backwards in the state of coma. The same effects I have seen, when closed folding doors, or a thick screen of wood, were interposed.

The little person being wakened by rubbing her eyebrows *more solito*, is told to take hold of any object (a tumbler of water, for instance, on the table before her); she accordingly closes her hands upon it: some one, at the same time standing behind her, points his hands towards her's, at the distance of four or five inches, and then moves his hands away divellently. Her hands follow his slowly, and against her will, she plaintively wondering why she cannot take or keep hold of the object before her. Her arms are at this time in a state of strong muscular spasm. Every muscle seems in its strongest action. It requires nearly the utmost force you can exert, not only to prevent her hand following the operator's, but to make it move faster than it is now convulsively moving in that direction. In a few seconds she drops comatose.

In the production of the latter phenomena, the influence exerted acts exclusively locally.

Three of those present tried a modification of the last experiment upon one of the little somnambulists: one addressed himself to one hand, another to the other; a third endeavoured to produce a general upward motion of her head and body. The three independent effects took place simultaneously and distinctly, in the short interval before she fell into coma.

Again, the influence depends upon the proximity and motion of the hand of the operator, and cannot be effected by mere motion and intention.

An artificial hand of paper was tied to the end of a stick, and the person who held it, not changing his place, moved the stick; when the hand of paper was moved away once, and again, and repeatedly, no effect followed; the hand of the somnambulist, who was at this time engaged in conversing with Professors Sylvester and Wheatstone, did not move. But on the hand of the operator being substituted for the fictitious hand and stick, the hand of the somnambulist immediately rose, and she fell into coma.

The effect produced depends upon the quantity of surface used to mesmerise.

A pasteboard screen being interposed between Dr. Elliotson and the little somnambulist, when he advanced his hand, with one finger lifted, towards her, she became mute and dull, but did not fall; when he repeated the experiment with a dose of three fingers, she fell at once. This must necessarily appear ridiculous, unless you saw it, and saw it repeatedly succeed, as I have.

There is, however, some seeming caprice in all these results; sometimes it happens that, no sensible cause having interfered, for many minutes no mesmeric phenomena can be produced; sometimes the cause may be traced. When, for instance, the little somnambulists are much interested in any thing going on before them, the efforts of the magnetizer are temporarily baffled.

The mesmeric influence shewn in the production of coma may be conveyed from one person in the state of somnambulism to another.

The three little patients, in the state of mesmeric waking, were seated in three chairs, holding each others' hands. A sheet of pasteboard was then interposed between the first and second. The first was then thrown into the state of

coma; the other two immediatcly fell into the same. On trying then to wake the first to the state of somnambulism, the ordinary means failed; the influence of the coma of the two others beat the means employed to wake the first. On separating her hand from that of the second, she was awakened at once by the common means.

The mesmeric influence will not travel through a person not in the state of somnambulism, from one mesmeric somnambulist to another.

Dr. Elliotson sat down in the place of the middle one of the three engaged in the last experiment, holding a hand of each of the others. On throwing one of the latter into coma, the other was unaffected, and remained, as before, in the state of somnambulism.

The mesmeric influence, shewn in the production of muscular movements, may be conveyed from one person in the state of somnambulism to another.

The three little patients being disposed as in the last instance but one, and a pasteboard screen being interposed between the first and second, on exciting motions in the unengaged hand of the first by the proximity of the hand of the operator, similar motions took place in the unengaged hand of the third, and some motion in the clasped hands of the second and third.

IV. It seems reasonable to presume that the subjects of this influence are few in number. Those of delicate nerves, or whose nervous system is already shaken by disease, are probably its most easy, if not its only subjects. Women scem more sensitive of it than men. But it is yet to be ascertained how many classes of persons are susceptible of its agency, and what additional effects may be produced upon such, beyond those which have been already verified. I have seen nothing produced beyond the kind of fit, and the kind of sleep-walking, with the attendant phenomena, which I have described; and although prepared to expect that more phenomena of a like sort may possibly be obtained, I disbelieve as wild extravagance the pretended mesmeric *clairvoyance*.

But one is sure to be finally met by the question—*Cui bono* the limited mesmerism which you admit? Some one on a similar occasion made the same inquiry of Franklin, who replied to the question of the minute philosopher, by asking another—what is the good of a new-born infant? As it happens, our infant at its present birth has shewn practical prowess and utility; it has vanquished in its cradle an epilepsy and an hysteric palsy. One feels, however, mistrustful rather than sanguine as to its general remedial efficacy, not doubting that there is good it may be turned to, and certain that, in Dr. Elliotson's hands, it will be turned to the best purposes, but more apprehensive of the plentiful misuse that may be made of it by others —I am, sir,

Your obedient servant,

HERBERT MAYO.

19, George Street, Hanover Square,
April 18, 1838.

MEDICAL GAZETTE.

Saturday, April 21, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE NEW FRENCH PHARMACOPŒIA.

THE publication of a new edition of the French Pharmacopœia last year was passed over in silence, if we are not mistaken, by all the medical journals printed in these islands. This remarkable omission tends to show the prevalent turn of the medical public just at present for speculation rather than practice. It is difficult to conceive that if any ingenious Frenchman had put forth a new arrangement of the Campanulaceæ or the Urticaceæ, every scientific Briton would have forgotten to review and analyse it; and, assuredly, if any Parisian physiologist had discovered an additional ventricle in the brain of the dog, it would have given rise to much eager discussion on this side of the water, and perhaps to some angry claims. Yet, besides the great importance of the Codex to the thirty thousand physicians

of France, whose prescriptions must, more or less, be framed in accordance with its formulæ, the practitioners of other nations cannot but derive some benefit from the study of the pharmaceutical refinements of their French brethren.

We may remark, in starting, that although the French, in popular opinion, are believed to be, above all other people, *novarum rerum sectatores*, they falsify this the current creed, in the matter of *codices*, or Pharmacopœiæ; for while we sober English have had four new Pharmacopœiæ since the one of 1745, namely, those of 1787, 1809*, 1824, and 1836, our neighbours have had but two since the edition of 1748, namely, those of 1818 and 1837.

The first novelty of the Codex, and one which unquestionably will overwhelm *l'Europe savante* with amazement, is the language in which it is written, which is no longer Latin, but French. Whether this innovation is an improvement, would be difficult to decide; as Sir Roger de Coverley used to observe, much may be said on both sides of the question.

It might be urged in favour of the old plan of writing Pharmacopœiæ in Latin, that this language has been for ages the chosen dialect of learning, and that it is consequently desirable to make the knowledge of it familiar to all the members of a profession, who would otherwise be unable to read, except through the dim medium of translations, the treasured observations of their predecessors; that this language being still familiarly used in a great part of Europe, forms a common mode of communication between the physicians of Moscow and Madrid, of Dublin and Vienna; and that as long as the upper classes make some proficiency in Latin the test of a gentlemanly education, it would be dangerous for the leading men of any profession to do any thing which, by diminishing the literary proficiency of the bulk of their colleagues, should have a tendency to lower their position in society.

On the other hand, we must allow the force of the arguments in favour of the work being in French, which the authors have brought forward in their preface. They allege that the Codex is specially intended for France, where it is to serve as a practical guide, and that the most essential point is, that it should be perfectly intelligible, so as to leave no doubt in the minds of those who are to consult it. Moreover, if the commission had drawn it up in Latin, they had no means of preventing the immediate publication of a French translation, however incorrect.

On the whole, we are compelled to allow that the innovation is an improvement, and that, in a practical point of view, it is better that every Pharmacopœia should be in the language of the country where it is published.

The authors add, however, that by the side of the French name of a simple or compound medicine, they have placed the Latin term; so that the druggist may easily make up a prescription written in Latin, and foreigners may consult the Codex without feeling the least doubt as to the drugs intended.

This double nomenclature will fulfil the latter of the intended purposes, but, for want of a Latin index, will not answer the former one. Thus if a prescription were sent to a French druggist, beginning with ℞ *Sclerotii clavi drachmam*, he could not find out, by any method short of reading the whole Codex through, that the drug intended was the *Seigle ergoté récent*. But the foreigner who reads the Codex, and comes to the terms *Seigle ergoté récent*, or *Agaric blanc*, finds, by the interpre-

* The edition of 1815 might be added to the list, being an improved reprint of that of 1809; but the French probably had reprints of the same kind.

tation, that *Sclerotium clavus* and *Boletus laricis* are intended; or should he know the French term, and wish to find it in the work, the French index is at his service.

Another point in which the commission has thought it expedient to use a double nomenclature, or rather a double notation, is weights and measures. The decimal system, however, recommended by theory, has not made much progress in practice; people still talk of *sous*, rather than *centimes*, and reckon by pounds and ounces instead of *grammes*; and, therefore, to accommodate the work to old-fashioned readers, or compounders, the qualities are commonly, though not always, given in both ways*.

Some useful tables are given of the weight of various fluids, and of the weight of water contained in ordinary spoons or glasses. Thus, twenty drops of sulphuric æther at 66° of the *pèse liquide*, weigh 7 grains, or 0·35 of a gramme, while the same quantity of Hoffmann's anodyne solution (Spir. Æther. Sulph. Comp.) weighs 9 grains, or 0·45 of a gramme.

A coffee-spoonful of common water weighs 90 grains, or 5 grammes; a pinch of chamomile flowers 2 drachms, or 4 [8] grammes; of arnica flowers 120 grains, or 6·2 grammes; a new-laid egg, on an average, 2 ounces and 2 drachms, or 72 grammes; and its yolk alone 5 drachms and 15 grains, or 21 grammes.

Another point worthy of notice is, the comprehensiveness of the Codex; the number of drugs and their preparations amounts to 2021, of which 687 (distinguished by a star) are to be kept by every druggist. The number of preparations is 1561; the number of those in the London Pharmacopœia is only 406.

The list of simples, though fearfully long, does not contain more than half a dozen of any note which are not in the London catalogue; among them are Arnica, Winter's Bark, and the Solanum nigrum. Among the animal substances are frogs, vipers, snails, coral, and milk. The kind of milk is not defined in the present codex: in the one of 1818 it was said, "*Lacte utimur vaccino, caprino, asinino, quandoque equino.*"

The use of mare's milk in France is of long standing. Lord Chesterfield recommends it to his son, and says, "You will find both asses' and mare's milk enough in the South of France, where it was much drank when I was there. Guy Patin recommends to a patient to have no doctor but a horse; and no apothecary but an ass."

The calf furnishes not only his flesh, but his lungs to the list; the latter being employed to make a syrup, which is called *sirop de mou de veau*, or *syrupus cum pulmonibus vitulinis compositus.* It is composed of calves' lights, dates, jujubes, raisins, liquorice root, comfrey root, the leaves of pulmonaria or spotted lung-wort, white sugar, and river water. This is a curious mixture, certainly, and looks like what dispensatorial critics are apt to call "relics of old pharmacy." These relics in the shape of compounds containing a host of ingredients, are not rare in the codex, and form a singular contrast to the bare and shorn simplicity with which modern French practice is perhaps justly reproached. Thus the *theriaca* contains seventy-one ingredients, the *diascordium* eighteen, the *catholicum duplicatum rheo* fourteen, the lenitive electuary

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* The following may serve as a specimen:—

ARSE'NITE DE POTASSE.
(Liqueur arsénicale de Fowler.)
ARSENIS POTASSICUS

Pr. Acide arsénieux *(Acidum arseniosum)* 1 gros 18 grains, ou	5
Carbonate de potasse *(Carbonas potassicus)* 1 gros 18 grains, ou	5
Eau distillée *(Aqua)* une livre, ou.....	500
Alcool de mélisse composé *(Alcoolatum cum melissâ compositum)* demi-once, ou.....	16

The figures in the right-hand column denote grammes; a gramme is equal to nearly 18½ grains, but in the Codex is supposed to be equal to exactly 18 gr.; half an ounce is equal to 15·62 gr., but in the Codex is supposed equal to 16 gr., &c. The *gros* or drachm is 72 grains.

sixteen, the *tinctura aromatica dicta vulneraria* nineteen; and there is a *pulvis ad condienda cadavera*, or embalming powder, with sixteen.

The Codex is rather rich, as might be expected, in new remedies, among which we find tannic acid, codeine, cinchonine, brucine, emetine, acetate of quinine, sulphate of morphia, sulphate of cinchonine, hydro-cyano-ferrate of quinine, and sulphate of strychnine, none of which are in the London Pharmacopœia. Many of these might, no doubt, be spared; and some readers would be apt to cry out that the compilers of the Codex had violated both the good old rules:—

"Be not the first by whom the new is tried,
Nor yet the last to lay the old aside."

To find *theriaca*, threescore-and-ten ingredients strong, in the same Pharmacopœia with codeine, is strange, yet still defensible; for as scarcely any error can be greater in the compilers of a work of this kind than that of catering for the tastes of their own coterie alone, and forgetting that they are the medical representatives of a great empire, and must gratify their scattered constituents,

"Poscentes vario multum diversa palato;"

so it must be allowed to be a considerable merit if the physicians of Picardy and Provence, of Brittany and Alsace, find their opinions severally reflected in a national work. The pharmaceutical traditions of various schools are thus assembled together in a refined state; and what was the privilege of a single district becomes the inheritance of all.

It is to be regretted that the patriotic scheme of having a united pharmacopœia for our three kingdoms has failed for the present; indeed, as Dr. Christison has been for a considerable time engaged in a new code for Scotland, there seems no present prospect of this desirable union being effected.

CLINICAL LECTURE

ON

TUMORS OF THE JAWS; LOCAL TREATMENT AFTER OPERATIONS; CASE OF SLOUGHING CHANCRE IN A FEMALE.

Delivered at St. Bartholomew's Hospital,

By Wm. Lawrence, Esq. F.R.S. &c.

Mary Edwards, 32 years of age, a married woman, and mother of four healthy children, came into the hospital from the country, on account of a swelling occupying the alveolar margin, part of the palatine plate, and the external surface of the left superior maxillary bone. She has always enjoyed good health, but has suffered much, since the age of fifteen or sixteen, from toothache and gum-boils. The teeth have gradually decayed and broken off, leaving rotten stumps in the jaw; she has had four or five drawn; two remain in the upper jaw, where there are still several fangs worn to a level with the gum. The present tumor made its appearance two years since, when she considered it as the beginning of another gum-boil: however it slowly enlarged, without inflammation, suppuration, or pain.

The enlargement, which is about the size of a large walnut, begins at the socket of the lateral incisor, and extends to that of the first molaris: it passes upwards, nearly to the edge of the orbit, raising the lip and cheek, so as to constitute a conspicuous deformity; and it continues for a short distance on the palatine aspect of the bone. It has a dense and firm feel, resembling in these respects the gum itself. It has been, and is, quite free from pain, and will bear considerable pressure. The surface is covered by entire and sound mucous membrane, and presents some slight inequalities on the convexity of the tumor. The colour is a deep red, approaching to livid. There is no enlargement of absorbent glands, nor any other morbid affection. The swelling already interferes considerably with mastication and articulation; it is increasing, and will become more troublesome: hence the patient wishes that it should be removed by operation, if it can be done safely. I have not hesitated to assure her that the operation is free from danger, and that there is little or no fear of relapse. The slow growth, the firm consistence, the absence of pain and of disease in the absorbent glands, satisfied me that the malady was not malignant, although I could not venture to determine its exact nature. The only circumstance that could be regarded with suspicion, was the indication of considerable vascularity afforded by the deep red colour.

The patient having been prepared by the usual preliminary measures in respect to diet and the state of the bowels, as well as by removing the two remaining front teeth in the upper jaw, and two or three stumps, underwent the operation six days ago. A perpendicular incision was carried from the edge of the orbit through the entire thickness of the cheek and upper lip, terminating near the angle of the mouth. The lateral flaps formed by this incision were quickly dissected off, so as to expose the tumor completely. An incision was next made through the gum and palatine membrane at the base of the disease, and then a groove was formed by means of a small saw, in the alveolar process at the front of the swelling. This enabled me to insert the blades of a cutting forceps, and to carry the instrument along the base of the swelling, which was thus easily loosened and detached. It came away as a distinct round mass, leaving a superficial depression in the alveolar aspect and external surface of the bone, of which the substance was perfectly healthy, both in this situation and where it had been cut through by the forceps. In the excavation which had lodged the morbid growth, the bone was bare and rough, from the pressure which it had undergone, and it exhibited a little increase of vascularity. There was free bleeding from several arteries, particularly the coronary and the trunk of the infra orbital, rendering the patient faint: the coronary was secured by ligature. The wound of the lip was united by a hare-lip pin and twisted suture, applied near the edge of the red portion; a single suture was placed an inch higher up, and the intermediate portion was brought together by a small strip of adhesive plaster. The part was covered with a soft doubled rag, kept wet by dipping in cold water. In twenty-four hours after the operation the suture was cut out, and in forty-eight hours the hare-lip pin was removed, the union of the wound by adhesion being complete throughout. The wounded surface of the cheek has become united to the excavation in the bone, so that the latter is no longer visible. The patient is now, on the sixth day, sitting up in her bed well, and quite able to return to her home, in Sussex, which she means to do in a day or two.

The tumor, which I now show you, is not clearly referable to any of the various growths which have been designated by that name. It does not originate from the bone, nor is it a change of structure in the gum. It is not one of the compact fibrous productions intermixed with osseous spiculæ or laminæ, originating from a bone, and constituting an innocent species of osteo-sarcoma; it is not one of the vascular fungous growths from the gum, nor one of the compact fibro-cartilaginous productions arising from a change of structure in the gum and palatine membrane; it does not present, in any degree, the consistence and other characters of medullary growths. It is a well-defined tumor; close, compact, and homogeneous in texture, presenting a mottled appearance on a section, inasmuch as it is generally red, with an intermixture of paler portions; not otherwise connected with the bone than by being partially imbedded in a hollow on its surface, and adhering closely below to the gum and palatine membrane. It seems to me a new formation, or tumor, in the proper sense of that term, approximating in closeness and compactness of structure, as a morbid production, to the natural organization of the gum. I see in it no evidence of malignant character, and therefore expect that the cure will be permanent. It would come under the technical denomination of epulis, which includes morbid growths from the gum, or others closely connected with it.

Some years ago I removed a mass, twice as large as the present, from the alveolar edge of the left upper jaw-bone, in a strong healthy woman, 50 years of age. The disease was of six years' duration, and considered by the patient to have originated from a decayed tooth; it had increased slowly, and without pain. It was a large mass, extending from the alveolus of the canine tooth to the back of the upper jaw, where the two last grinders were imbedded in it, filling one side of the mouth, and causing so much annoyance by its interference with articulation and mastication, that the patient was very desirous to have it removed. This growth was of cartilaginous firmness, of pale colour, like that of the palate, and with irregularities of surface. Its limits could not be ascertained accurately towards the cheek and back of the mouth. The imbedded teeth were extracted before the operation. An incision, commencing a little above the angle of the mouth, was carried through the upper lip and cheek to the lower edge of the zygoma; it was slightly curved, with the convexity upwards, to avoid the parotid duct. Several arteries bled freely, and three were tied; but a large quantity of blood was lost, not less than between one and two pints. The base of the growth was then laid bare on the outer side, and a slight groove was made in the same situation by a small saw. The tumor was now readily detached with the cutting forceps, the antrum not being exposed. The wound was united by three sutures, and by a hare-lip pin placed at the angle of the mouth; and the parts were covered

by a damp cloth. The passage of blood into the throat, from vessels seated too deeply to admit of ligature, was a source of considerable inconvenience in the operation, by exciting efforts to vomit. In the course of the evening free bleeding came on, and the blood again passed into the throat. It was necessary to open the wound; a large vomiting of blood took place, and the patient was supposed to have lost twice as much blood as she did in the operation. The wound was again brought together as before. It healed throughout by adhesion, and recovery was complete in about a week, the left side of the jaw merely presenting the same appearance as in those who have lost their teeth. This tumor was of close, compact, fibro-cartilaginous structure, at least equal in density to the substance of the gum. There were spiculæ of bone at its basis, and it was necessary to smooth off two or three bony irregularities after the mass had been detached.

[We may conclude that there has been no return of disease in these cases, as both patients were addressed to me by persons whom I knew, and they promised to return to the hospital, or to let me know, if there should be any fresh growth.]

The jaws are so frequently swollen from disease of the bones, or of parts immediately connected with them, and these affections are so various in their nature and treatment, that any contribution calculated to elucidate the latter points is of some value. I shall therefore mention to you shortly two cases which have recently come under my observation :—

A healthy female, between 30 and 40, came to the hospital for my opinion respecting a swelling on her lower jaw. In size and shape it might be compared to the half of a large walnut; it was seated on the outer surface of the lower jaw, and inseparably connected to the bone. Examined through the skin it appeared firm and solid; it was readily felt through the membrane of the mouth, just below the gum ; and here fluctuation was perceptible in it. It had been growing for more than a year, and was unattended with pain. The corresponding portion of the gum contained the fangs of a molar tooth, which had been lost by gradual decay. I punctured the part from the mouth, and let out about a dessert spoonful of fluid, of watery consistence, containing the minute glistening particles which are not unfrequently seen in the fluid of hydrocele. Nothing unhealthy could be discovered in the bone. After a few days the opening was found closed, and the swelling reproduced; it now contained a thickish bloody fluid. I exposed the cavity more largely, and had the decayed stumps removed. In a little time the puncture was closed, and the tumor had disappeared.

Soon after I saw a similar but larger swelling in the upper jaw of a middle-aged woman. It occupied the excavation of the bone below the orbit, causing a conspicuous swelling, and projected a little at the base of the gum; there was manifest fluctuation. The decayed fangs of a grinding tooth had the same relation to the swelling as in the former instance. The complaint had existed for three or four years, without causing pain; and the patient was now induced to seek assistance by the swelling, which was increasing. The case was treated like the last, with the same result. The fluid contained in the swelling was similar to that in the two other cases.

[A third case has lately been seen at the hospital, corresponding with the two preceding, except that no carious teeth were present, though some had been extracted shortly before the swelling was noticed. It was in the upper jaw of a man aged about 30, and had existed about two years. About a table spoonful of fluid escaped when the cyst was punctured. A probe introduced at the opening penetrated so far, and could be moved so freely, that there was no doubt of its having entered the antrum, although fluid subsequently injected has not passed into the nose. A portion of lint was introduced to maintain the opening, from which a puriform discharge to some extent has occurred. The lint was left out at the end of three or four weeks, and a solution of sulphate of zinc was injected; but the opening has not yet closed, although eight or ten weeks have elapsed since the puncture.]

Four or five years ago I saw a boy, about 15 years of age, with a swelling of the right upper jaw-bone, apparently arising from a general enlargement of the antrum. The affection had come on gradually, and was unattended with pain. The enlargement of the bone caused swelling of the cheek, and unnatural projection in the mouth above the alveolar process. The prominent part was a little irregular on the surface, and the projections yielded a little to pressure, so as to make it probable that there might be fluid within. The palatine plate was depressed. There was no disease of the soft parts. I punctured the swelling in the mouth, and its contents escaped in the form of a thin mucous fluid. I then cut out a thin slip of the swelling with the cutting forceps, so as to expose the cavity freely. Nothing unhealthy was observed in the lining of the antrum. The opening slowly closed, the swelling subsided, and no further incon-

venience has been experienced from this disease, which seemed to be simple enlargement of the antrum from increase of its natural secretion.

I beg to direct your attention to the means employed for uniting the wound, in the case of Mary Edwards, and to their completely successful effect. You have had occasion to witness a similar proceeding and result in the case of the female whose breast I removed for carcinomatous disease three weeks ago. Although it was necessary to remove the entire mammary gland, the integuments were perfectly healthy, and were saved in sufficient quantity to close the wound, which was about eight inches in length. It was brought together by sutures and then covered with a wetted cloth. The sutures were cut out in twenty-four hours, and the incision was united by adhesion in its whole length, so completely, that it was necessary to open it a little three days afterwards, to let out some bloody purulent fluid collected in the tract of the ligatures. At the end of a week this patient was sitting up in bed quite well, and could then have travelled safely to her residence in the country, fifty miles distant; but her friends did not come for her till three or four days after.

The uniting of wounds with sutures is not in favour with modern surgical authorities, who represent it as at least unnecessarily painful, and often directly injurious by causing irritation, and consequent inflammation and suppuration of the wound. From some of the representations on this subject, it might be inferred that the use of sutures in wounds would prove a surgeon to be unacquainted with the principles of his profession. I consider these views to be entirely incorrect, and that the union of wounds by suture is much better calculated to secure adhesion than the employment of adhesive plasters. The objections to the use of sutures formerly employed, are applicable to the mode and not to the principle of proceeding. Large needles were used, and thick threads; these were carried deeply, with the object of uniting the wound throughout; and they were left in until they excited inflammation, swelling, and suppuration, and often cut through the parts embraced. We should employ small needles with sharp points and edges, and slender silk threads, such as we use for tying arteries: we should include integuments alone, or in conjunction with the subjacent adipose texture, and not attempt, in general, to unite muscular parts; and, which is a point of particular importance, we should cut the sutures out at the end of twenty-four, or at furthest, of forty-eight hours. The edges of the wound are sufficiently agglutinated to hold together before the earliest of these periods: if, however, union by adhesion has not occurred before that time, it certainly will not take place afterwards, and the sutures consequently can be of no further use. When employed in this manner, sutures are sometimes necessary, often advantageous, and never hurtful. In this mode of proceeding the wound and the adjacent parts are left uncovered, so that they can be kept cool by exposure to the air, or by the application of wetted cloths.

The adhesive plasters commonly employed are irritating, and capable of causing heat and redness in the sound skin of a healthy person; how much more likely are they to irritate a wound, where the parts may be expected to inflame in consequence of the operation. When a considerable wound, and the neighbouring surface, to the extent of some inches, are thickly covered with such plasters, with the addition of external dressings and bandages we may reasonably expect that the adhesive process will fail—that inflammation and suppuration will ensue. The local mischief and suffering are not the worst part of the evil under such circumstances. The inflamed wound disturbs sympathetically the alimentary canal, disorders the circulation and the secretions, thus inducing a constitutional disturbance which is appropriately denominated *traumatic* fever: this reacts on the wound, exciting and maintaining spreading inflammation of the integuments.

It is no slight argument in favour of sutures that we use them on most occasions when it is important to obtain accurate adjustment of the divided parts and speedy union by adhesion. The operation for hare-lip, those for the removal of the upper and lower jaw, and wounds of the face, are examples. The best specimens that I have seen of healing by the first intention have been in wounds treated by sutures in the manner already described, of which I could cite to you numerous and various instances. In a case where I removed the upper jaw, the disease having been attended with considerable enlargement, so that extensive incisions were required, the flaps were united by three or four hare-lip pins and several sutures. The latter were all cut out in twenty-four hours, and the former were removed at the end of forty-eight hours. Union by adhesion was perfect throughout, except in a small part of the wound near the external angle of the eye, where I had trusted to adhesive plaster; at this part the skin was red, and the wound suppurated.

Case of sloughing chancre in a young female.—The cases which I mentioned to you in a former lecture, of sloughing, as the primary effect of the venereal poison, were all in males. We have now in the hospital a girl of 16, whose case is interesting in reference to the natural history of syphilis, showing that the venereal poison may destroy the vitality of the part to which it is applied, without exciting surrounding disturbance or disordering health. Sarah Woodruff, in Patience ward, was servant in a family in Goswell Street. She went out in the evening with two females older than herself, and passed the night with a man, with whom she represents that she had sexual intercourse for the first time. This happened a few days before she came to the hospital. On her admission she had a slough at the right side of the entrance of the vagina; its longest axis was nearly an inch. There was no material inflammation or swelling of the surrounding parts, and little or no pain. The slough was bounded by a line, at which the process of separation had hardly begun. The appetite and sleep were unimpaired, the circulation undisturbed, and the girl appeared in perfect health. She was confined to bed; the part was poulticed; and no medicine administered, except an occasional aperient. The separation took place favourably, the depth of the mortification being at least one-third of an inch, and the surface healed rapidly, a little discharge from the vagina remaining after the cicatrix had been completed. The patient is now ready to leave the hospital. The story told by this female was corroborated by the state of her sexual organs, which were healthy, except at the mortified part. The gangrenous affection was not referrible to irritation from excessive coition, nor to any constitutional unhealthiness; it must be regarded, therefore, as a local effect produced by the application of an animal poison. The influence of the virus seems to be exhausted in the destruction of the part; the subsequent processes of granulation and cicatrisation being performed as healthily as in the case of slough caused by any other agency. The symptoms are more severe when this affection occurs in the male, where the suffering seems to arise principally from the pressure of the inflamed prepuce on the inflamed glans, and is immediately relieved by dividing the prepuce. This aggravation of the mischief does not occur in the female, where the parts are free from all pressure. The cases formerly related shew, that in the male as well as in the female, the operation of the virus is arrested by the occurrence of mortification; the dead part is cast off, and no further noxious agency is observed.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

April 10, 1838.

The President in the Chair.

On increased Thickness of the Parietes of one of the Ventricles of the Heart, with Diminution of its Cavity. By George Budd, M.B. F.R.S., Physician to the Seaman's Hospital Ship Dreadnought. Communicated by Mr. Perry.

After observing that unnatural thickness of the parietes of one of the ventricles of the heart, first noticed by M. Bertin, in 1811, and called by him "concentric hypertrophy", is generally acknowledged by pathologists as a diseased condition, the author proceeds to explain the opposite opinion of M. Cruveilhier, who holds that the obliteration of the cavity and the proportionably increased thickness of the parietes, are only results of the mode of death. "The hearts of all those," says M. Cruveilhier, "whom I have had an opportunity of examining who died by the executioner, have presented the double phenomenon in the highest degree; the parietes of the ventricle were in contact at all points." The dissent of so respectable an authority from the received opinions, together with the corroborating observations of Mr. Jackson, who found the same condition in those who had died of cholera, induced the author to compare the results of his own experience with the records of authors, with the view of determining whether it must be considered a disease, or whether it is merely a passing condition of the ventricle. The author relates four cases which came under his own observation, in which, on dissection, the state called concentric hypertrophy was found, yet no obstacle to the circulation existed in the valvular apparatus of the heart. Of the fifteen cases which he finds recorded by other writers, the author observes that four were of this kind; that six offered considerable obstruction to the circulation, from thickening and cartilaginous induration of the valves; and that the remaining five presented evident signs of congenital malformation of the heart. After a careful examination of these several groups of cases, the author comes to the following conclusions: 1st, that there was no permanent diminution of the cavity during life in the cases recorded of concentric hypertrophy of one of the ventricles, unconnected with valvular disease. 2dly, That in the six cases complicated with extensive valvular disease the diminution of the cavity cannot be explained by the hypothesis of an obstacle behind it, and in some of these cases the existence of an obstacle before

it renders it highly probable that this diminution was merely a passing condition of the ventricle, and, as the appearances of concentric hypertrophy were not more marked in these cases than in those of the former category, and as the symptoms of obstacle to the circulation observed in these cases were such as would result from the diseased valves alone, we cannot admit the existence of concentric hypertrophy in the cases we are now considering. 3dly, That concentric hypertrophy of a ventricle, with obstruction at its discharging orifice, and an extraordinary passage for the blood, occasionally exists as a congenital malformation, and generally on the right side, And 4thly, that hypertrophy of the heart, to whatever extent it exist, when it is exempt from dilatation of the cavities and from disease of the valves, does not produce any of the symptoms of an obstacle to the circulation through the heart.

INSTRUMENT FOR EXTRACTING CATARACT.

To the Editor of the Medical Gazette.

Sir,

I hope you will insert a few lines in reply to a correspondent in your last number, whose name is represented by five little stars.

If your correspondent had reflected on the subject at issue, he must have perceived that the operation proposed by me is very different from that proposed by Sir James Earle*—materially different, as to the construction of the instruments employed; importantly different, as to the mode in which they are desired to operate; essentially different, as to the object intended to be accomplished.

Brief description of Sir James Earle's instrument and operation.—(Is your correspondent's extract "correct to the letter?" "A spear-pointed *lancet*, of a *proper breadth*," employed for the purpose of incising the sclerotica sufficiently to permit the extraction of a hard (and the harder the better, or the forceps may not retain their hold) lens. My instrument is *not a lancet*, intended to incise and *extensively divide*, but a needle, so constructed as to form a *small opening*, through which it is *not* intended to remove a *large hard lens*, but a compressible capsule, which, when interposed between the blades of a forceps, occupies an almost inappreciable space. In short, Sir James Earle's instrument is intended to *divide extensively*; mine, to *puncture slightly*: his, to extract one form of disease; mine, to remove another; and although both our proposals may ultimately be rejected by practical men, it cannot be for the same reasons, for they are in almost every respect materially different*.—I am, sir,

Your obedient servant,

R. Middlemore,
Surgeon to the Birmingham Eye Infirmary.

April 17, 1838.

* I do not possess Sir James Earle's book, but my friend, the late Mr. Earle, of London, intimated, in a letter written a few months before his death, that he supposed his "father was not satisfied with the publication, and withdrew it from circulation, and subsequently destroyed all the copies;" and this he mentioned in explanation of the difficulty of obtaining the work. I have not read Sir James Earle's work attentively, but when my friend gave me all his father's writings except his "Account of a New Mode of Operation for the Removal of the Opacity in the Eye called Cataract," I concluded it would not be desirable to refer to Sir James as an authority; and, in fact, his proposal is in no respect novel. But presuming it to be novel when proposed by Sir James, in what respects do our proposals resemble each other? The removal of a *capsule* through a *minute opening* in the sclerotica, bears no relation to the extraction of a *hard lens* through an *extensive incision* of the sclerotica. The objection to Sir James Earle's proposal (though it did not originate with him) consisted in the large size of the incision, which the nature of the substance to be removed rendered it necessary to make.

CASE OF SATURNINE COLIC,

FOLLOWED BY SUDDEN DEATH.

On the 6th of January, a man aged 46 was admitted into the hospital of La Charité. He was a plater, of a dry and bilious temperament, working in a room where mixtures of lead and tin are continually melted. He had been constipated for six days, when, on the 5th of January, after having remained several hours near a chafing-dish of burning charcoal, he was seized with numbness and vertigo; and when these symptoms disappeared, he was attacked with a very painful colic. The pains in the abdomen had hardly ever ceased till the following morning; during the night they increased, and he vomited, for the first time, a small quantity of a viscous and greenish fluid. He felt as if there was a bar across the abdomen; and

* The reader may refer to the following works respecting the removal of *lenticular* cataract, by an *extensive division* of the selerotica:—Wenzell: Traité de la Cataracte, avec des Observations. Paris, 1786.—Ware's Translation of Wenzell's Treatise on the Cataract. London, 1812. Page 42. Muter: Practical Observations on various novel modes of Operating on Cataract. Page 45. London, 1811.—Von Embden and Chapman, in Edinburgh Medical and Surgical Journal, Vol. xiii. pp. 56 and 300.

the abdomen itself was not sensibly larger, but from time to time was inflated. Pressure was not painful upon any spot. The region of the liver rather resisted pressure; the tongue was whitish; there had been no stool for a week; the pulse was very slow, but sufficiently developed. Neither the skin nor the conjunctiva was tinged with yellow. The face was pale, with an expression of pain. The mind was clear, but during the night there had been delirium.

Four pills of the watery extract of opium were prescribed, and a potion containing a grain of hydrochlorate of morphia, of which a spoonful was to be taken every two hours.

The next day, the 7th, there was no diminution of the pain, but the patient had slept three hours during the night. All the symptoms were the same as before, with the addition of a slight headache. Four pills of the extract of opium were prescribed, and a potion containing two grains of the hydrochlorate of morphia. A clyster of pure water was also ordered, but it was not administered.

On the 8th of January, there had been no colic since the preceding evening; the patient had slept during the night; the pulse was more frequent, and approached the normal rhythm; the abdomen was soft, and not painful on pressure; there was slight headache. The prescriptions were, a clyster of pure water; a potion containing two grains of hydrochlorate of morphia; four grains of the aqueous extract of opium; and five basins of broth.

During the day the patient was visited by his relations, who brought him bread, wine, and figs.

On the 9th of January, about four in the morning, without having experienced any thing unusual, he went to the close-stool; his efforts or his pains made him utter faint groans, but he was not able to expel the fæces. He returned to bed; half an hour afterwards his breathing became stertorous, and he died at seven in the morning.

Post-mortem examination, 24 hours after death.

The Thorax. — The lungs were extremely congested throughout; at their apex was a black induration of long standing, besprinkled with chalky tubercles and calcareous masses, which were very hard, very irregular, and several lines in extent. The left cavity of the heart was much narrowed; its parietes were thickened, and very firm.

The Abdomen.—The liver was congested, and quite black. The intestines had those strictures and black concretions which are usual in persons suffering from lead colic; moreover, the lower part of the small intestines, the cæcum, and the large intestines, were distended by fæcal matter: some patches of Peyer's glands were visible. The kidneys were congested, as well as the spleen.

The Head.—The vascular net-work of the pia-mater was gorged with blood. The consistence of the brain was normal; the white matter was gritty, and small drops of blood flowed from it. The grey matter was evidently of a deeper colour than is usual. There was no serum in the ventricles.

This case might have been an instance of the cure of lead colic by the administration of the preparations of opium only; but unfortunately the termination prevents any conclusion from being drawn. It is probable that death was caused by the general congestion of every organ, and that this was produced by the efforts which the patient made to overcome the obstinate costiveness, which had lasted ten days, and which stopped the mass of food that he swallowed the day before.

Did not the preparations of opium contribute to the continuance of the constipation?—*Gazette des Hôpitaux*, March 3, 1838.

[This case appears to us a very glaring instance of the experimental practice of which our neighbours are so fond. Why were purgatives not given with the opiates? In this disease they are usually administered, we believe, on the other side of the water, as well as in England.—Translator.]

ENORMOUS DILATATION OF THE STOMACH.

A peasant, aged 28, lately died in the clinical ward of M. Blumenthal, at St. Petersburgh. On post-mortem examination, the stomach was found to be of immense size, so that it held fifty pints of water*. The parietes were thickened, and as white as lime.

The spleen and liver were in a complete state of atrophy; yet the stomach contained twenty pints of black bile, and the patient had continually vomited large quantities of black bile during his illness. Hence M. Blumenthal presumed, that the stomach, in this case, performed the office of the liver, and that the bile was secreted by its parietes. The gall-bladder contained a minute quantity of very clear bile.—*Gazette des Hôpitaux*, Feb. 20, 1838.

* According to Soemmerring, the stomach of a middle-sized man will hold from five to eleven pounds of water. (De corporis humani fabricâ, tom. vi. p. 214.)—*Translator's Note.*

DR. M. HALL AND PROCHASKA.

To the Editor of the Medical Gazette.

SIR,

IN reply to the observations made in the MEDICAL GAZETTE of April the 7th, 1838, p. 73, I beg to refer you to the following correspondence with the Librarian of the Medico-Chirurgical Society :—

"14, Manchester Square,
April 13, 1838.

"Dear Sir,—Will you do me the favour (though I fear I am giving you much trouble) to inform me on what occasion I have had Prochaska's works from the Medico-Chirurgical Library?

"As your note is for the purpose of publication, I will thank you to let it be very short, but full and explicit.

I am, dear sir,
Yours very truly,
"MARSHALL HALL."

"*To Mr. Williams, Librarian, Royal Medico-Chirurgical Society.*"

"Royal Med. and Chirurg. Society,
April 14, 1838.

"Sir,—In reply to your inquiries, I beg leave to inform you, that you have taken from the library Prochaska's works, on the following dates — December 30th, 1835; January 30th, 1837; January 20th, 1838.—I have the honour to be, sir,

Your obedient servant,
"THOS. WILLIAMS."
"Assistant-Librarian."

"*To Dr. Marshall Hall.*"

It is only necessary for me to add, that my first paper, "On a particular Function of the Nervous System," was read to the Zoological Society, November the 27th, 1832; and that my Memoir, "On the Reflex Function of the Medulla Oblongata and Medulla Spinalis," was read to the Royal Society on June the 20th, 1833; the former upwards of three years, the latter two years and a half, before the first of the dates on which I consulted Prochaska.—I am, sir,

Your obedient servant,
MARSHALL HALL.

14, Manchester Square,
April 16, 1838.*

[The first remark we have to make on the preceding, is, that we think it shows very considerable liberality on our part, to admit any communication whatever from Dr. M. Hall, after the unjust and abusive attack made by him, upon this journal, in a recent lecture. Our next remark is, that the Librarian's letter shows that Dr. M. Hall did take Prochaska's work out of the Medico-Chirurgical library in 1835. It is thus unequivocally proved, that, supposing the worthy Doctor never to have seen any copy of Prochaska except that in Berners-street, still he has been in the habit of consulting the work in question during three years; whereas the contents of the volume are brought before the English public now for the first time—not by Dr. M. Hall, but in spite of him.—ED. GAZ.]

* There is a P.S., which, though but of two lines, is an advertisement; it may appear on the wrapper next week, if Dr. M. Hall desires it.—ED. GAZ.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Apr.* 10, 1838.

Abscess	2	Inflammation	12
Age and Debility	35	Bowels & Stomach	1
Apoplexy	5	Brain	4
Asthma	9	Lungs and Pleura	9
Childbirth	3	Influenza	1
Consumption	34	Insanity	1
Convulsions	21	Liver, diseased	1
Dentition or Teething	2	Measles	4
Dropsy	6	Mortification	1
Dropsy in the Brain	5	Paralysis	1
Dropsy in the Chest	1	Scrofula	1
Erysipelas	2	Small-pox	12
Fever	25	Sore Throat and Quinsey	2
Fever, Scarlet	2	Thrush	2
Fever, Typhus	3	Unknown Causes	192
Gout	1		
Heart, diseased	3	Casualties	10
Hooping Cough	12		

Increase of Burials, as compared with the preceding week . . . 78

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

April.	THERMOMETER.		BAROMETER.	
Thursday . 5	from 38	to 59	29·94	to 29·86
Friday . . . 6	41	60	29·68	29·60
Saturday . 7	45	52	29 55	29·24
Sunday . . 8	35	47	29·13	29 45
Monday . . 9	31	48	29·65	29 88
Tuesday . 10	25	65	29 95	30·03
Wednesday 11	45	67	30·04	30·05

Winds W. and S.W.

Except the 6th, 7th, and morning of the 8th, generally clear; rain on the 5th, 6th, and 7th.

Rain fallen, ·3875 of an inch.

Thursday . 12	from 32	to 57	30·09	to 30·12
Friday . . 13	29	51	30·14	30·04
Saturday . 14	37	50	29 98	29·87
Sunday . . 15	36	61	29·75	29·68
Monday . . 16	26·5	40	29·66	29·60
Tuesday . . 17	25	45	29 57	29 64
Wednesday 18	26	39	29 64	29.71

Winds W. by N. and W. by S.

Except the 15th, cloudy; snow and hail on the 16th and 18th; rain on the 17th.

Rain fallen, ·2 of an inch.

CHARLES HENRY ADAMS.

ERRATA.—Page 78, col. 2, line 34, *for* "scial," *read* "sciat;" line 40, *for* "theoists," *read* "theorists."

WILSON & SON, Printers, 57, Skinner-st., London.

(Extra limites.)

DISPROOF OF DR. ALCOCK'S STATEMENTS IN HIS LATE LETTERS TO THE "MEDICAL GAZETTE;"

Whether Anonymous or with his Signature.

To the Editor of the Medical Gazette.

Sir,

To understand these remarks, your readers must remember that six letters in your periodical; viz.—First in No. 18, 20th Jan. (pp. 655—659); second in No. 20, 10th Feb. (pp. 767—770); third in No. 23, 3d March (pp. 909*—911*, extra limites); fourth and fifth in No. 24, 10th March (p. 932, and again at pp. 941*—943*, ditto); sixth in No. 27, 31st March (p. 1040*, ditto); the first and fifth signed "A Professor in the School of Apothecaries' Hall, Dublin;" the second assuming a new signature, as "A Member of the College of Surgeons of Ireland;" the third, fourth, and sixth, shifting their ground again to the name of "Benjamin Alcock, M.B. M.R.C.S."—are all written by an individual, and therefore but one testimony as to any fact, but one judgment as to matter of opinion. Both witness and sentiment too are those, recollect, of a person interested to uphold his own profits, depending on the success of illegal practices of the apothecaries' body, and unlawful pretensions of their Hall-School; with which, unauthorized by their incorporation, contradicted by the charters of other bodies, and disavowed by his College, he has lately joined himself. But statements or assertions, in any one letter, if contradicted by another, must be conclusive, of course, against the correctness of testimony, or soundness of judgment, of the identical writer of all.

Thus, to use Dr. A.'s words (p. 942*), "The next paragraph of his letter furnishes matter for much and grave comment." For (p. 767) he says, "Hitherto I have looked on, because it did not appear to me that the time had arrived when interference was likely to prove seasonable or effectual." He had, to use his words, as to others (p. 942*), in this "misconduct," the "cunning" not to put date or residence to that letter; but it was not published till "10th February," and yet his first, as "Professor," was dated "9th Jan." and had appeared on "20th Jan." Thus he changed his primary for an equally anonymous signature (in the same journal, remember), making it appear as independent testimony of a second witness; and states, he had formerly only "looked on," not "interfered," because the "time had not arrived." This Dr. A. would perhaps call, as to another, (p. 941*), "very proper and dignified" conduct, and "becoming amazingly a gentleman:" your readers well know by what terms to characterize double dealing, for I will not use Dr. A.'s phrase (p. 942*) "duplicity;" but I may add his words (p. 493), "I cannot permit myself to comment at length upon the assertions which I have adduced; wilful misrepresentation I do not impute to the gentleman;" "but the man must be silly indeed who could persuade himself that such errors would be permitted to pass without exposure. I have left only the alternative, '*aut insanit homo, aut versus facit*,'" &c. Let your readers choose either alternative.

In page 909*, Dr. A. *naively* acknowledges a reason for neither signing his name to the second letter nor continuing his designation "Professor of Apothecaries' Hall School;" either would suggest he had perhaps a self-interest in the question, blinding himself, or misleading his readers, which therefore might make him take or give a partialized view, of which "a Member of the College of Surgeons" would not be suspected. He says, it was "under the impression that the letter of a Member of the Corporation would excite more attention than my own name would command." We may fairly take his own explanation of motives.

But to proceed to business: Dr. A. says justly, p. 909*, "If my letter was, as asserted by your correspondent, neither more nor less than a collection of direct falsehoods, what more was necessary than to have refuted them?" I shall therefore proceed to do so, though not his previous answerer, twice under the signature of "A Member of the Royal

College of Surgeons in Ireland" (in pp. 732—734, and 819—820).

In pp. 910*—911*, he professes to make two quotations from the "Licentiates' Committee's Report," but instead of quoting from its correct copy, as furnished to the College by their secretary, he does so from an imperfect one, obtained, as he states, through some other licentiate; while, in the sentence first quoted, he omitted the material words, "who does not interfere with practice," after "unjust to the apothecary," by which the sense is precisely reversed. I have authority from the secretary of the Licentiates' General Meeting and Committee (Surgeon Nixon), to say he never gave or made any fair copy except that sent to the College, nor authorized any copy to be made or given; that he never showed, or permitted any one to show, his rough copy to him; nor even to read it to him, or to any one else; and did not know of any one having done so till Dr. A.'s letter appeared. The perusal or copy of the incorrect rough draft must, therefore, to use Dr. A.'s words (p. 768), have been obtained by "surreptitious" means, by some one for him. May we not use his words to you, Mr. Editor (p. 911*), "You have been duped;" and (pp. 941*—942*), "Alas! this is but a sorry shift; were it true, I should agree with him, but it is not, emphatically not!" "And am I to be responsible for their" (his) "misconduct? or to be the victim of their" (his) "cunning? Assuredly not; nor shall any power make me so." "And now let the party who allowed themselves" (himself) "to be betrayed into so rash and unseemly a step, retrace it as they" (he) "best can. If not, let them" (him) "pay the penalty, and pay it they will" (he shall). Dr. A. justly asks, "Can you, sir, after this have any doubt that my statement is incorrect, if not false; and that that, with which he has" (I have) "favoured you is the true one?" To this appeal, of course you, Mr. Editor, will respond, by truly characterizing the statement; and your opinion will be confirmed by the fact that the first sentence quoted by Dr. A. is introduced in the Licentiates' Report by these words, which he has omitted: "Your Committee, in adopting this view, do not seek or desire to deprive any man, or set of men, of the just profits of their calling; but they do not hesitate to offer it as their conviction, that the intrusion of the apothecary into the place of the physician and surgeon, and his assumption of their duties, is as injurious to the public, as it is unjust to the qualified practitioner; and they have to express their deep regret," &c. &c. as finished by Dr. A. (p. 911*); and, at the end of his second quotation (on same page), he has omitted these words, after "to bend;" "but which they must adopt in their own defence if they cannot succeed in compelling the apothecary to remain in the position for which he was intended; and where he would be most useful to the public; namely, as the *servus medicorum.*" In page 770, he says: "Why has the protest of the Licentiates been withheld from the College? Why have the Committee not even alluded to it in their Report?" &c.; and, in page 909*, he says: "He has pronounced my letter to be a collection of direct falsehoods;" and he has "pledged himself to support his statements by reference to the minutes of the College and its Committees." I accept his challenge, and I call upon him to redeem his pledge; I call also upon you to bear witness to the pledge, and to be umpire in the quarrel;" and, in p. 911*, "you have given insertion to the charges against me, on a pledge that they shall be substantiated," &c. &c. But, as Dr. A. says (p. 909), "And now for the falsehoods;" and (p. 911*), "he shall not be permitted to take shelter under any pretext," &c. First, it is only by omission of the above words he has any shadow for calling it a "protest;" as it is the reverse, and in favour of all the arguments, causes, and reasons of the College plans; and next, in contradiction to the writer of these six letters, under three signatures, contradicting themselves too, I state, with my name, that the Licentiates' Report was laid on the College table, at a large meeting, by my hands, and its substance stated, by my mouth, on "9th January 1838;" your publication of this assertion in Dr. A.'s letter being not till "10th February;" and, moreover, he was present at the time; and, after I concluded, rose to ask a question,

and did not leave the room until subsequently. To use his own words, can it be, that this "violence" (to truth) "was a last resource, — a gamester's hazard, to retrieve the falling fortunes of his party? I cannot answer; the result must decide." For, as he says (p. 942*), "It can hardly be supposed that any man would so commit himself as to contradict another, without being assured that he himself was in the right; or that he would expose himself to be convicted of absolute and inexcusable," &c.

Even Dr. A.'s quotations from that Report, incorrect and limited as they are, prove, that in all main points, motives, and plans, they agree with the College members and its Committee; and entirely, as to the evils of present laws and customs, their causes and remedy. But being either wilfully ignorant, or intentionally concealing, that "apothecary" means, with our members and licentiates, a person making up the prescriptions of others, who are physicians or surgeons, he thinks, or tries to lead you to think, that the licentiates oppose our plan of giving our members and licentiates, and "all qualified persons, a right to prepare and administer the medicines necessary in their practice." This explains the fallacy of his imperfect quotation of the Licentiates' Report, "expressing their deep regret that, to the discredit of the medical profession, instances are not wanting where a licensed practitioner has combined the PROFESSION of a PHYSICIAN with the BUSINESS of APOTHECARY; an union which your Committee strongly deprecate, as one disreputable to our profession, as at present instituted, unjust to the apothecary, WHO DOES NOT INTERFERE WITH PRACTICE" (which latter words Dr. A. leaves out), "and highly dangerous to the interests of society." Thus he represents them as adverse to our plan of general practitioners; concealing that such will not at all act as "apothecaries," for others or the public, but only make up their own patients' medicine, if they like: let your readers turn to p. 820, and they will now see the other "Member's" contradiction to "No. 5" of Dr. A. perfectly borne out; and, as Dr. A. says (p. 910*), "What think you now, sir? and where is the falsehood now, sir?" But the following quotation of another part of that Report will conclusively show, that when the Member said of Dr. A.'s assertion (in p. 770), that "the licentiates of Dublin have actually protested against the measure," "this is totally false" (p. 820), he spoke truly, though rather roughly.

"Your Committee have now to offer to your attention the conclusion at which they have arrived.

"In investigating the causes of the grievances under which the Licentiates labour as a body, and of which they at length loudly and justly complain, it is not to be denied or overlooked that their principal, it might also be said, their only cause, is to be traced to one source; namely, the intrusion of the apothecary into the place of the regularly educated physician and surgeon, and his assumption of the duties which properly belong to them.

"While every desire should be evinced, and every effort made, to co-operate with the Pharmacy Committee, and the members of the College generally, in their plans for the improvement of the profession, it should, at the same time, be distinctly understood, as the feeling of the Licentiates, that in any Act of Parliament, to be sought for with the hope of redressing the disabilities under which they at present suffer, this real cause of these disabilities should be brought prominently before the legislature; namely, the assumption to themselves, by the Apothecaries, of the privilege of visiting and prescribing for the sick, and undertaking, in very many instances, what they are pleased to call the minor operations of surgery; and this, too, in most cases without the necessary previous education, to the manifest danger of the public safety, and the great injury of the just rights of the regularly educated physician and surgeon." Is that a protest against the College and Committee?

Let me next premise, that the "Committee of the College for revising its charter, &c. &c." commonly called, for brevity, "Pharmacy Committee," being like all others, open, may be attended by any Member; Dr. A. was present at the "notice" for the Committee, and at its appointment; I have also met him at it; let him say too, did I not, in Committee,

ask him to let me propose him, as an additional member? did he not say, it would take five hours to explain his reasons for not complying, and his objections to our views? Did I not say, what we wish to hear are precise objections to any measures we are consulting about, in order to decide wisely; as we are not such fools as to think, "No doubt but (we) are the people, and wisdom shall die with (us)?" Job. xii. 2. Dr. A. also got the "Registrar" of the College to show him our fair book of proceedings, and examined it for a long time privately in our absence once or twice. Yet still he says (p. 110), "Why have the Committee not even alluded to it in their Report (the Licentiates' Report)?" Why did he not state, as he saw in our minutes, that our Report was agreed to on "5th January," the Licentiates' not till "6th," which, too, was only sent to us on the "8th," and, therefore, could not be included in ours, the fair copy of which was then presented, to be ready for the College meeting next day, viz. the "9th," but both were presented to the College on the "9th" by my hands, ours read at length, and unanimously adopted; the substance of the Licentiates' Report, stated by me orally, and itself laid on the table, with ours, in the hearing and view and for the inspection of all the members present, of whom Dr. Alcock was one.

After this explanation let me adopt his words (p. 770) attempting to vilify the Committee, and address him with his "readers;"—"Let them answer also this question: what moral epithet does such a proceeding deserve, or of what kind must be the cause which requires it?" This he calls (p. 909*) the contrary of "abandonment of temper and of courtesy" and of "vituperative violence." If so, I wonder what he means by these words. His own rule is, "no man loses his temper until he has lost his cause." "But it shall not pass." He says (p. 941*), "He that attacks, and chooses his weapons, is not at liberty to decline. He is bound either to submit or see it out, &c.;" and "yes, yes, but though he will not hairsplit, he seems not to consider it beneath him to gloss and to pervert." As Dr. A. says (p. 941*), "Such provocations as these are beneath my notice;" and his assertion "is a calumny, and an unfounded one." In p. 770 he says, "Next, sir, for the communications from the country:—The Committee, according to their own statement, received nearly one hundred communications from members and licentiates in the country; some adverse, the majority favourable." Now there are about thirty members, and upwards of two hundred licentiates, resident in the country; so that making due allowance for the word "nearly," and for the adverse communications, the proportion of those favourable to the change does not exceed a third of the entire. Let his readers refer to the Committee's publication, headed "Irish College of Surgeons—Pharmacy Laws," in your "GAZETTE," (p. 667-668), and see, 1st, that he wholly misstates fact; for the Committee never made any statement as to "nearly one hundred communications," nor as to "some adverse," nor "the majority favourable." (In one of the other "Member's" letters, which are not from the Committee at all, this expression occurs, "97 or 98 communications were received, of which two were adverse, one doubtful, and the remainder unreservedly favourable." 2nd, That the words "nearly," and "adverse communications," do not occur, nor any words like them, in any part of our advertisement; but only these, "the Committee also stated, that they had received communications from all parts of the country, expressing satisfaction at the prospect of a remedy for the present abuses, and affording most valuable information and suggestions." Does not Dr. A. state nearly the same truth (p. 76), when saying, "As yet the tide has flowed in one direction." In page 910* he says, "And here I have to lay before the profession a flagrant breach of privilege committed against me; I have sought to see these letters; it is my right, as a member of the College, to see them, if I please, and when I please; and they are withheld from me." I shall not stop now to disprove his asserted right, as a member, to see the documents of a Committee, entrusted with a particular business by the College, when and where he pleases; as the well-known practice of the College, which does all

its business by open Committee is, that every member may attend them, but that in the intervals all the Committee's documents are kept locked up in the committee boxes by the Secretary or Chairman of each; until on the conclusion of the business and dissolution of the Committee, they are handed over to the College Secretary or Registrar. Until then, any Member not on the Committee wishing to see or examine them, must make a motion to that effect in the College, and obtain leave, especially if he wants to quote them. But let me ask Dr. A. was not this Committee open? Did I not see him at it? Could he not be present any, every, and all day; and thus hear all letters read or received? Did he not privately, in our absence, get our minute-book from the Registrar and spend a long time looking over our proceedings, and the documents pasted or fastened into our book, which happened not to be then locked up by me? But if right when stating he never has seen our letters at all, what becomes of all his calculations as to the proportions in favour of or against our plans! I am not the "Member of the Royal C. of S. in I," who wrote answers under that name to Dr. A.'s first letter, signed "A Professor;" or his second, which he signed "A Member of the C. of S. in I." (See these answers in "GAZETTE," No. xix. pp. 732-734; and No. xxi. pp. 819-820); but as I have been Secretary of the above-named Committee, I as a member of the College, distinctly state, that all the contradictions in Dr. A.'s statements by that Member (whom he states, in p. 1040*, to be Dr. Maunsell), are true and well founded; and I shall further prove in this paper that Dr. A. has misquoted documents, and equally misrepresented and maligned the motives, designs, and conduct of the College and Committee; his assertions or re-assertions of the matters denied by that member are in vain; for facts, truly stated, do not support them, and his arguments also are mere fallacies.

I say also, distinctly, that I now believe Dr. Alcock came to that Committee chiefly to serve the views of the "Apothecaries," and of their and his "School," and privately examined our minutes and documents, to gain means of attempting a reply to that Member, and to serve personal interests, connected with their usurping and lawless attempts, who induced him to help them in an illegal school, and illegal claims, by becoming a "Professor" in it; and that College interests cannot be safely entrusted to him or his fellows in it. Besides, his own statement (p. 932) "Allow me now to protect myself against another misconception on the part of the profession: I am an advocate for the possession, by every physician and surgeon, of a right to give medicine if they please, but not to deal in or sell it, proves he had no valid objection to join us, but solely that so he would give offence to his masters, the "Apothecaries." By the by I pity him, when they read this, his opinion, which will probably make them distrust or dismiss him. In truth, he thus shows agreement in essential matters with the College views and its Committee's plans; for all we want, as to physicians and surgeons (as our statement in your pp. 667-668 proves), is "to enable all qualified practitioners to prepare and administer the medicines necessary in their practice." But he cavils at our stating that the object is "not, however, for the purpose of deriving income from the sale of them, but to secure correctness; and to deprive the apothecaries of the opportunity of meddling or tampering with our patients." This he asserts (p. 768), was intended to deceive the reader into thinking medicines would never be charged, but given gratuitously; saying, "This is as great a mistake, on the part of the public, as I believe the assertion was deceitful on the part of the writer." Though I was not the writer, yet as it was fully sanctioned by Committee, I may say it was not at all intended to convey that meaning; but merely to express that the object was not profit, but security against interference; not money, but assurance of genuineness of medicine. In fact, the design was so universally known, that such misconception was not supposed possible; otherwise that brief abstract ought perhaps to have been worded more diffusely. We had previously, too, published that we only wished ourselves, alike with others, to have a right to make up our medicines if we wished; and our plan was, to let

every man give them or not as he, individually, might please. For it never entered our thoughts to make rules, saying to any physician or surgeon, "You must always or never sell," or "always or never give." We interfere not with individual or local circumstances, but let each man and place regulate the matter as there requisite. A great many members, myself among them, and many licentiates, do not care as to ourselves, individually, what rule should be proposed; for we neither intend to keep, make up, give, or sell our own, or any others' medicines; but to continue sending prescriptions to pure apothecaries, who are honest men, did and do deserve support; but we are determined every member and licentiate, actual or future, shall have power and right to make up his own recipe, giving or selling it as he and his patients like. The present law in Ireland makes it illegal, and even penal, to make it up at all, to administer it to our patients, gratuitously, or even in urgent cases; an intolerable grievance, to which we will no longer submit, junior members or licentiates, nor those entering the profession.

Dr. A. says (p. 910*), "The communications from the members and licentiates resident in the country have been removed from the College;" thus insinuating something wrong. Yet himself says, he knew their number, and various enclosed statistical returns of practitioners throughout Ireland caused them to be "entrusted to a clerk to make an abstract of them!" and with three notes of admiration he adds, in amaze, "An abstract of 98 (108) letters from different persons!!! How can any man talk such nonsense? Of what use would multitudinous letters and extensive returns be unless reduced to abstract and tabular forms, to facilitate re-perusal and reference, index, and digest? He says, "The Secretary of the Committee declining to inform me who this person is, or where he is to be found, without leave of the Committee, who have no authority in the matter." This, as he says (p. 941*), "casts a personal and individual aspersion on myself;" by misrepresenting two notes of mine to him, when he asked me to send them to him; and finding I had not got them, to direct the clerk to let him examine them. I told him they had been necessarily put into a clerk's hand to abstract and tabulate, as the College "Registrar" had not time; and that as this was ordered by Committee, and would take at least a fortnight's hard work, I must decline to call them back out of the clerk's hands, or put them into his, without the Committee's leave, and though not aware of his exact object at the time, as he was not then avowed to be the anonymous writer in your columns, I now believe, that had I entrusted them to him, I should have betrayed the confidential communications of our correspondents to his friends the Apothecaries; and that he would, as I have proved in other instances, probably have misquoted, or have culled sentences and half sentences, that could only be proved, not to mean what he said, by publishing all the letters in your columns, which, as you would not permit, would leave his statements uncontradicted. And specially on seeing his signed letters in your "GAZETTE," we all became aware what little respect he would be likely to pay to privacy of College documents, or even statements made confidentially to it by correspondents; and that they would not be safely entrusted, without risk of misquoting, or partial quotations, even without the writer's leave had or asked; and as he had full power to hear all read, on arrival, I trust the Committee will now not give them to him, or any similarly hostile member, till they resign all into the College hands, on completion of their business; and I am sure the College will not permit any member to betray the confidence of their members or licentiates in the country to the Apothecaries, his task-masters.

In p. 658, Dr. A. says, "The School of the Apothecaries' Hall "has been established by a body having a charter." In p. 767, he acknowledges it has not, by saying, "There exist, in Ireland, three medical corporations, &c. &c., either by royal charter or by act of parliament." As "Professor" in that school he must know that even the Hall itself has no charter, but was established by act of the Irish parliament on 24th June, 1791," and their attempted school is not only

uncontemplated, but contrary to its letter and spirit. Will he shew one word in their act, authorizing them to expend funds, time, or ground, in doing what their authorized advertisement, in the "Post-Office Directory, for 1838," states thus, "The Governor and Council have erected a spacious theatre at the hall, in which the annual courses of lectures, &c. &c. will be delivered, commencing first day of November, and ending last day of April." (See also "Pettigrew and Oulton's Dublin Almanack, &c. 1837, p. 149.") He says, "The School of Physic has a charter:" as professionally educated and resident in Dublin, he must know the School of Physic to be established by an "Irish act" of 1800, not by charter; that "the King and Queen's College of Physicians" charter of "William and Mary" does not contemplate a school at all; and that even the three professors, attached to the University, are part of its general education for all students, who are obliged to pay for them, and it is only by "Act of Parliament" they and three other professors at Sir P. Dun's hospital, are combined and constituted a school of physic. He says, "But they say, the school of the College is a chartered school—a chartered schol, indeed! Chartered, in an Irish sense of the word, *i. e.* a school without a charter. Where is its charter? It has not one." It has not; albeit it has been set forth in the Medical Almanacks as one of the two chartered medical schools with which Dublin is provided, 'and as established by act of Parliament.'" And again, (p. 941*), because this mis-statement was not, with others, contradicted by the second "Member," he re-asserts the same. "I repeat that the school of the College was established without any authority of charter; and neither in its present, nor its former one, is there provision for the foundation of a single professorship." The College of Surgeons' school is never published as one of the Dublin chartered professional schools, for it is the only one, nor as "established by act of Parliament," for it is by "royal charter." Will he explain why were many thousand pounds twice granted by Parliament, except to build and set on foot its school? A member who promised to obey its charter, Dr. A. must have read its sections, which specify the way in which funds are to be obtained and applied, "sufficient for keeping the several buildings and schools of said College in repair, enlarging them, when required, &c. &c. as well as for discharging all salaries, and defraying all other expenses which said College may incur." Where is memory gone, when he makes statements that any one can contradict by his own documents? As he says (p. 941*), "those who live in houses made of glass, ought not to throw stones."

Dr. A. (p. 941*) glorifies himself that his first letter, as "Professor," had met no "reply" from the "author" of the newspaper letter, against apothecaries' misconduct, whom he calls "the original calumniator," "one of the parties who have assailed us" in a "wanton assault" (p. 941*), to which squib he alluded in it, and had expected that author's "imputation either justified or retracted by him." You, Mr. Editor, know the author was not either "unable or unwilling to maintain his charge," that his answer was written at once and sent, and that it was not that "author's" fault that it was not published long since; a delay of which he had cause to complain, for as Dr. A. commenced the controversy in your pages, the writer on whom he commented should have been first allowed to reply; and yet, four letters of Dr. A. and two of another, in reply to two of his, were inserted since, and Dr. A.'s fifth, too, was allowed to state, uncontradicted, that his first had not been answered by the "Graduate of three Colleges;" namely, myself.

In p. 943*, Dr. A. gives, between inverted commas, as quotation from the other "Member's" letter, a sentence which he introduces thus, "Those who have perused his succeeding paragraph will, I suspect, entertain but little doubt upon this point. What does it say? Hear, College of Surgeons in London! Hear, College of Surgeons in Ireland! Hear, ye sister, and hitherto rival institutions, now such no longer!" "The apothecaries of England are the brethren of the Irish surgeons?" and then Dr. A. himself adds, "and, of course, the Apothe-

caries' Society of London is the sister institution of the Royal College of Surgeons in Ireland!" This is wholesale fabrication; no such sentence occurs in either of the "Member's" letters, nor in any other publication to which Dr. A. refers. The "Member's" words are, in addressing you, Mr. Editor, "Sir, you will no doubt recognise an identity between the Irish apothecary and the English chemist and druggist; let not the name of apothecary, by its different signification in England, blind your READERS, nor prevent them" (your readers) "from sympathizing heartily with their" (your readers') "brethren in this neglected country." I adopt Dr. A.'s words (p. 943*), as to this falsification of another's words, for he is not so ignorant of grammar as to mistake: "This speaks for itself, and needs no comment, but will the C. of S. in I. permit the statement to pass without notice, and without contradiction?"

On his own fabricated statement (p. 943*), that "English apothecaries" and "Irish surgeons" are "brethren," and, "of course," that "London Society of Apothecaries" and "Irish Royal College of Surgeons" are "sister institutions," he rises to vent treasured spleen and bottled wrath, thus: "If it be true, it is time to admit that all the College's past claims have been but pretension; that she has robbed alike the public and the profession; and that she has been as dishonest as pretending;" and the reason he gives is as ludicrous as irrelevant, "for the apothecaries neither profess surgery, nor even require attendance upon a surgical lecture," as if surgeons' practice consisted only in "surgery," and their education solely in attending "a surgical lecture" course. He then seems to glory in anticipating the fall of his College, and waxes grandiloquent, "It cannot be—the Institution which would be so degenerate as to sanction such a disavowal of duty and station, has fulfilled its destinies, and is unworthy to continue. Its sun has gone down, and the hour of its dissolution has arrived;" and in prospect, I suppose, of the beneficial effect this would have on his and his fellows' classes, at the "Hall School," and upon the Apothecaries' Company's usurping plans, he adds, "but let us hope for better things—the profession can now indeed decide whether" the College members, in their present plans, "are desirous to degrade it and its members from the station they have hitherto held." This vacination of ruin and dishonour Dr. A., in his fourth letter (p. 932), calls his being "the advocate of the College; its defender," "warmly attached," and "contending for the preservation of the College." However, let us see, is his assertion, that apothecaries do not "profess surgery," or "even require attendance upon a surgical lecture," true? All know they pretend to practise it; I stop not to prove that "profession." But did not Dr. A. know and sanction the following circular? "School of Apothecaries' Hall." "The next winter season will commence, Tuesday, 31st October 1837, at two o'clock; and terminate 30th April 1838." "The business of the School will be conducted by the PROFESSORS, as follows:" "SURGERY, every day, 3¼ o'clock, Mr. Ellis;" "The fee to each course two guineas." "The anatomical and surgical departments will be conducted by Professors Alcock and Ellis, in the School of Anatomy and Surgery, Peter-street, until the buildings of the New School of the Apothecaries' Hall be completed." Does not this contradict his assertion, that they meddle not with surgery? Did he not know, and sanction, this circular also? "Apothecaries' Hall, Mary-street, Dublin; October 9th, 1837." "Sir, I have been directed, by the Council of Apothecaries' Hall, to communicate to you that they have extended their curriculum of study, according to the annexed standard," &c. "The Council further beg your attention to the circumstance, that although the course of study has been extended, the time occupied therein must not, of necessity, be prolonged beyond the term of two years, the time hitherto allotted for the same purpose." "Curriculum of study. Candidates for the diploma of the Apothecaries' Hall will, after the first of October 1839, be required to produce proof of having attended a course on lectures on each of the following subjects, delivered in the School of Apothecaries' Hall, or in some School of Medicine recognised

by the Court." "The order of study here laid down is recommended for the guidance of students." "Surgery, six months." "School of Apothecaries' Hall." "Professor of surgery, Andrew Ellis, M.R.C.S.I. surgeon to Jervis-street Hospital." "In the course of the last year, six months' attendance of the entire practice of a Medico-Chirurgical Hospital, recognised by the Court, containing at least fifty beds, in which clinical instruction is regularly given."

"Signed by order of the Council,
CHARLES HENRY LEET,
Secretary."

Now, in spite of all this, Dr. A. has dared to say, the Hall does not "profess surgery, nor even require attendance on a surgical lecture." Its own "secretary," his own school's advertisement and circulars, say it does interfere, it does require surgical lectures; it does require surgical hospital, and it does require clinical surgical instruction; but perhaps he will say, he only postponed truth to his present purpose, by using "profess" and "require" in the present tense, because the circular says, they will, next October, require a certificate. A mere evasive equivocation, unworthy of any man, and besides not the fact, for a young man intending to be examined, "1st October 1839," must, by this new circular, and what they pretend as to "two years'" study, have commenced two years before, just when Dr. A. and Mr. E. were appointed, namely, last year, in 1837; so that they do now require young men to attend, to be prepared with certificates, requisite next October, ere examination,

Besides the authorised advertisement of "Apothecaries' Hall, incorporated by act of parliament, in 1791, for regulating the profession of apothecary, &c. &c. and establishing a Hall in Dublin," &c. contradicts him by saying, in page 156, of "Pettigrew and Oulton's Dublin Almanack and General Register of Ireland, for 1838." "The School of Apothecaries' Hall, established by the Governor and Council, under direction of the Court of Examiners, for the better promotion of the professional education of candidates for license." "Attendance upon the several courses of lectures, in each department of medical science, with hospital practice, as prescribed in the published curriculum, as well as indentures of apprenticeship, are indispensably requisite, in every person seeking to be admitted to an examination for the diploma of the Hall."

"Professors."—"Anatomy and Physiology," "Benjamin Alcock, A.M. M.B. M.R.C.S.I."

"Surgery, Andrew Ellis, M.R.C.S.I. Surgeon to Jervis-street Hospital."

(See also the same statement, as to all the other lectures in the same book, for 1337, p. 149.)

In p. 767 he represents the plans of the College as "a change proposed to be made in the character and position of the C. of S. in I." and as if "the welfare of the public, and character of that College were at stake," and as "altering its original intention and character;" whereas it is merely an assertion of its constant and unchanged right. This he calls "strictly and temperately weighing the matter." In p. 708 he says, "hitherto the C. of S. has been, and still is, a corporation devoted exclusively to surgery;" yet he knows it has professors of medicine, pharmacy, chemistry, midwifery, and requires attendance on them, and examines in them.

Is it any wonder, when a Committee of many members—at first unanimously appointed, their report next unanimously adopted, their petition then unanimously approved—found that some one, under the name of "member," had anonymously misquoted documents, falsified facts, maligned conduct, traduced them, misrepresented proceedings, both of College and of a Committee, empowered to take all necessary steps to accomplish College objects, they should order the "registrar," to write to ask who the masked scribbler and anonymous caviller was, that we might at least defend ourselves. In doing so, we had the sanction of official authority; and for it, we deserve the commendation of the College, and shall receive it for demanding its innominate maligner's cognomen and domicile. I shall recur to this matter. Let me now explain, that the *tactique* of all Dr. A.'s letters is, to assert, first, unfoundedly, that the College, its committee, and members, have, in a "wanton assault," "assailed" (p. 941*), and not merely

defended themselves against him and the apothecaries. Secondly, that all acts of the College are acts of "a party" (p. 941*), in it "who would subvert its original intention and constitution, and change its character and functions," though he knows full well, that "the Committee for the revision of the charter, especially in reference to getting that part of it which prevents the members and licentiates from dispensing medicines, even to their own patients, remodelled," which Dr. A. calls the "pharmacy committee" (p. 910*) was unanimously appointed, on "9th November," its report adopted unanimously on "9th January," and its petition, founded thereon, similarly approved unanimously on the "16th;" and then, thirdly, after abusing the thrice unanimous College as a party, to say (p. 932), he has "not entered the lists against the College," that he is not attacking it, but, "to the utmost of his power," "promoting its reputation, honour, and dignity," according to his "oath." (See his quotation of it in p. 657.) Thus, in p. 655, and again in p. 943*, he says "they are desirous to degrade the College and its members from the station they have hitherto held;" and in p. 656, "Out upon them—out upon them—grovelling guardians of the science and honour of surgery!" Now what is the degradation? When I visited the late celebrated Surgeon Hey, in 1812, and was again in 1813, on a visit at his house, and attended the "Leeds Hospital," he supplied, and had all his life, and did till his death, supply his private patients with medicines, kept, made up, dispensed, booked, billed, and charged for, from his own "pharmacy." Was any man in England more respected through life and less degraded? Was he not several times Mayor of Leeds? Did his doing so degrade the London College? Again, when a student in Edinburgh, about the same time, my friend, Dr. Abercromby, whose friendship I was so happy as to acquire, and happier to retain ever since, supplied and charged his own patients similarly; though he has long since given it up. Did he degrade himself thereby? or was he a faithless guardian of the honour of Edinburgh College of Surgeons, or University; of both of which he was, and is, the most distinguished professional member in the three kingdoms? What we want, is only to be let do, what Hey, Abercromby, and hundreds of honourable, eminent, and respectable men have done, and do, in England and Scotland, and this Dr. Alcock calls "grovelling."

You have correctly stated the fact, when you say (p. 907) that Dr. A., by his letter, "has come forward spontaneously, and declared his readiness to enter the lists against the Irish College." Let him remember your unbiassed description of his letters, and see how exactly it agrees with that given by the author of the newspaper letter, signed "A Graduate," &c., which he had quoted at pp. 656—657, &c.

Dr. A. says (p. 942*), those who have been active about the revision of the College charter "have thought proper to make enemies to themselves of the members of the Corporation," (Apothecaries) "which had elected me and my colleagues their professors." Will he please tell us in what year and month he was appointed? and in what previous month and year the apothecaries issued their various circulars against the College, which showed them, as one writer called them, "bitter and avowed enemies;" and yet he has "not united himself with" such (p. 942).

Dr. A. says, in the same page, the College "commenced their hostilities at the beginning of the present session." Why not mention that the apothecaries, whom he has now joined, had carried on hostilities, though principally privately, ever since we foolishly tied ourselves up in our new charter, in 1828; and, of late years, openly? Will he deny that the Apothecaries' "circular," of "9th October, 1837," was published some weeks before the "lecture" he mentions as by the "professor and president of the College," "on the malpractices of apothecaries?" Let all acts of the Hall, and circulars, letters, and advertisements, of late years, by "Mr. Madden" and "C. H. Leet," "secretaries to the Hall," testify that they began the contest, both clandestinely and avowedly.

Dr. A. does not know, or wish to know, as is evident from p. 942*, the meaning of the word "apothecary" in Ireland. Their act of 1791 defines

him, as a person to make up prescriptions of physicians. We never, as I said before, contemplated making up any but our own. That he also does not know, or pretends not, the meaning of "general practitioner," as is evident too, from pp. 942*–943*, for he represents it as meaning an apothecary compounding others' recipes, and at the same time visiting out, attending and prescribing, surgically, medically, and obstetrically, for patients, and making up all his own nostrums besides. We never contemplated undertaking such multifarious work. We mean by "general practitioner" (see p. 733, second column), a man thoroughly educated in surgery, medicine, and midwifery, practising two or three of these branches, and, if he pleases, also dispensing his own prescriptions alone. Hence the folly and falsity of his assertions in first and fifth letters (p. 658 and 942*), that the "members of the College" want to make "apothecaries of themselves!" Not they indeed, but to keep apothecaries such.

Dr. A. (p. 942, second column), conceals altogether that by Irish Apothecaries' act of 1791, no one can practise as apothecary in Ireland, unless examined for apprenticeship, and reexamined afterwards, both times, by the Hall in Dublin; thus excluding all English and Scotch educated apothecaries, however eminent, which is an unfair monopoly.

Dr. A. says (pp. 768 and 769), the article headed "Irish College of Surgeons—Pharmacy Laws," was not sanctioned by "the College," nor "published by its order, and with its sanction." The Committee's Report, as said before, was unanimously adopted on "9th January." This article is a brief but perfect abstract of every thing important in it, prepared by the Committee, on the "10th," and published in all the papers, on the "11th." The petition, too, drawn up in committee, in exact conformity with their report, was afterwards unanimously adopted by the College on "16th January;" and the original appointment of the Committee on "9th November 1837," by a very large meeting, after "notice," and printed summonses to all members, contains these word, "and that, fourthly, they be authorised, &c. and to take all such other measures as may be necessary thereto; and to expend any sum that they may find necessary for carrying these objects into effect, not exceeding in the whole," &c. Now, Mr. Editor, was it not authorised, and were not all our acts so? Let Dr. A. say, was he not present at these meetings?

How can he, then, speak correctly, except in sound, when saying, "The Committee is not the College, and I recognize no right, in any Committee, to make use of the name of the College," which also he knows is done every day, as all its business is done by Committees. He adds, "I demand the production of the minutes of the College, which give sanction to the publication; they exist not." You have them above, and the College approve our proceedings; and to use his ensuing words, "I call again on the College to see to it," viz. his conduct.

Dr. A. says, (pp. 769 and 909*), "The proposed change has not received due consideration from the College or the profession." As to much or little consideration by the profession at large, we have nothing to do, or with his assertion. All our present concern, of course, is with our own College Members and Licentiates. Now I ask him was there not a "Pharmacy Committee" of five appointed on "6th August, 1832," by the College, when 30 Members were present, of whom he was one? Were not he and I appointed on that Committee? Did they not act energetically? Did we not present our "Report" on "23d Nov." to 37 Members present, of whom he was one? Was not its "first clause" discussed on "7th December" by 31 Members, and a division had on the question, whether Members and Licentiates should be allowed to charge for medicines, and carried in the affirmative? Were not its "second and third clauses" discussed on "14th Dec." when 30 attended, and the other discussed on "21st Dec." when 28 were present; and were not the Committee then requested to embody all matters agreed on in a complete Report? Did not that Committee, on which he was, take the opinions of the three most eminent counsel in Ireland, as to the legality of the Apothecaries' proceedings, and of their School? And

was not the resolution of the College of "5th Nov. 1832," (see your p. 1036*) which was "unanimous," grounded on those legal opinions, taken by the Committee of which he and I were Members? Yet he has since joined that school; and of course says, he has no interested object in doing so, and promotes the College honour thereby. At some of the above meetings Dr. A., it appears, was not present, and as he is not old, he himself says, (p. 910*), "I hardly think that you will admit indolence to be a sufficient apology for absence on such an occasion." Were not all those questions deliberated on again in 1833 and 1834? Did not the College receive the final Report of that Pharmacy Committee on "25th January, 1834," and discuss it all on "17th Feb." when 39 Members were present? Was not a second Committee, to carry on the plans, appointed on that day? Did not the "Secretary" of the College, on "4th May, 1835," give a "notice" (which, by the by, the "Lancet" of 1834-5, vol. ii. p. 329, by the dishonest connivance of some Member, published,) similar to that which I gave on "6th Nov. 1837," for the appointment of the third Pharmacy Committee since 1832, to effect this? and was not the mind of the great majority of the College then made up, after several years' consideration, that, without a division, they affirmed the same on "9th Nov. 1837." Dr. A. says, that "from the 9th Nov. when the Committee was appointed, to 8th January, when they agreed to their Report," was "an amazing length of time, doubtless, for so unimportant (important?) a measure." He knows as well as I, that this final Committee was not to consider its advisableness, for that the College had previously decided unanimously; but to prepare and take the most effectual means to accomplish it without delay, for the measure had been constantly discussed since 1832; and he cannot forget, that on one division in the College, on 17th Feb. 1834, some person was in a glorious minority of 1 to 29.

Yet because the other "Member" speaks (p. 819) of "the appointment of a Committee to take measures for its accomplishment," he asserts, (p. 910), "But, sir, the truth, according to his own statement, (for out of his own mouth I'll judge him) is, that the proposed change had not received any consideration at all. For what does he say? "Besides thus totally misrepresenting what the other said, he does not perceive his own nonsense, in thinking that the appointing a Committee to take measures for accomplishing an object, does not imply that the body appointing had already fully made up their minds to do what he says is still to be done, viz. to "decide upon the principle and the propriety of its adoption;" for he says, "but that seems to have been deemed unnecessary in the case, and to have been disregarded." I have proved Dr. A.'s own and the College's discussion of the matter in 1832, 1833, and 1834, and ever since, until it acquired overwhelming majority of numbers, and at last unanimity, at three meetings, viz. to appoint the present Committee, adopt its Report, and sanction the petition as explained before. (See also p. 810.) As to the other "Member's" assertion (p. 819), "that the Report (of the Committee) was unanimously agreed to at one of the largest meetings of the College known for years," Dr. A. has the hardihood to say (p. 910*), "Sir, this is a rash—a mad assertion," and wastes many paragraphs, computing the number of Members in Dublin and the country. It is notorious, that except at the annual election (when also, generally, nothing is discussed), there is scarcely ever so large a meeting as on the above days; and what more wants he than three unanimous decisions at meetings called by previous "notices" and printed summonses to every Member, explaining exact nature of motions? He says, (p. 769), "the College numbers between 130 and 140 Members;" but until compelled by the other "Member," he does not hint, that "there are not more than about 90 Members resident in Dublin" (p. 910*), nor that the "about 30 Members," whom he states as "resident in the country" (p. 770), are a part of "between 130 or 140," who, he leads your readers to suppose, should all have been present, but of the whole of which 130 or 140 he says, "on no occasion has one-third of the number voted upon the question": and (p. 769), "less than a third of the body take

upon themselves to drive on a measure subversive, &c. and in despite of the protest," &c. There neither was, nor is, any protest but his own, for his own object; nor any subversion but of the Apothecaries' schemes. At the election of officers there is always a great attendance, as there is a natural wish to get themselves or friends appointed, and yet this year there were only 55; in 1837, but 45; in 1836, but 58; in 1835 and 1834, when peculiar interest was excited, there were 89 and 79. These two unusually large attendances at elections happening just within five years, explain why Dr. A. (p. 910*) ran back a period of five years, and said, "I demand to know—1st, how many Members composed the College at which the Report was agreed to? 2d, what is the greatest number of Members who have attended a meeting of the College within, we shall say, the last five years? On the two answers to these questions I might rest my cause, they alone would suffice." The above table will prove, as I shall just now show, that the meetings, when Committee was appointed, its Report adopted, and its Petition agreed to, were large, even compared with election days. Yet Dr. A says (p. 769), "There should have been a call of the house; the Members should have been specially summoned, and, if necessary, under fines; if this had been done, there would have been a different result, or I am mistaken." He knows well they were all "specially summoned," that we are not a parliament, and that by charter we cannot be fined unless sufficient Censors and Members are present to make a College; namely, two of former, and six of latter, with President or Vice-President. It is rare to see such large meetings as on those three days, and on all without division and unanimously, as not more than two at the first, and one at the last, expressed any objection; but even they did not divide. Yet Dr. A. objects to his statement of the "proportion" being called an "equivocation," because, he says, the "Member" "does not deny the accuracy of his premises." He did deny, and I disprove them. Were not all Members in Dublin summoned to every meeting by printed notices of intended business? Were not 51 present on the day of the Committee's appointment? Were there not, at the previous meeting, when I gave notice, present 53, of whom 16 were not present on the second day among the above 51? and were not the two succeeding meetings for adoption of Report and Petition also large ones of 37 and 33 (13 also being at the latter who were not at the former), when various others were present, who were not among the 51 or the 16? Now what becomes of his assertion (p. 910*), that the measure "had not received any consideration at all?" Does not the above list, too, prove that at least nearly 70, and, perhaps, nearly 80, out of about 90 or 100 in Dublin, considered it at different times, even within a few months? and that all had the opportunity?

In (p. 910*) Dr. A. misprints a quotation from the other "Member's" letter, thus—"No. 4. The proportion of the communications from the country favourable to the change does not exceed a third of the entire," whereas the other had quoted Dr. A.'s words in p. 770 correctly, by printing it (p. 820) "those" (communications from Members and Licentiates resident in the country) "favourable," &c. to show that he joined different parts of Dr. A.'s whole sentence in p. 770. Dr. A. then adds, "I demand that the falsehood of this statement shall be proved, by the production of the letters." Does he mean, we should publish all in your Journal? Did he not hear some, could he not have heard all, read at one open Committee? Does he not say, he knows them so well as to assert (p. 770), "some adverse, the majority favourable?" and yet even here to contradict himself, by saying also (p 770), "the proportion of those favourable to the change does not exceed a third of the entire." He who scruples not unfounded statements ought to try to be consistent in his story, and have a better memory than to contradict himself.

Though the whole discussion between the two "Members" was about opinions of our own College's Licentiates and Members, Dr. A. attempts to prove his assertion by saying (p. 910*), "the profession in Cork have declared themselves

averse to interfering with the province of the Apotheearies," which is true in our meaning of the word "Apothecary," but not in Dr. A.'s. But even if true in every sense, it would not concern the question, as the meeting there was of Graduates of all Colleges, and only few of ours attended. To use Dr. A.'s words (p. 942*), "but it is not true, emphatically not," that they oppose us, for they passed this 2d resolution, (see Cork Standard, 26th January 1838), "That it is expedient that the relative position and duties of the several branches of the profession be positively defined by the Legislature, so as to be equally conducive to the interests of all; and condemning, as we do, that several of the provisions of the bill understood to be proposed by the Royal C. of S. for regulating the practice of Pharmacy in this country, are well calculated for placing the medical profession on a respectable foundation; we at the same time deprecate demanding of the Legislature any penal enactments controlling society in the selection of professional services." This, the sole resolution passed on the subject, approves every part of the proposed bill, except that no man be allowed to practise who determines to make up others' prescriptions also. And all the speeches in that paper praise our plan, except that one or two expressed doubts as to the encouraging English and Scotch Apotheearies to settle in Ireland (see p. 668). As to Sligo, he similarly misrepresents; for our Licentiates sent us their published resolutions (commenting on some violent resolutions of Sligo Apotheearies) as their answer to our circular, and these sanction all the main parts of the plan. And even Dr. A.'s quotations from some prove the reverse of what he says, for they express exactly our views, namely, that we should prefer the revival and enforcement of the old plan of Apothecaries staying at home minding their shops, and making up our prescriptions; but that as we see no chance of that, we, or they, in Dr. A.'s quotation, "anticipate," and yet "would lament, the necessity which would require" us and "them to make up medicines for their (our) patients." (See Sligo Champion, or Sligo News, 27th January, 1838.) But Dr. A. carefully suppresses another resolution (see same paper), as follows: "We merely express a wish that the Apotheearies of Ireland may bring in such a bill as may place their profession on its ancient scale of respectability, as we cannot but look upon the Apotheeary who conscientiously attends to the dispensation of genuine and good medicines as a very valuable member of society." Thus clearly stating, they prefer the old plan of wholly non-prescribing Apotheearies who make up others' prescriptions; and implying, also, that present Apothecaries often do not attend to their business, nor make up receipes correctly. Besides, even the quotation Dr. A. gives, (p. 910*) is only part of our Sligo Licentiates' resolution, in answer to the 3d by the Sligo Apotheearies, which (Sligo Journal, 16th January, 1838) impudently orders, that "a petition also be forwarded to both houses of parliament, praying that the Licentiates of the Irish C. of S. may be restrained from meddling with the legitimate province of the Physician; and that they confine themselves to the treatment of purely surgical diseases."

Dr. A. says (p. 942*), "every Graduate in medicine, or Licentiate in surgery, who has taken the license of the Apotheearies' Hall, Dublin, is by law entitled to practise, both as Apothecary and Physician, or Surgeon, throughout Ireland, except in Dublin, or within a few miles thereof." He should have said, "every licensed Apothecary, examined after a seven years' apprenticeship in a shop, who studies for, and takes also a degree in medicine or surgery." He adds, "it is notorious, that Ireland abounds with men who practise in this double capacity;" but he omits, that these Apothecaries, in 90 cases out of 100, do so without any medical or surgical education or degree whatever, as proved by their own "curriculum" of 9th October, 1837," and their "authorised" list of "Licentiate Apothecaries," (in Pettigrew and Oulton's Almanack for 1838, pp. 537-543) by which it appears that very few have any medical or surgical diploma. He says, "In the North the practitioners are chiefly of that class;" but conceals that these are mostly persons educated and examined in sur-

gery, medicine, and pharmacy, at Edinburgh or Glasgow, most of whom practise as Apothecaries in defiance of the Hall and their vile monopoly (by 22d section of act of 1791), which makes it illegal, and even penal, to "open shop or act in the art and mystery of an Apothecary within Ireland," unless first examined, to be apprenticed to themselves, and re-examined afterwards at the Hall, in Dublin, Dr. A. says (p. 942*), that "Apothecaries" have acted in the double capacity" as "Apothecary and Physician, or Surgeon," also, according to the Licentiates of Dublin College of Surgeons resident in Sligo, "from time immemorial." Not to stop to say, that if the Apothecaries did so, it was and is illegal, this sentence, "from time immemorial," does not occur in the Sligo Licentiate Surgeons' resolutions (Sligo Champion, or Sligo News, 26th Jan. 1838), but appeared in 4th resolution of Cork Apothecaries, and in 3rd of Galway Apothecaries. (See Cork Constitution, 20th Dec. 1837; or Dublin Evening Mail, 5th Jan. 1838; and Galway Patriot, 17th Jan. 1838.) The Cork Apothecaries' 4th resolution is, "From time immemorial the Apothecaries of these countries have been regarded and employed by the public in general as medical attendants in ordinary cases," &c. &c. The Galway Apothecaries' 3rd, "From time immemorial the Irish Apothecary has been employed in ordinary cases of illness by the largest portion of the inhabitants of this country," &c. &c. What the Sligo surgeons say is in ridicule only, "In reference to the 3rd (Sligo Apothecaries') resolution, how can we look upon it without at once exposing its absurdities? The Apothecaries of Ireland claim to be medical and surgical Practitioners, as well as Apothecaries; and yet, &c." and they then go on to say, that what the Apothecaries claim to do, and to have done, "time out of mind," how can they consistently seek to restrict others from doing? So that Dr. A.'s syllogism is this:—

1. Every Apothecary pretends, or has long pretended, to be also a Physician and Surgeon.

2. The Cork, Galway, and Sligo Licentiate Apothecaries claim to have done so "from time immemorial."

3. Therefore, without falsehood, I may say, the Sligo Surgeons acknowledge they have been, are, and ought to be so; and I may also put the words of one into the other's mouth."

Dr. A. says (pp. 942*, 943*), he "cannot even imagine" what the Member's "authority may be" for asserting that "the apothecaries have boldly put themselves forward, not merely as general practitioners, but the sole licensers and instructors of that class of the profession," namely, as he says, those who practise medicine, surgery, and pharmacy. Has he read the "important notice" of "27th October, 1837," which states that there is "no such special or suitable means of education provided elsewhere," but only at their "Hall?" Has he read their "secretary Wm. Madden's" many advertisements; for example, that "by order" of "21st September 1832," which says, "We, the governor, deputy-governor, and directors of Apothecaries' Hall, Dublin, have this day duly and carefully examined Mr. ....., of ....., in anatomy, physiology, theory and practice of physic, chemistry, pharmacy, materia medica, and botany, and think him properly qualified to commence the profession of an apothecary?" Does not all this farrago of pretended examination mean he is to be "physician?" and have I not proved the same as to "surgeon" too, as well as apothecary; in fact, a "pharmaco-medico-chirurgus," or "general medical practitioner," and "surgeon-apothecary?" Has Dr. A. not read late advertisements of apothecaries of Cork, Limerick, Sligo, Carlow, Galway, Belfast, King's County, in our minute-book (privately examined), which are too long to quote, but which declare themselves alone competent to act as general practitioners, and that surgeons and physicians are not, and cannot be? If not, I can produce them. And will he deny that the above and other advertisements, circulars, and letters of Mr. Madden, as secretary of the Hall, were the cause why, on the subsequent "5th November 1832," the College unanimously declared, when 41 members were present, of whom Dr. A. was one, on the opinion of the three most eminent counsel in Dublin (taken by Committee, of which he was one) that their whole school,

with professorships' certificates, pretended diplomas and all, illegal and inadmissible. This he wholly conceals.

Dr. A. represents a proposition by the voluntary "society of apothecaries" (in their "Report on the present state of the Medical Profession, with suggestions for its future legislation; published by order" last winter) that general practitioners should be licensed by a Board of physicians, surgeons, and apothecaries, as the act of the "body of apothecaries;" and thus conceals that it is not the recommendation of the incorporated Apothecaries' Hall.

In the following sentence Dr. A. mixes up, and thus confounds, five plain things: first, the Committee's unanimous appointment, on 9th November 1837, to take measures to effect the College object; second, its report of 6th January, how best to do so, unanimously approved by the College on the 9th January; third, the brief abstract of its contents published by the Committee on the 11th, in the papers; fourth, the petition to Parliament founded on that report, prepared by Committee on 14th, and unanimously adopted, too, on 16th; and, fifth, the intended future act of parliament. He says of the 3d and 4th (p. 911*), "Now, sir, the published statement contains but six propositions, the petition contains seven." If he listened to, or read the petition, he could not say seven, as it contains many more paragraphs; but he conceals from his readers, that the 3d, or abstract as to "pharmacy laws," on 11th January, was an abstract of the 2d, or report of the 6th and 9th, and of course not of the 4th, or petition, at all, which was not presented to the College until 16th January, nor even prepared until the 14th. But, he says, six propositions only are published, and one suppressed. Had he taken the trouble to look a second time, he would see that though the published abstract (pp. 667, 668) says, 1st, 2d, 3d "provisions," its divisions (which are quite different things) do not correspond exactly either with report or petition, and yet express all the contents of the report fully and truly; though the abstract has only five heads actually reckoned up, and the report seven, before each proceeds to supplementary matter; for the seventh is, in fact, included in the first five, and only a recapitulation of their object, as a total; and, besides, the 7th section in the petition does not at all coincide with the 7th in the Report, being about a different matter. But Dr. A, says (p. 768), "The other provisions which it is intended that the act of parliament shall contain have not been made public; that however there are others of a most important character, I can positively state, and which require the most serious consideration." Here he shifts his ground to an intended act. I have shown that the abstract was of a report, not of an act of parliament, no part of which is yet ready, nor even of the petition (for some such act), which was not at the time prepared by the Committee; but he says, "I demand the seventh proposition; I demand the answer. The published statement contains it not." I have shown that it could not; but as Dr. A. was present at presentation of the report and petition, and has shown little respect to etiquette or propriety, as to publishing even incorrectly College documents, why did he not publish the seventh proposition of both himself, to prove his point. In the report it is this, and let any honest man read the article "Pharmacy Laws," and see if the same be not included in that abstract of the whole report "7th. To assert and defend the rights conferred on the College by its charter, and to sustain the power given it, to enforce a proper system of surgical education in Ireland; disregarding the unwarrantable pretensions of a body, created to discharge duties necessarily subordinate, to meddle with matters foreign to their pursuits and habits, and of which they must consequently be entirely ignorant." This is the 7th paragraph of the report of which the article "Pharmacy Laws" was an abstract. The 7th of the petition, prepared some days after it was published, is as follows (and as this petition was not presented, until the other day, to the House, it would have been contrary to parliamentary etiquette to have published it, or any part of it, before presentation, after which it becomes of course a public document open to all):—"And, 7thly, To compel the Apothecaries' Company to examine every person who shall have enjoyed reasonable and

proper opportunities of acquiring a knowledge of the legitimate business of an apothecary; and, if qualified, to grant him a certificate to that effect; and at the same time to restrain them from examining such persons on any other subjects than chemistry, botany, and the art of preparing and compounding medicine, or demanding certificates of attendance on any other lectures than those on such subjects."

And now, sir, let your readers judge if the "Member's" contradiction of Dr. A.'s statement of something material being suppressed, was not correct, when he said (p. 819*), "No. 2. All the provisions which it is intended that the act of parliament should contain have not been made public." "This is altogether false, as can be proved by the minutes of the Committee, to which every member has free access;" and, again, when he contradicted Dr. A.'s malicious statement (p. 770), "No. 6. They appear to me to be only a mask for another as yet not avowed design," by saying, "I can only, on the part of the College, the Committee, and myself, flatly contradict this statement." Dr. A. (p. 909*) says, "No. 2. I refer to No. 6," and on p. 911* he prefixes accordingly "No. 2" to "No. 6," but he ingeniously omits the "Member's" answer to "No. 2" and quotes only his answer to "No. 6," as if intended for both; and then says, that this "method" of answering and "proving false his assertions," is calculated to raise a "smile," and requires the "Member" to learn the "meaning of language." Sir, the "Member's" meaning is very clear; namely, to deny the facts which Dr. A. said existed, and made it appear so to him. And to say it does not appear so to Dr. A. because it could not to any mind knowing the facts at issue; and to add, that Dr. A. had means to know them, and therefore his statement as to supposed fact and pretended appearance is "No. 6." But Dr. A. says (p. 911*), "Is it, or is it not, a part of the instruction to the Committee, that the College shall be empowered to educate and qualify a general practitioner?" The published statement "Pharmacy Laws" contains it in express terms; but Dr. A. is so blinded by the false definition of "general practitioner," in his own mind, as meaning an "apothecary," who, either with or without medical or surgical degree, with or without education, with or without law or usage, tries to practise all in the ring—medicine, surgery, and midwifery, and make up his own and others' recipes, that he cannot understand the publication—"Pharmacy Laws." Why, the very object of the Committee's appointment by the College, the very essence of their report to it, the very quintessence of that published abstract, the very prayer of the petition adopted by the College, the very gist of the intended act of parliament solicited by it, is to make all our members general practitioners in every sense of the words, if they wish; and, of course, to enable the College to educate and license all its future apprentices and pupils as such. But Dr. A., it seems, "cannot see the wood for trees."

He says (p. 769), "the College of Surgeons have no right, authority, or sanction, to take upon itself to regulate, or prepare laws for the regulation of the practice of pharmacy." This is mere nonsense, as the College, and its members, and every individual in the state, has a right to propose laws to parliament to regulate every thing in the kingdom, if they appear ill regulated; and parliament, of course, is the only body that can adopt, make, or confirm them.

We know that the "Apothecaries" have not regulated their own profession well, and have not only made themselves unfit to serve us, but attempted, without law, to infringe upon ours. Did their mismanagement merely injure themselves, we would not interfere, but it deprives us of pure apothecaries to compound our prescriptions. We therefore require a revision of the law and practice. Has the "Society of Apothecaries" the same excuse for interfering with the "medical profession" by its "report on its state," "with suggestions for future legislation." Besides, Dr. A. knows the title and object of our Committee to be for "revising our own charter," &c. as before quoted. And I suppose he will allow we have "right, authority, and sanction," to do that, and to regulate our own concerns, and to propose correction of all collateral evils. This cannot be done without compelling others to cease from intrusive usurpation, assumption

and interference, and to mind their own business.

And now, I am neither the "Member" who answered two of Dr. A.'s letters before, whom he first called "Professor in the School of the College," and latterly "Dr. Maunsell," nor the "reserve champion of the party" of his, p. 911*, nor "the third of the Curiatii" whom he says (p. 943*), he expected to see next. By the way, let Dr. A. remember, that the third of the three Horatii (whom he would compare, I suppose, to the "Hall," "the Society of Apothecaries," and last himself), was condemned to ignominy and death, for murdering his own sister, by his own associates, whom we may compare to the College members, as he calls it the "sister institution" of the "Apothecaries' Company."

I cannot pretend to such quadruple slaughter of Curatii and betrothed sister as he, but as Solomon says, "Let not him that putteth on his armour, boast, as he that putteth it off." I am neither Horatius nor Curiatus, but simply "the Graduate of three Colleges," as he terms me, in a foolish attempt at ridicule, because I am (as signed in all my letters in the Dublin papers, and in my answer to his first letter as "Professor in Apothecaries'-Hall School,") "a graduate in medicine, surgery and midwifery, of Dublin, Edinburgh, and London."

I have the honour to subscribe myself,

Your obedient humble servant,

CHAS. EDW. HERBERT ORPEN,
M.D. EDIN.,

Member of the Royal Colleges of Surgeons of Ireland and in London, &c.

11, North Great George's Street,
Dublin, April 7, 1838.

DR. ALCOCK'S REPLY TO DR. MAUNSELL.

To the Editor of the Medical Gazette.

SIR,

I HAVE read, in the last number of your GAZETTE, a letter from Dr. Maunsell, in which he declines to fulfil the pledge which he gave to you in his letter of the 17th of February, though on the strength of such assurance alone I must presume that you consented to publish such a letter.

When replying to that letter, I had occasion to express my surprise at "the total abandonment of temper and courtesy displayed" therein. I have now to express equal surprise, but mingled with a very different feeling, that the man who then so vauntingly, so offensively, gave a pledge to substantiate his statements, has now shrunk so meanly from its redemption.

The gentleman says, "that having, *as he thought*, done his duty toward the body to which he has the honour to belong, by taking measures to expose the author of a letter calumniating that body, he did not conceive that he had any further *personal* concern in the matter." Sir, I have had occasion already to point out the ignorance of the meaning of language (whether used by himself or others) displayed in his first letter; I have now to expose the less excusable ignorance which is manifested in his last, of the responsibility entailed on him by his own conduct and relative position. He tells you, that having on his own judgment taken certain measures for a certain purpose, that he did not conceive "he had any further *personal* concern in the matter." Sir, it is he who acts upon his own judgment that *is* further *personally* responsible, as well to those toward whom he acts, as to those in whose name he takes upon himself to do so. His act on that occasion was an assumption; it was done without authority from the body in whose name he presumed to speak; for, besides his own admission, at the time he wrote and published his letter of the 17th February, he had no authority from the College. It has been stated by certain members of the Pharmacy Committee, that at that time he was sent to London by that Committee: if so, he was its agent, and not that of the College; and if sent by the Committee, he was sent secretly, for the only evidence of his mission to be found in the proceedings of the Committee is an order (to that effect), *interlined and in a strange hand*, in the report of their proceedings as engrossed by the registrar; and the explanation of this extraordinary fact, given by their secretary, is, that the insertion of the order was delayed, that members of the College adverse to the views of the Committee

might be kept in ignorance of it. On Monday, March 5th, and not until then (his letter having been published Feb. 17th, and he himself having returned to Dublin in the interim), his friends obtained from the College authority for him to return to London as its agent. This authority was obtained before it was avowed that he was the author of the letter; and the motive for seeking it does not require explanation. It may be that he now displays the authority; and if so, its date will verify my statement.

He has imposed upon me the necessity of now stating the position in which the proceeding stands, which has been taken in the College regarding myself, and the letters of which I am the author. He has said, that "it is still, he believes, under the consideration of the proper tribunal," but he has omitted to explain what that tribunal is. The question has been for some time referred to the consideration of counsel. The gentleman and his friends forced the College into a course which it was neither called on to adopt, nor, as I conceived, empowered to pursue; and having thereby committed it, they were compelled, at the twelfth hour, and when they could with safety proceed no further, to retrace their steps, and to do what ought to have been done in the first instance, viz. to submit the matter for the opinion of counsel; and so it remains. This certainly does not for the present add to the dignity of the College; but it will do so, for it will teach a certain party in the College a lesson which they much required. Meantime, let those who may be disposed to complain, attribute the result to their conduct, and not to mine. Had I pursued any other course than that which I adopted, or had I complied with the demands of that party, there would have been an end to the very semblance of independence in the body.

The gentleman's apology for leaving his pledge unredeemed is, "that it is totally impossible for him to enter into *any controversy* with a gentleman whose views of the obligations of honourable men with regard to truth differ so remarkably from his." This may be either a compliment or it may not. I will not suppose that he would take advantage of, or presume upon, the intimation contained in my letter of the 3d March, that he must maintain his charges by other means than the "duello;" and as I find that the evidence upon which he has founded his opinion of our comparative regard for truth is altogether unfounded and untrue, I cannot regret if his views and mine differ altogether upon so very important a point. The evidence upon which he relies is, that in my letter, signed "A Member of the College of Surgeons in Ireland," and published 10th February, I have said, "Hitherto I have looked on, because it did not appear to me that the time for seasonable or effectual interference had arrived;" and that I have since acknowledged myself the author of a letter "*upon the same subject*," signed, "A Professor in the School of the Apothecaries' Hall," &c., and published on the 20th January. This statement contains two assertions, which are utterly devoid of truth:—

1. It is not true that I acknowledged myself the author of a previous letter "*upon the same subject.*"

2. It is not true that the previous letter, of which I did acknowledge myself the author, and signed "A Professor in the School of the Apothecaries' Hall," is *upon the same subject* with my letter of the 10th February. On the contrary, the latter is expressly written upon "the change proposed to be made in the character and functions of the College of Surgeons," and upon the "Pharmacy Laws," proposed in an anonymous statement published in the newspapers, and in the same number of the Medical Gazette which contained my letter signed "A Professor," &c.; whereas the letter so signed is as expressly written for the purpose of repelling an injurious, unfounded, and unprovoked imputation cast upon myself and my colleagues, in a letter published in the *public prints*, by Dr. Orpen, the secretary of the Pharmacy Committee, and circulated throughout the kingdom by that Committee, of which Dr. Maunsell is a member, at the expense of the College, of the very body whose members are therein maligned and calumniated. These facts have all been since admitted in the College, by the parties themselves; yet are they allowed to go unpunished, and are still entrusted with the guardianship of the interests and the reputation of the College and the profession. Now, sir, in proof of the intention of my letter of the 20th January, I take the following extracts from its preamble:—"The main desire of the writer of this letter appears to be two-

fold." "With this design I feel myself no further concerned than the other members of medical and surgical professions at large; *nor should I have thought it necessary to notice the letter, had its author been content to confine himself to the discussion of the question at issue between him and the apothecaries.*" "In the course of his letter, however, he has thought fit to arraign the conduct of those persons who have become professors in the school of the Apothecaries' Hall, and to charge them with perjury." "You will, therefore, I doubt not, admit the validity of the motives which induce *me to undertake the exculpation of the Professors of the Hall School.*" Further, sir, my letter of the 20th January was written before the proposed Pharmacy Laws were published, or had come to my knowledge. They were first submitted to the College on the 9th January, and my letter is dated, and was dispatched (to the best of my recollection), on the same day; and the anonymous statement of the laws, to which my letter of the 10th February had express reference, was not published for days afterwards. Finally, my letter of the 20th January contained no discussion of the pharmacy laws, or of the pharmacy question, nor any reference thereto, beyond a taunting allusion to the well-known intention of my adversaries upon the subject; and I repeated my purpose in the conclusion, thus—"Having, I trust, satifactorily shown that the honour of their professions and Colleges has been in no wise affected by the conduct of those physicians and surgeons who have become professors in the School of the Apothecaries' Hall, I postpone till my next, &c." May I not now with confidence ask, who but the gentleman could imagine that the two letters were "*upon the same subject?*"

But, sir, even though I had, whether from dissimulation or oversight, fallen into the discrepancy which the gentleman has charged upon me, what then? How does that exonerate him? Sir, he is altogether in error. In the first place, my views and my conduct would not justify his; in the second, he is not *asked* to *enter into any controversy* with me. To you and to the profession he gave the pledge, and with you he must redeem it, or be prepared to stand a self-convicted slanderer.

Sir, you can now estimate at its real value the pretext under which Dr. Maunsell attempts to escape from the redemption of his pledge; and as he has chosen the alternative of sheltering himself under the protection of his College-party, I leave him to your judgment and to that of the profession, only adding, that, according to my code, "honourable men" are bound, when they give pledges, either to fulfil them or to acknowledge their error.

I have the honour, for the present, to remain

Your obedient servant,
BENJAMIN ALCOCK.

Dublin, April 15, 1833.

ROYAL COLLEGE OF SURGEONS IN IRELAND.

To the Editor of the Medical Gazette.

SIR,

I AM directed by the Pharmacy Committee to request you will be so good as to insert the inclosed in the MEDICAL GAZETTE.

I am, sir,
Your very obedient servant,
C. O'KEEFE,
Registrar.

Royal College of Surgeons in Ireland,
6th April, 1833.

AT a meeting of the Pharmacy Committee appointed by the College,

THE PRESIDENT, W. H. PORTER, ESQ. IN THE CHAIR,—

The following resolutions were unanimously adopted:—

That this committee fully concurs in the contradiction published by Dr. Maunsell in the MEDICAL GAZETTE of the 17th of February, respecting the statements contained in an anonymous letter published in that journal on the 10th of the same month, since ascertained to have been written by Dr. Benjamin Alcock.

That Dr. Alcock, while he concealed his name, having used language offensive to this committee, and published unfounded statements respecting its proceedings, and not having since adopted any course calculated to remove the impression made by these communications, it is the opinion of this committee that Dr. Maunsell should not attend to any communication from him, or afford any explanation at his suggestion.

(Signed) J. W. CUSACK,
Secretary.

Royal College of Surgeons in Ireland,
5th April, 1833.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, APRIL 28, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton-Street, near St George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XVIII.

Diseases of the Pleura (concluded)—Hydrothorax: its causes—Signs and Treatment—Pneumothorax—From Adhesions—Spontaneous—From perforation of the Pleura—Signs and Symptoms—Metallic Tinkling, &c.—Treatment—Altered Sensibility of the Pleura—Neuralgia Pectoris—Pleurodynia—Treatment of Nervous Pains of the Chest—Tumors in the Pleural Sac—Signs.

We shall find no difficulty in comprehending within the present lecture all that needs to be said of other affections of the pleura, not immediately connected with inflammation.

The serous secretion of the pleura may be increased by various circumstances affecting the circulation within the chest, such as organic diseases of the heart and great vessels, tumors at the root of the lungs, especially when these circumstances much interfere with the functions of the organs, as towards their fatal termination. But idiopathic *hydrothorax* is a rare disease; and all that you have heard about water in the chest causing orthopnœa, starting from sleep, lividity of face, &c., belongs rather to the organic disease of the heart than to the liquid effusion, which is often absent with these symptoms, and as we have seen, is present to a great extent in latent pleurisy without producing these symptoms. Hydrothorax is sometimes produced in general dropsical states resulting from febrile action, from a cachectic state of the system, or from diseased kidneys; and when extensive, becomes, together with œdema of the lungs, the cause of oppression and danger in such cases. Slighter degrees of it very commonly take place shortly before the fatal termination of many diseases not essentially connected with the chest, and are probably the consequence of the congestion of blood caused by the gradual failure of the circulation and respiration. In all these cases the effusion may affect both sides; but it is generally more abundant on one side. So also in hydrothorax connected with organic disease in the chest, there is commonly more fluid on one side, and that not always the most diseased. I have frequently remarked that the effusion accompanying organic disease of the heart is most abundant on the right side; this is also often the case when the pericardium is adherent to the heart. The liquid of hydrothorax is simply serum; but in other cases in which great congestion of the thoracic vessels has been its cause, we may often see in it a bloody tinge, or an approach to an inflammatory product, in a few slight loose films of lymph—proofs that the overflow extended also to the nutritive secretion of the membrane, in consequence, probably, of the temporary struggles of reaction which the vascular system frequently displays during the sinking of the vital powers. They resemble the congestive peripneumonies of the dying, which we shall soon have to notice, and should not be confounded with the products of real inflammation.

Now you can readily perceive that the

physical signs of hydrothorax are the same as those of the effusion in pleurisy. There will be the dulness on percussion, and diminution of the respiratory murmur in the dependent parts of the chest, and afterwards you may have ægophony in the middle regions; but as the effusion is seldom so extensive in hydrothorax as in pleurisy, or so much confined to one side, you do not get that abolition of the sound on percussion, and of the respiration and voice, or the displacements of organs, or the puerile respiration on the opposite side, which occur in the latter disease. The general symptoms, exhibiting a dropsical tendency, with effusions in other parts, together with the signs of disease of the heart or other organ affecting the circulation, and an absence of the general and local signs of inflammation, may generally enable you to distinguish the effusion of hydrothorax from that produced by an inflamed pleura.

The treatment of hydrothorax will vary a good deal with the nature of its cause—from what organic lesion or what constitutional condition it proceeds. Thus, that from disease of the heart will require one set of remedies; that from diseased kidneys another: the former being most frequently relieved by diuretics and hydragogue purgatives, the latter by mercurial aperients and diaphoretics. Again, that connected with a febrile condition of the system, as after scarlatina, may often be removed by an antiphlogistic plan; whilst that arising from a cachectic condition will receive most relief from tonics. To enter into the details of these different lines of treatment would require more time than we can spare; for we have superabundance of matter for the remaining part of the course more immediately connected with diseases of the chest.

Pneumothorax, or air in the pleura, may be produced in three different ways. 1. It may be the consequence of a partial pleurisy. We have before seen that after a pleuritic effusion has long compressed the lung, and the compression has been perpetuated by a rigid false membrane which has been formed over it, the absorption of the liquid leaves a void, which the collapse or contraction of the walls of the chest is in some cases insufficient to obliterate, and this void is sometimes filled with air secreted by the membranes. I have seen two instances of a partial pneumothorax apparently produced in this way. They each occupied about half of the pleural sac—one the upper, the other the lower half; and the lung in both cases was strongly bound down by fibro-cartilaginous membrane, and condensed in the part contiguous to the empty space. There was also some contraction of the chest in both cases. This kind of pneumothorax is very rare; and I do not think that it is described by any author.

2. Another kind of pneumothorax is that which may be called idiopathic, and arises from an effusion or secretion of air into the sac of the pleura without perforation. This is also of very rare occurrence. It is said to be sometimes met with towards the termination of fatal diseases, in the same manner as tympanitis occasionally occupies the peritoneal sac under similar circumstances. I cannot say that I have ever met with a case in which I discovered any signs of such a form of pneumothorax during life; although I have several times seen air in the pleural sac on opening bodies after death, where no perforation could be discovered in the pleura. It is possible that the air may have been exhaled from the animal fluids after death, and then increased by exosmosis through the lung. The facility with which air pervades dead membranes countenances such a notion. Pneumothorax is also said, by Dr. Graves and others, to have occurred in a few instances at the commencement of pneumonia, and to have soon afterwards disappeared; but as the chief sign in these cases was a remarkable resonance on percussion, I suspect that these were examples of the production of tracheal or amphoric sound on percussion, from consolidation of the upper lobe of the lungs, and not cases of pneumothorax.

3. By far the most common kind of pneumothorax is that caused by some unnatural communication between the pleural sac and the external air; and this may be by a perforation either of the external parietes or of the pulmonary pleura. The latter case is the now usually recognized cause of pneumothorax, and constitutes the great bulk of the examples that are met with. The perforation depends on the progress of ulceration, generally of a tuberculous character, rarely of gangrenous abscess, through the pleura. The circumstance of ulceration reaching and perforating the pleura, indicates a low state of the reparative powers, and a want of plasticity in the products of inflammation; for under ordinary circumstances, ulceration could not approach the pleura without causing it to inflame and throw out coagulable lymph, which, becoming organized, forms either a protecting thickness of membrane, or close adhesions to the costal pleura. We see this in most cases of chronic phthisis, where the upper lobes are generally adherent to the ribs. I have seen an ulceration extend from a tuberculous cavern across the two layers of the pleura, thickened and adherent, and completely through the walls of the chest;

so that when the patient coughed, air bubbled out of two or three fistulous openings in the front of the chest: but here was no pneumothorax. On the other hand, I have met with more than one case in which the adhesive process seemed quite incapable of protecting the pleura, which was consequently perforated in several points; wherever, in fact, the ulceration of the lung extended to it, and the air freely entered the sac of the pleura by all these holes. More commonly, however, there is only one perforation, and this is generally near the apex of the lung, in connexion with some of the cavities that first form there.

The completion of the perforation is in most instances sudden; a part thinned by ulceration, and imperfectly adherent, giving way during a fit of coughing, or some other unusually forcible act of respiration. The immediate effect is to admit air more or less rapidly into the pleural sac, which, by equalizing the atmospheric pressure outside and inside the lung, permits it to assume that state of collapse to which its natural elastic contractility would reduce it. Hence dyspnœa, sudden and severe, in proportion to the extent to which the air enters and the lung becomes collapsed. But the access of atmospheric air to a serous membrane totally unaccustomed to it, also occasions great irritation and consequent inflammation. Hence a sudden sharp pain and dry cough, with spasms of the intercostal muscles, and a weak, quick, and sometimes irregular pulse. Soon the irritation becomes accompanied with inflammation, and then follow the symptoms of acute pleurisy, with heat of skin and inflammatory pulse; and a liquid effusion is added to the air in the pleural sac.

Now, although a perforation of the pleura will not fail to introduce air into the pleura, the amount and effect of the introduction of this air will vary considerably, according to the size and other circumstances of the ulcerated aperture. If this be very small, or if, as it not unfrequently happens, it be so placed that the walls of the chest close it in expiration, by which it is rendered valvular; or if it be below the level of the liquid, the air introduced by each inspiration will not escape as freely in expiration, and the result will be progressive accumulation of air in the pleura, and a consequent increase of compression of the lung and of the dyspnœa; and in this way perforation of the lung has, in some cases, caused suffocation within a few hours of its occurrence; in others, this catastrophe has been delayed by the vent given to the air by accidental changes of position, or the effect of violent cough, or by puncturing the chest. If the aperture be of larger size, and no impediment occur to the passage of air through it, it will interfere with respiration only so far as it suffers air to pass outside of, instead of into, the lung. But when the air passes thus freely, the pleura is more irritated by it, and there is a more copious secretion of liquid, which is generally purulent, and often fœtid. In either of these cases, after the subsidence of the spasm first caused by the entry of the atmospheric air, there are no particular symptoms which can serve to distinguish pneumothorax, especially of either of the first kinds. The occurrence of perforation is sometimes denoted by the sudden acute pain of the side, and embarrassment of breathing; which the patient, in some instances, refers to something giving way during a fit of coughing; but such sudden attacks sometimes take place from pleurisy, without perforation; and I have repeatedly known perforation to occur without being followed by any remarkable increase of pain or distress.

The physical signs of pneumothorax are generally very remarkable and distinctive. The presence of air in the pleura will give to the walls of the chest a freedom of vibration, and therefore a degree of resonance on percussion, even greater than that which the air-filled structure of the lung confers on them; so that percussion will give more of the drum-like note or tone which you get by striking over the region of the stomach or of the cæcum. This will be more marked in proportion as the quantity of air is considerable. The same circumstance will also impair or destroy the sound of respiration; for the air not only removes to a distance the pulmonary structure where this sound is produced, but also by its pressure diminishes that entry of air into the cells on which the sound depends. There will be, therefore, this remarkable contrast of signs to distinguish pneumothorax—a good or hollow sound on percussion, with little or no sound of respiration, whilst the opposite side gives a worse sound on percussion, but a much more distinct respiratory murmur.

But there is another class of sounds produced in air-filled cavities, which may often give decisive evidence of their existence. Apply the mouth of this India-rubber bottle to your ear, and observe the sound which striking on its outside causes. It is a little tinkling note, like the clink of a piece of metal or glass. This note is an echo produced by the reverberations or repeated reflections of the impulse from the walls of the cavity, and it is shrill and acute, because the reflections are short and quick, in so confined a space. It is the same sort of note that you hear on

listening to the mouth of an empty cask, but it is there less shrill, because the space is larger. Any sound proceeding from, or communicated to, the cask, the India-rubber bottle, or any cavity in the body with reflecting walls, will be accompanied or followed by this sort of tinkling or ringing echo, which will be more prolonged and distinct in proportion as the walls are perfectly and uniformly reflecting. You may often hear sounds of this kind, on using the stethoscope over the stomach or large intestines, as their contents move and cause a sound within them. So, too, you may hear the tinkling echo accompanying the sounds proceeding from an air-filled cavity in the chest, and it becomes a distinctive sign of the existence of such a cavity.

In idiopathic pneumothorax, and in that partial kind resulting from the absorption of a partial pleuritic effusion, although the cavity be present, there may be no sound produced in it, or transmitted to it, so as to cause the tinkling echo. Sometimes, percussion on the external walls will do this; but care must be taken not to strike too near the spot where the stethoscope is applied, or the clink sound produced by abrupt impulses on the tympanum may be mistaken for the tinkling echo. Sometimes the voice or cough may be transmitted to the cavity by the condensed tissue of the lung. I have heard the metallic tinkling accompany both of these in a case of partial pneumothorax, in which there was neither liquid effusion, nor perforation of the pleura. But it is where liquid is present, and where the pleura is perforated, that you will most commonly hear the phenomenon of metallic tinkling; not, as Laennec supposed, because these are essential to its production, but because the motions of the liquid, or of the air through the orifice, make sounds within the cavity, which serve to shew its echoing properties. So metallic tinkling has often been heard after the operation for empyema; shewing the presence of air in the pleura, which I have before told you is a very undesirable result of the operation.

Perforation of the pleura, with its consequence, pneumothorax and liquid effusion, is not a very uncommon accident in the course of phthisis, and its physical signs are so remarkable that they can scarcely fail to be recognized even by those who are but moderately versed in auscultation. The tinkling reverberation may present several modifications, which it is useful to notice, as they may serve to give a more accurate knowledge of the condition of the parts, and of their tendencies. When the perforation is small, or obstructed by its position against the walls of the chest, or below the level of the liquid, the tinkling is seldom heard except on coughing or taking a full inspiration, which reaches the cavity, and may throw the liquid into bubbles. The voice may also sometimes find access to the cavity through consolidated portions of lung, and then it will be accompanied by a tinkling. When the orifice is large and free, the air will pass in and out in ordinary breathing, and will produce in its vicinity a sound like that of blowing into the mouth of a glass bottle; and this kind of respiration is therefore called *amphoric*. In such cases there is seldom so much oppression of the breathing as in those where the air passes less freely, and accumulates in the cavity. In listening for the tinkling phenomena, you must recollect that they may be audible only in certain parts of the chest where the lung is not adherent, and where the liquid effusion does not reach. Generally, in the sitting posture, they are heard best about the mamma, and at the lower parts of the scapula; but I have heard them in every part of the affected side, and sometimes only in one spot. In fact, there must be a certain degree of tension in the walls of the cavity to make them good reflecting surfaces; and if this be deficient at the spot opposite to that on which the stethoscope is applied, the sound may be absorbed, and not reflected.

The addition of the liquid to the air in the chest makes the diagnosis still more easy. By percussion you find the exact level which separates the two, and you find that change of position varies the position of the liquid much more distinctly than when there is no air in the pleura. The motions of the liquid also may give very decisive evidence of its presence in a free cavity. On change of posture, or on coughing, the liquid will sometimes drop from the parts which have just been immersed, and the sound of this will exhibit the metallic ringing in so distinct a manner, that it resembles the note which a glass or porcelain vessel gives when struck. If the liquid be agitated more forcibly, as by the patient giving his body an abrupt jerking half turn, it can be heard to splash most distinctly against the walls of the chest; and this is the sign of *succussion* which was described by Hippocrates. This may also be accompanied and followed by the tinkling echo. The splashing is not easily heard unless there be a good deal of air in the pleura, and a certain quantity of liquid. The proportions of these are, however, better to be ascertained by percussion.

I have nothing to say on the treatment of simple pneumothorax. When it is the consequence of the long continuance of a

circumscribed pleuritic effusion, no artificial means can remove it; but if the pleurisy were treated in time it might probably be prevented.

In case of perforation of the pleura, it is generally necessary to adopt measures to relieve the irritation and oppression occasioned by the first access of air into the pleura sac. In the first instance this accident is often accompanied by considerable prostration from the sudden shock to the system, and the pain and cough are rather those of irritation than inflammation. Considerable doses of opium are necessary to allay this pain and the spasms into which the muscles are thrown: it may be advantageously combined with calomel and antimonial medicines; and sinapisms, or warm fomentations, may be applied to the side; but more active antiphlogistic measures cannot be used until the reaction takes place, which generally begins in a few hours, bringing with it heat of the skin, strength and hardness of the pulse, and great soreness as well as pain of the whole affected side. Then blood-letting, chiefly local, must be used with aperients and salines, according to the strength of the patient, and the degree of fever present, and followed by blistering, or other means of counter-irritation, in proportion to the continuance of the inflammatory symptoms. But it is not to be forgotten, that perforation of the pleura and its consequences are, in by far the greater number of instances, added to a previously existing disease, tuberculous phthisis; and the degree of advancement which this may have reached must much limit the propriety and the efficacy of the measures for this accidental inflammation that has been excited. The same considerations are to be held in view when, in consequence of the smallness of the perforation, or its valvular condition, air accumulates in the chest, and becomes a cause of oppressive dyspnœa. The immediate indication in this case is, doubtless, to give vent to the air by puncturing the chest; and this has been done with temporary relief in several examples; but before this operation is performed, it should be considered whether, as it can give only temporary relief, the condition of the patient be such as to make this likely to outweigh the pain and risks of the operation. These certainly are not great, but when added to the dubious view in which the friends of the patient may regard an operation which proves but imperfectly successful, are enough to deter us from the responsibility of recommending the operation in many cases. The case is different when the accident occurs before the consumptive disease has advanced far, when there is still much flesh and strength, and the physical signs have shewn that there is a considerable quantity of sound lung. The operation may be repeated if the air accumulate again. As it is impossible to avoid the continued introduction of air into the chest, the mode of performing the operation is a matter of much less consequence than in empyema. It is more desirable to puncture low down in the chest, to permit the discharge of the liquid as well as the air.

I have only now to say a few words on *altered sensibility* of the pleura and contiguous textures, particularly those nervous pains of the chest which sometimes simulate pleurisy. It not unfrequently happens in sensitive frames, particularly those of females, that an acute pain suddenly seizes some part of the chest, causing shortness of breath, and perhaps cough, very like the stitch of pleurisy. But there is no heat of skin; and the pulse, although often quickened, is not hard. The respiratory motions and sounds may be diminished by the restraining influence of the pain; but the other physical signs of pleurisy are wanting. There is no friction sound, or dulness on percussion; but there is sometimes a continued dull rumbling sound, which is produced by the vibrating contraction of the muscles, which is kept up by the sensation of pain. These affections appear to be neuralgic, and are often connected with a condition of the system the very opposite of inflammatory, such as that which comes on after considerable losses of blood, or when the blood is in a poor watery state, as in chlorotic females. I have known them, however, in connexion with irregular menstruation, to occur whilst this discharge was dodging, and to be relieved when it took place. In such cases blood-letting relieves the pain, but at the expense of the natural function. There are other kinds of pain in the chest, which may be called nervous, such as those associated with indigestion or a disordered state of the stomach, which are generally referred to the sternum, and, in the case of gastrodynia, are so severe as to cause great apparent dyspnœa. Pains are also felt in the chest and shoulders from a congested state of the liver. The affection called *pleurodynia* is generally considered to be of a rheumatic character, either in the intercostal muscles, or in the fibrous fasciæ lining the chest. If it occur singly it may be of little consequence; but if connected with constitutional rheumatic disease in other parts, whether accompanied by much fever or not, it is not to be lightly thought of, for it may readily be converted into a pleuritic or pericardial inflammation.

The treatment of the sharp nervous

pains in the chest must be directed more to the condition of the system which induces them, than to the part thus temporarily affected; for it may be to-day the chest, to-morrow the abdomen, another day a limb, that is the seat of these pains. Sinapisms, hot fomentations, and stimulating or anodyne liniments or plasters, will generally relieve the pain. Where the nervous irritation seems to arise from an undue depression or depraved state of the vascular function, as in chlorosis, the careful administration of tonics, especially steel medicines, with due attention to the excretions, will be most beneficial. When the pain seems to be the result of misdirected rather than excessive nervous influence, as in amenorrhœa without chlorosis, those remedies are indicated which tend to draw blood and nervous irritation towards the uterus, such as small doses of aloes, the hip-bath, and for a more continued effect, wearing flannel over the hips and thighs, and riding on horseback. If blood is to be drawn at all for temporary relief, it should be by a few leeches to the inside of the thighs; and I have known this measure, repeated for several nights in succession, bring on the natural relief when all other means had failed. But these are details which we have not time to pursue further.

I need scarcely occupy your attention with the rare cases of tumors situated in the pleural sac. Enlarged bronchial or thymous glands, and malignant disease originating in them, or in the cervical or axillary lymphatic glands, have been known to form tumors in the chest, especially the upper and anterior parts, pushing aside the lung, and endangering and destroying life by the obstruction which they occasion to the respiration and circulation. Their general symptoms are chiefly referrible to disorder of these functions; and they are not always accompanied with pain or other irritation, although in some instances their growth is very rapid. They are occasionally accompanied by hydrothorax, or pleuritic effusion. The physical signs which they produce are sometimes puzzling. They are,—dulness on percussion, and abolition of the respiratory murmur, in the parts entirely occupied by the tumor, and sometimes an outward prominence of the walls of the chest at these parts; bronchial respiration, sonorous rhonchi, and resonance of the voice in other parts, the results of partial pressure on the displaced lung; or a total abolition of these sounds when the oempression is more complete. In such cases we may be aided in our diagnosis by examining the glands in the neck and axilla. If these are enlarged, it is more than probable that the disease of the chest is of the same character. The distinction between these and aneurismal tumors will be considered hereafter.

When of a malignant character, these cases are, of course, beyond the reach of medicine. I have seen two or three cases of tumors in the chest, which, after threatening destruction by their rapid growth, and pressure on the vital organs, subsided under the influence of local bleedings, the internal use of liquor potassæ and hydriodate of potash, and other means calculated to improve the general health.

LECTURES ON BLOOD-LETTING,

Delivered from time to time,
At the General Dispensary, Aldersgate Street,

By Henry Clutterbuck, M.D.

Formerly one of the Physicians to that Institution.

Lecture III.

General Effects of Blood-letting in health.

Having, at our last meeting, given you a concise, but, I trust, a sufficiently-extended history of *blood letting*, as a branch of the healing art, I shall proceed to describe its effects generally on the system; by attention to which, we shall be better enabled to understand its operation in the removal of disease, or at all events to employ it with greater safety.

When blood, then, is drawn from any large vessel, either vein or artery, to a certain amount, and with a certain degree of velocity, (for this you will find to be a point of no small importance to be attended to,) the loss is followed by a number of changes in the system, which vary according to the quantity of blood lost, the rapidity with which it is taken, and the particular state of the individual at the time, in regard to strength, age, and other circumstances. These changes may be divided into the primary or immediate, and the secondary or more remote. This distinction is necessary in a practical point of view, as we are sometimes desirous of obtaining the primary, sometimes the secondary or more remote effects of the evacuation.

In adults of ordinary bulk, and at the middle periods of life, (excluding from consideration, for the present, both infancy and extreme old age, which will be noticed hereafter,) the abstraction of six or eight ounces of blood from a vein, slowly and quietly, as in ordinary venesection, produces, commonly, no striking or obvious effect; nor, generally speaking, any that is

perceptible afterwards: neither the feelings, nor any of the other functions, are sensibly disturbed or impaired by it. But if the evacuation be carried to a greater extent; as, suppose, from twelve to sixteen or twenty ounces, (the quantity varying, however, in different individuals;) or, if the smaller quantity indicated above, be taken away rapidly; then great and important changes generally ensue, and which take place in a somewhat regular order, as perceptible in the state of the different functions, both of body and mind.

There is commonly first experienced a slight feeling of languor; and if the pulse at the wrist be examined, it will be found to beat more feebly, and often more slowly, than before, though sometimes the reverse of this takes place. Breathing also becomes slower, in conformity with the pulse, and it is often irregular, with deep sighs. If the flow of blood goes on, the languor increases; the pulse becomes still more feeble, and sometimes fluttering. To these succeed paleness and coldness of the skin, and shrinking of the features; cold drops of sweat hang on the forehead, and sometimes perspiration breaks out over the whole surface; the eyes look glassy, and the pupils are dilated. Occasionally, also, there is vomiting, with other involuntary discharges; and, in a few instances, epilepsy, or convulsive movements, more or less general, take place. The pulse, at last, is not to be felt; respiration ceases, and consciousness is wholly lost. This is the state termed *syncope or fainting;* during which, if complete, there is, in appearance, an entire suspension of all vital movements, morbid as well as healthy;—it is, in fact, a state of apparent death.

The effects now described are, for the most part, temporary in duration, and continue but for a short space of time; it may be for a few minutes only, rarely for half an hour; when the pulse again begins to be felt at the wrist; respiration is renewed, often with yawning; and consciousness returns. Hysterical laughing or crying occasionally takes place, as the patient recovers from the fainting state. The recovery from this state is hastened by a sudden application of cold to the skin; and by placing the body in the horizontal posture. This latter, indeed, is a powerful means of preventing the occurrence of syncope altogether.

In some instances, headache of a throbbing kind takes place soon after bleeding, and which may continue for several hours; and, sometimes, the following night is passed without sleep: both circumstances shewing that not only the vascular action of the brain, but its functions also, have been disturbed by the evacuation. These effects, however, are not constant, nor even frequent.

When the primary and temporary effects resulting from the evacuation, as described above, have gone off, there often remain permanent changes, which depend upon the absolute quantity of blood lost, rather than upon the rapidity with which it is taken. If the quantity taken away should have been considerable, as a pound or more, there is not only a feeling of languor induced by it, but actual weakness; which is equally observed in the vascular and muscular systems: in the former, indicated by smallness and feebleness of the pulse—in the latter, by an inability to make the usual voluntary exertions.

When the loss of blood has been very large, as in cases of violent hæmorrhage, whether spontaneous or accidental; or where blood has been largely and repeatedly drawn, (as is often requisite for the relief of some violent and dangerous disease,) the weakness induced by it is in proportion to the quantity of blood lost, and is often very durable. The skin remains pale, and bloodless in appearance, for many months: there is great languor, or feeling of weakness; and an imperfect, and sometimes an irregular, performance of all or most of the functions.

The effects of blood-letting, now described, are by no means always in strict accordance with either the quantity of blood lost, or the rapidity with which it is discharged from the vessels; but are modified by a number of different circumstances; such as the bulk, and strength, and age of the individual; peculiarity of constitution; season, climate, and mode of life;—all these influence, more or less, the effects of this agent: and it is necessary you should make yourself acquainted with them, when about to apply it for the cure of diseases.

That some attention is due to the size or bulk of the individual from whom blood is about to be drawn, seems too obvious to require mentioning. The strong, also, you will readily conceive, bear bleeding better than the weak. This, no doubt, is true in a general sense; but I may add, that although strength of system is always an encouragement to bleeding, where the circumstances of the case are such as to require this evacuation, weakness, even when considerable, is not of itself, or necessarily, prohibitive of it. The nature of a disease is not changed by the patient being in a state of weakness at the time; though such a state renders more caution necessary in the use of this, as of all other active remedies.

As to age, there is hardly any that absolutely, and in all cases, precludes the use of blood-letting; for no age is exempt from the diseases and injuries which (in their very nature, it might be almost said) call for the use of this remedy. You must

not, for instance, adopt the notion, so commonly entertained, that infancy is a state of weakness that does not allow of the use of blood-letting. Strength and weakness in living beings have been looked at in too narrow a point of view, and measured chiefly by muscular power. Thus, it is said that a man is strong and a child weak; and that a horse is stronger than a man, and so on: strength, however, in living beings, is to be measured by the more or less perfect performance of the different functions of life in the aggregate. In this sense, strength is synonymous with perfect health. Physiologically, and also medically speaking, the strength of infants not only equals, but exceeds, that of adults. Vitality is greater in early life than at the later periods; all the actions of life, whether healthy or morbid, are then performed with greater energy. In infants, for example, inflammation is both more frequent and more active than in adults; and it runs its course more rapidly, through its different stages, to disorganization and death. Greater promptitude and activity of treatment are, therefore, necessary in the application of remedies in early life; and of blood-letting amongst the rest, when called for by the circumstances of the case.

The diseases of old age, likewise, are not unfrequently so active and inflammatory in their nature, as to call for loss of blood, as indispensable to the safety of the patient. The progress of disease, however, especially of inflammation, is commonly less rapid in advanced life; so as to afford more time and opportunity for the employment of remedial measures, or (which is scarcely of less importance) for the spontaneous subsidence of the disease—a mode of termination which, though exceedingly frequent in a great number of diseases, is by far too little regarded in general practice.

But both in infancy and old age much caution is necessary, in regard to the quantity of blood that is to be taken away. Under certain circumstances, infants may be bled with safety and advantage, even a few days after birth; as I know by repeated trial. But in such cases, a drachm or two of blood are to be regarded as equivalent to many ounces in the adult; so in old age, again, where bleeding is often called for, attention to quantity is required. The safe limits here may be taken, perhaps, as extending from four to six or eight ounces; and such quantities produce, in general, a very perceptible, as well as beneficial effect.

Peculiarities of constitution, or idiosyncrasies, as they are called, are met with occasionally, in regard to this, as well as other remedies. The strongest and most courageous man will sometimes become faint, from the loss of a few ounces of blood; while a weak and timid female may bear to lose a much larger quantity, without any sensible effect. Such peculiarities of constitution, however, do not seem, according to my observation, to influence materially the medicinal application of the remedy; so as to render it less necessary in the one case, or more so in the other, and they may disappear in the course of the disease; so that persons will often faint on the first bleeding, at the outset of the disease, even from the loss of a few ounces of blood, while they bear a repetition of it afterwards, and that for many times, without any such effect—a proof that blood-letting does not effect its purpose, as a remedial agent, simply by weakening the system.

Season and climate are, likewise, circumstances that have an influence on the use of this remedy. In winter, and in spring, diseases are said to be of a more active and inflammatory character, so as to bear blood-letting better than when they occur in summer and autumn. But the difference here, I conceive, is one of degree chiefly. In summer and autumn, the body becomes irritable from over-excitement, but is not rendered strong in proportion; nor can abstraction of blood, to the same amount, be so well borne as in the colder seasons of the year. Inflammation, however, is not less prevalent in the former than in the latter, though it commonly affects different organs; nor does it less require blood-letting for its relief, but not to an equal extent. The same observation applies to climate.

Luxurious living begets a tendency to inflammatory diseases. Hence the inhabitants of large towns require blood-letting more frequently than those residing in the country. The contrary of this, I know, has often been asserted; but, I think, without foundation. The Italian physician, Ramazzini, remarks, in his work "*De Morbis Artificum*," that countrymen, accustomed to hard labour and harder fare, bear loss of blood worse than the inhabitants of cities, who live luxuriously.

Such are the effects of loss of blood on the system generally. But it may be worth while to notice somewhat more particularly the way in which it appears to influence the functions individually; for there is here a considerable difference to be observed.

The first, and most immediate effect of blood-letting, is that which it exerts on the sanguiferous system, the heart and blood-vessels; the condition and action of which, it is capable of controlling more effectually and permanently, perhaps, than most other medicinal agents. This is, in

fact, its most important application, and the most deserving your attention; for the most violent and fatal diseases,—those, too, of the most frequent occurrence, are seated essentially in this class of organs; in proof of which, it is sufficient to mention fevers and inflammations of all kinds, in which the action of the heart and arteries, and in consequence the circulation of the blood, are always more or less disturbed. Blood-letting, accordingly, from its power of influencing the state of these, is one of the remedies the most frequently called for in practice, and which, when judiciously administered, is also one of the most effectual for the purpose. The effects of blood-letting on the sanguiferous system, therefore, are to be especially studied.

Loss of blood does not affect equally or uniformly the different parts of the sanguiferous system; whether we consider it in the light of a *depletory* measure (to use an Americanism), or as influencing the living actions, a striking difference, in this respect, will be observed. Thus, with respect to the heart, the action of this organ is sometimes rendered less, sometimes more frequent, and often irregular also, from loss of blood, according to the quantity lost, the rapidity with which it is taken, the state of the individual at the time in regard to general strength, and also according to the particular irritability of the heart itself. The effect, again, is influenced by disease, whether of the heart primarily, or of other parts of the system. These differences in regard to the action of the heart are generally indicated by corresponding states of the pulse at the wrist; but occasionally it is observed that the pulse is regular where the action of the heart is quite otherwise—thus proving the great share the blood-vessels have in effecting the movement of the blood.

So, again, with regard to the blood-vessels, the condition of which, in regard to strength and fulness, we commonly judge of by the pulse. A large abstraction of blood has very generally the effect of diminishing the strength and fulness of the pulse. On certain occasions, however, it has the contrary effect, the pulse rising, as it is termed, or becoming fuller and stronger than before. In order to understand this, you must recollect that the blood-vessels are not mere elastic tubes, but irritable muscular structures; and, like other muscles, under the influence of the nervous power, by which their irritability and contractility are determined. This contractility varies naturally in different parts of the system. It is greater, *cæteris paribus*, in the arteries than in the veins, though evidently possessed by both. It is greater in the smaller than in the larger vessels; and greatest of all in the capillaries.

It results from this difference in the irritability and contractility of different parts of the sanguiferous system, that disturbing causes of any kind produce different effects on different parts of this system. Certain emotions of mind, for example, will occasion the heart to throb violently and irregularly, without any perceptible change in the pulsation of the artery at the wrist. A feeling of dread, or the sight of a disgusting object, will often occasion a sudden contraction of the capillaries, so that the blood will desert the surface, and the pulse become nearly imperceptible. The sudden application of cold to the skin, however partial, will have the same general effect. The contractility of the heart and blood-vessels is also materially influenced by disease of different kinds, as will be shewn hereafter. In short, by no management can blood be so drawn as to diminish equally the fulness or tension of the whole vascular system.

The different secretions, in a healthy state of the body, are commonly diminished by loss of blood. When, however, they are suppressed from active disease, as in recent catarrh and other inflammations of the mucous membrane, blood-letting, by taking off or reducing the inflammation, occasions a return of the secretion, and that often in a degree beyond the natural.

Respiration, in ordinary cases of loss of blood, is only affected in conformity with the action of the heart. If this become slower, the breathing undergoes a corresponding change; but if the evacuation be carried so far as to induce syncope, or even an approach to it, respiration becomes very irregular, till at length it ceases altogether.

The effect of blood letting on the organs of nutrition is not a little curious. If the blood be so taken as not to disturb sensibly or materially any of the more important functions of the body, the appetite for food is often observed to be increased, with an equal improvement in the processes of digestion and assimilation; so that the body is better nourished afterwards, and the loss it has sustained is speedily compensated. Small and repeated abstractions of blood, in this way, appear to have the further effect of inducing a subdued and calmer state of the general circulation; in consequence of which there is less waste taking place by secretion and exhalation. This state of the circulation seems to be favourable to the deposition of fat in the cellular interstices of the body, and greater corpulency is often the result. The same is observed in other

animals, a circumstance that is taken advantage of by butchers, in preparing calves for slaughter.

We may learn from this, by the by, that blood-letting and abstinence, though generally combined in practice, are by no means identical, or even similar, in their manner of influencing the system; so as to admit of their being at all times fit substitutes for one another. Abundant experience has convinced me, that blood letting may be usefully resorted to, in a number of cases where abstinence is not at all required; and where, indeed, it would be rather hurtful, than beneficial.

I believe it may be safely laid down, as a general rule, with exceedingly few exceptions, that wherever there is a decided inclination for food of a simple and ordinary description, whether animal or vegetable, the appetite may be safely indulged; not merely without injury, but even with advantage. The effect of thus moderately exciting the stomach is, to equalize the circulation, a point of no small importance in diseases in general; a disproportionate determination of blood almost always taking place towards the diseased organ, whatever this may be.

Absorption from the cavities and interstices of the body would seem rather to be increased than diminished by loss of blood, provided the evacuation be so moderate as not to produce material disorder of functions. This seems analogous to the increased absorption by the lacteals, just mentioned, and answering the same purpose in the animal economy, namely, the supplying the system with what it has lost. The relief derived from blood-letting in some cases of dropsy has been ascribed to increased absorption. But the effect may, I think, with greater reason be referred to diminished secretion or exhalation. It may be doubted whether blood-letting can ever be usefully employed for the simple purpose of producing absorption, in dropsy; although its advantages, on numerous occasions, as a remedy for this disease, (or, to speak more correctly, for removing its cause,) is not to be questioned. But more of this hereafter.

The effect of blood-letting on the nervous system, or rather on the cranial portion of it, the brain, and its functions, (for we are but little acquainted with its influence on the other parts of the nervous system,) is peculiar, and worthy of notice. Sensibility to all kinds of impressions, internal as well as external, appears to be heightened, rather than diminished, by loss of blood: persons become more sensitive, or nervous, as the expression is. And with regard to the voluntary power, mobility, or the disposition to act in the muscles, both voluntary and involuntary, appears to be increased, while the power of acting with effect, and continuously, is diminished. Hence the tremulous, but, at the same time, feeble movements, that are performed in such circumstances.

The mental power, as far as one can judge, is not sensibly impaired, nor disordered, by even a considerable loss of blood; provided this take place so slowly and quietly as not materially to disturb the general system. It is observed accordingly, that in persons dying of hæmorrhage, and in many other diseases in which the brain itself does not participate, the cerebral functions of sensation, volition, and thought, continue to be exercised to the last, and long indeed after the pulse ceases to be felt. The independence, to a certain degree, of the brain upon the heart, is thus strikingly shown.

In the administration of blood-letting for the relief of disease, it is highly necessary to attend to the state of the mind and feelings of the patient; for these may be such as to render it difficult to judge of its necessity, and, indeed, sufficient to countervail any advantage likely to be derived from it. In persons of a timid disposition, the bare proposal of the operation of bleeding, or even the apprehension or expectation of it, will sometimes occasion such general disorder in the system, and in the pulse more especially, as may lead us to form an erroneous opinion as to the existing malady, and its treatment. In such cases, time must be given for the subsidence of the emotion, before the measure is determined on. You must not, however, in cases of emergency, be deterred by trivial circumstances of this kind, from insisting on the point; for the life of the patient often hangs on your decision. A little delay, with persuasion, will often do much towards reconciling him to the measure proposed.

ON THE

EMPLOYMENT OF CREOSOTE

AS AN

ODONTALGIC REMEDY.

By Edwin Saunders, Esq.*

Under the term *toothache* is commonly included a variety of affections, in which the teeth are in any manner implicated; whether or not they are the organs primarily affected, or the immediate exciting cause of the disease.

A very slight acquaintance with the

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structure and physiology of these organs will show that they may excite pain and morbid action from a variety of causes. Thus they may themselves fall into a diseased condition, and become the source of pain of unusual acuteness and severity, or they may excite irritation of an equally intolerable though less exquisite character, by producing inflammation in the membrane lining the alveolar cavities, and investing their roots. It is, also, by no means an uncommon circumstance for a morbid degree of sensibility to be induced in the bony structure of the tooth, rendering it painfully susceptible from the slightest change of temperature, simple pressure, or the action of acid.

Here, then, we find three different structures liable to come into a morbid condition, and each productive, when in this state, of pain of an unusually severe description. On a closer examination, it will be found that in each of these cases the pain produced exhibits different and peculiar characters, varying with the particular structure which may be implicated. Thus, in the first case, or when the pain arises from exposure of the internal membrane, occasioned by what is commonly called caries in the bone of the tooth, which is by far the most common cause of toothache in early life, the pain will be of an exeruciating and intolerable character, occurring in paroxysms, which, however, may be excited by any slight change of temperature in the mouth, or by the presence of any foreign body in the cavity of the diseased organ. In that, however, which is produced by inflammation of the periosteum and lining membrane of the alveolus or socket, the pain will assume more of the chronic character, rendering the whole tooth exquisitely sensible to the slightest pressure, while in the former case the tenderness will be confined to a single spot in the diseased cavity; the pain will also be continuous and slightly exacerbated on becoming warm, by active exercise, or on retiring to rest. The third case, viz. that in which an extreme degree of sensibility exists in the bone of the tooth (one of the most striking proofs of the vascularity of this structure,) as it is of a less serious and more temporary nature than the others, ought not, perhaps, strictly to come under the class of diseases of the teeth. It is generally the result of sudden exposure of some portion of the neck of the tooth, occasioned by rapid absorption of the alveolar process and gum; but it is frequently nothing more than a symptomatic affection, prevalent especially amongst young ladies, at the termination of the season in London, and subsiding as the tone and vigour of the constitution are recovered. In such cases, however, the creosote is not valueless. A lotion, composed of distilled water two ounces, pure creosote four drops, frequently allays the tenderness in a very remarkable manner; and this, or the spiritus camphoræ, are decidedly the most efficacious applications under this affection.

Thus, then, rejecting the last-mentioned affection as not coming within the category of diseases of the teeth, we have at least two distinct species of toothache arising from two distinct sets of causes: the first originating in the organ itself, the last in the enveloping membrane; the former giving rise to one of the most excruciating pangs of which the human frame is susceptible, and which, unless judiciously treated, compels the individual speedily to seek relief in the injurious and unnatural operation of extraction, entailing upon him all the evil consequences of the premature loss of organs on the possession of which, health, and the comforts of life, materially depend; the latter, attended with scarcely less formidable results, terminating in alveolar abscess, involving neighbouring teeth, and in some instances even the maxillary bones themselves.

A reference to the anatomy of these organs will render this distinction, in the origin and seat of the disease under these different affections, still more evident. Anatomically regarded, a tooth consists of the crown or body, and an articulating portion constituting the root or roots. To the constricted portion at which these two parts unite, and which is embraced by the gum, anatomists have given the term neck. The whole of the crown being exposed to the action, chemical or mechanical, of whatever passes into the cavity of the mouth, is covered by an inorganic crystalline structure, termed enamel. The articulating portion, which is more or less complex, according to the size and function of the tooth, and which is received into corresponding little sockets, termed alveoli, is closely invested with a layer of periosteum. This layer of periosteum is derived from that common to the maxillary bones, in the following manner: This membrane, after covering the maxillary bones, dips down and lines the alveolus or socket, until it reaches the small opening through which the dental vessels are transmitted, and is from thence reflected over the root of the tooth which it invests, as far as the neck. Thus, between the bony socket and the root of the tooth, a double membrane is interposed, which not only affords additional means of nutrition, but prevents, like the interarticular cartilages of the joints, the unpleasant effects of the contact or concussion of bone. These, then, are the membranes which

are found external to the organ. On making a section of a tooth, with a view to discover its internal structure, we find a cavity occupying its central portion bearing a strict relation to the form and size of the tooth, diminishing in volume as it proceeds through the root or roots, until it terminates in a minute and almost imperceptible foramen. An opening, corresponding in size and situation, is found in the alveolus or socket, and these give passage to the nutritious vessels and nerves which subsequently ramify in the very substance of the tooth. Such, then, is briefly the internal structure of these organs; for as it is not necessary to the present inquiry to enter more fully into the physiological characters and distribution of their vessels, I shall not trespass farther upon your time in these purely anatomical details, but shall proceed at once to illustrate their application in the treatment of the morbid affections to which these organs are liable.

It is well known that the human teeth are exceedingly liable to fall into a morbid condition, in which a disorganization and decomposition of their structure take place, and which is familiarly recognized under the terms caries or gangrene. The origin, exciting causes, and precise nature of this disease, are still involved in much obscurity, and will probably long continue, as at present, to divide the opinions of physiologists. It is much less prevalent in the ruder and more uncivilized states of society, and almost unknown amongst the lower animals. It would seem, therefore, to be in some degree connected with a refined and artificial mode of existence, frequent indisposition, the free use of various drugs, and infantile diseases, which, occurring during the formation of the teeth, render their structure imperfect, and less capable of resisting morbid influences in after life. But whatever may be the causes of this very general affection, and probably if they could be clearly ascertained and defined they would be found altogether beyond control, the effects are uniformly and unequivocally injurious. Commencing by a small dark spot immediately under the enamel, which soon breaks down, from losing the support of the subjacent bone, portion after portion of the osseous structure of the tooth is destroyed, until the central cavity and its contained vessels become exposed. In this state the contact of atmospheric air, of the tongue, or the slightest pressure of a small particle of food on the delicate and extremely vascular structure thus suddenly denuded, is sufficient to excite in it a high degree of inflammation. From the circumstances of the case, however, being included within a circumscribed cavity whose hard unyielding walls admit of no swelling or expansion of the highly organized membrane it contains, an intensely congested state of the vessels and nerves takes place, and produces that hell o' a' diseases, as Burns has emphatically styled it, the toothache. In these cases antiphlogistic treatment is out of the question, from the difficulty of its application; and even in the few cases in which it is practicable, as I have more than once experienced, fails to afford the slightest relief. The only treatment of which such cases appear susceptible, and to which, if judiciously applied, the symptoms will almost invariably yield, and frequently in a very short space of time, is the reiterated application of some highly stimulating or narcotic drug. Amongst the most accessible and valuable preparations of this kind is the camphorated spirit: it is generally far more efficacious than the tinctura opii, lunar caustic, and many other of the popular remedies usually employed in such cases. The mineral acids, which sometimes afford a temporary relief, are highly objectionable, on account of their action upon the surrounding bone, which speedily destroys the tooth, re-exposes the membrane, and gives rise to a fresh, and probably still more severe attack of the pain. It is in these cases, however, that the creosote becomes valuable, and probably has a good claim to be considered a specific. In few cases of pure toothache, arising from exposure of the internal membrane, not complicated with inflammation of the periosteum, will it fail, after two or three applications, to afford relief. To obtain its best effects, however, some care is required in its application. Fremanger, in the *Bulletin Général Thérapeutique*, has laid much stress upon the importance of previously wiping out the cavity with a small piece of lint introduced upon the point of a probe: and whenever this is practicable, it is undoubtedly a desirable preliminary, because the mucus of the mouth, which is always abundant where the masticatory function is suspended, will prevent the application from coming immediately into contact with the diseased structure. In a vast number of cases, however, the patient will be utterly unable to endure the agony which this preparatory process inflicts, even when performed in the gentlest manner. A more important hint, but which is too seldom regarded, is to employ the cotton or lint which serves as the vehicle for its introduction, in as small a quantity as possible. The more common practice is to introduce a piece of lint or wool of sufficient size to fill the cavity, which requires so much pressure for its insertion as to cause it to part with a considerable portion of the drug, and

thus to prevent it reaching the seat of the disease. If relief be not obtained at once, fresh applications may be made every ten or fifteen minutes, until some effect appears to be produced. Care should also be taken that the wool or lint be not too fully charged with the creosote, considerable pain and inflammation being frequently the result of the effusion of a small quantity on the gums and lips. Mr. Cormack observes, "I once saw a pretty extensive sore, resembling a burn in appearance, caused by the overflow of saliva, which ensued upon the application of creosote to a carious tooth." With the utmost care, however, it is scarcely possible to prevent a small portion escaping into the mouth, more especially if the tooth be situated in the upper row; but this, if mixed with the saliva, which generally flows in great abundance on its application, will occasion but little inconvenience. If the diseased tooth be situated in the lower row, the effect will generally be obtained in a much shorter time than in the upper, because in the former case the creosote will tend towards the seat of disease by its own gravity, while in the latter, unless the patient be placed in a recumbent posture, that force will exert a contrary influence. It is an useful hint to make the patient recline with the head low, and on that side in which the affected tooth exists; as, for instance, if the application be made to one of the molar teeth, on the right side of the upper row, direct the patient to lie on the right side for a short time; the creosote will thus be likely to reach the affected spot, and the risk of any portion of it passing into the throat and stomach will thus be obviated. It would be scarcely necessary to observe, were it not that the notion has been entertained by some of its more zealous advocates, that creosote can exert no preservative effect upon the tooth itself. It simply alleviates the symptoms, without in any degree arresting the progress of the caries, for which different treatment must be sought. The opinion, however, which has been held by others, that it accelerates the decomposition of the bony structure of the tooth, seems to be equally unfounded, since neither alkaline nor acid qualities are developed on the application of the most delicate tests.

It is under these circumstances, then, and with these precautions, that creosote becomes a valuable odontalgic remedy. The precise mode of its action, like that of most other remedies employed for the relief of this affection, is not clearly understood. It has been said that it acts as an escharotic, and destroys the nerve; but a tooth which has been relieved by this remedy may, if the disease be allowed to proceed, again become painful from re-exposure of the membrane, which would not be the case if the nerve were really destroyed. But this is too vague and indefinite an hypothesis to deserve much attention, for if it be intended that the whole nerve and vessels are destroyed, it is obviously incorrect, for it cannot come into contact with the entire nerve, and cannot, therefore, exert so potent an effect upon it; and if it be merely intended that the exposed surface of the internal membrane is thus powerfully acted upon, the expression is obviously objectionable. It has also been suggested that it may enter into a new chemical combination, and form a protecting crust over the denuded membrane; and again, that it affords relief by coagulating the albumen, an effect which creosote is known to exert upon the dead body, but of which there is no proof during life. A more probable solution appears to be, that it acts as a powerful anodyne, by exhausting the nervous energy; and as creosote has been employed for arresting hæmorrhage, it is possible that it may relieve the congested state of the membrane, by causing a sudden contraction of its vessels. If this view be correct, an analogy will immediately be seen between its mode of action and that of the alcoholic solutions of some of the other anodynes, the essential oils, and similar preparations.

It is a remarkable fact, that the old and popular remedy, well known in many parts of the country, and still highly prized amongst the lower classes in London, the oil of burnt paper, which from a very early period has maintained a high reputation as an odontalgic remedy, has been found to contain a considerable portion of creosote, and has been justly supposed to be indebted to it for its peculiar virtues.

Creosote, then, is a valuable remedy in those cases in which the pain produced is of an acute and excruciating kind, arising from sudden exposure of the internal membrane. But another species of toothache, equally intolerable, though less acute, and which is sometimes described as of a rheumatic character, is frequently encountered in practice. This, then, constitutes that other species of toothache which arises from inflammation of the periosteum and membrane lining the socket. This may exist along with or independently of the former. It is frequently produced by cold, injury, or any of the ordinary causes of inflammation in a sound and previously healthy tooth, or in roots of teeth which have been destroyed by caries. Under this affection the pain is of a less poignant and local character, from the sympathetic inflammation which is

soon set up by continuity of structure in the periosteum of neighbouring teeth and the surrounding gum. There is a dull throbbing pain, not confined, as in the former case, to one particular tooth, or to one particular part of a tooth, but extending more or less over the entire side of the face, giving rise to earache and severe neuralgic pain about the temples. The tooth will be exceedingly tender, and pain, or at least an unusual sensation, will be experienced in closing the mouth, when it will be generally found to encounter the opposite teeth sooner than the others, indicating a slight degree of tumefaction of the periosteum, which has partially ejected the tooth from the alveolar cavity. This symptom is not, however, invariable, being dependent upon the form of the roots. If these assume a simple conical form, a slight tumefaction of the periosteum will have the effect of ejecting, in a very trifling degree, the tooth from the alveolus, and, provided the pressure of its antagonist be avoided, the pain will be in such cases relieved, and the symptoms speedily subside. Where, however, the articulating portion of the tooth, or the root, assumes an irregular form, or is dovetailed, as it were, into the maxillary bone, as is sometimes the case with the molar teeth, no such effect will be produced. In such cases the interposed membrane being unable to relieve itself, by a slight separation of the hard surfaces between which it is included, will take on a high degree of inflammatory action, speedily terminating in suppuration, or alveolar abscess. Such is the exacerbation of the symptoms attendant upon this conformation of the roots, that, under the most judicious and prompt treatment, this result can scarcely be avoided. In either case, however, previous to its arrival to this termination, a great degree of tenderness will be experienced in the tooth; and a greater degree of mobility may be detected in it than is naturally possessed by these organs. In such cases, creosote, at least in its undiluted state, and the ordinary stimulating applications indiscriminately recommended for the relief of the tooth-ache, are not only useless, but highly prejudicial. Warm fomentations, the abstraction of blood from the gum by scarification or leech, combined with the exhibition of saline aperient medicines, are the means to be adopted, while every thing stimulating is contra-indicated. More or less fever, or constitutional disturbance, will frequently be observed concomitant with this affection, and much may in all cases be accomplished by correcting the secretions, and attention to the general health. If scarification be resorted to, it should be freely and fearlessly performed, so as completely and suddenly to relieve the gorged and distended state of the vessels; and their immediate contraction, consequent upon this, should be rendered permanent by the use of some astringent lotion. For this purpose, if at all in these cases, the creosote, in the proportion of four or five drops in half a tumbler of water, may be useful. Equal parts, however, of the tinctures of myrrh and bark, with or without a small quantity of alum, form a very useful preparation of this kind, which probably affords all that can be desired.

It was doubtless in these cases, although the distinction was not pointed out, that the idea of relieving tooth-ache by rubbing in aconitine ointment was suggested, with the expectation of reaching the original seat of disease by directing the remedy to a part sympathetically affected. The practice, however, was soon abandoned, from its inefficacy, as it should have been from its erroneous principle. It was obviously proceeding upon a false assumption, to regard the tooth-ache as simply a neuralgic affection, of which there is not the slightest proof or even probability. Nor is there any foundation for the idea that the teeth are endowed with any special and peculiar sensibility; the high organization of the internal membrane, and the circumstances in which it is placed, being included within a circumscribed bony cavity, are amply sufficient to explain the unusually severe character of the pain: and the symptoms exhibited in inflammation of the periosteum are not more severe than might be anticipated from a membrane in such a state, interposed between two bony surfaces. As, however, the drug which forms the subject of this paper is not useful in this species of tooth-ache, I shall not occupy your time with any remarks upon its pathology and treatment, but shall conclude with a few hints respecting the diagnosis.

In a case of tooth ache, then, it will be necessary to inquire, first, whether the pain be of a violent and excruciating kind, or of a chronic and rheumatic character; secondly, whether the whole tooth be tender and unusually mobile, or whether there be one particular spot to which this exquisite sensibility is confined; thirdly, whether the pain originated in a sudden paroxysm, or supervened in a gradual manner, upon cold or injury. If it exhibit the former of these characters—that is, if the pain be exceedingly acute, confined to one spot in a carious cavity, with the exception of which the whole tooth may be pressed upon with impunity—and if it occur in sudden and violent paroxysms, creosote may be employed with the best results, in the man-

ner and with the precautions already pointed out. If, on the contrary, the symptoms be of the opposite description, it will be not only useless, but injurious. It is only, I am convinced, by discriminating in this manner the character and seat of the disease, that creosote can be employed with any confidence, or that a just estimate can be formed of its utility. It is a distinction which, though not generally recognized, I believe to be of the first importance in the treatment of toothache, and which, if duly observed, will reduce this apparently intractable and anomalous affection to the same obedience to the skill of the surgeon which other more formidable diseases exhibit. On this account, I have always brought it prominently forward in my lectures on the Anatomy and Diseases of the Teeth, delivered at St. Thomas's Hospital; and the same conviction of its importance has induced me, on this occasion, to trespass so long upon your indulgence.

16, Argyll-street, April 1838.

RECOLLECTIONS OF CHOLERA:

ITS NATURE AND TREATMENT.

By W. Griffin, M.D. Limerick.

No. XI.

Emetics.

In speaking of the use of emetics in cholera, it is meant to treat of such medicines when they are given specially to produce an emetic effect. Tartar emetic, ipecacuan, and sulphate of copper, have been administered, as we have seen, with no inconsiderable success, but neither with the object or the effect of occasioning vomiting, at least after the second or third doses. Emetic medicines, in fact, generally cease to operate as such when continued in large doses at short intervals, and if after the cessation of their ordinary operation their curative influence not only continues but increases, it must be attributed to some action very different from that which they have had upon the stomach, and cannot be considered as the result of mere emetic treatment. The use of the foregoing medicines, as well as sulphate of zinc and mustard, specially for their emetic effect, has nevertheless been very popular; more so, perhaps, than any other remedies, but there can be little doubt of their value having been much over-rated, and of their having proved to be of very variable utility, as they happened to be employed at an earlier or later stage of the disease.

I have seen the ipecacuan emetic very much given during the prevalence of cholera at Limerick. The regular practice, in fact, at all the hospitals, was to administer an emetic as soon as a patient was brought in, without much reference to the period of attack, except that a little mustard was usually added, or plain mustard substituted, if there was great collapse. This practice was often productive of good in the early stage, restoring the heat of skin when it was beginning to fail, and apparently rendering the disease more amenable to after treatment. Occasionally, but rarely, it altogether arrested the disease. On the whole, however, I question much whether the vast majority of cases were at all permanently influenced or benefited by it, and in some it certainly seemed to do direct mischief. This was, I think, especially the case in those states of deep collapse in which the nervous sensibility was so much diminished that an enormous dose was required to bring on the act of vomiting. I remember an instance of a young girl, brought into hospital in a state of great prostration of the vital powers, who expired almost immediately after the action of a mustard emetic. From her appearance, and the manner in which other patients similarly circumstanced lingered out, I believe she would have lived at least three or four hours if she had got occasional doses of some mild cordial, or had been left altogether alone.

There is one description of emetic—the saline, which from its having attained a high reputation in different and very distant countries, I cannot help thinking is entitled to some serious consideration; I can offer no experience of my own in its favour, but the published testimonials to its virtues are very strong, although, unfortunately, not supported by those incontrovertible evidences—classified tables of cases and results. Dr. Ockel, of Moscow, was in the habit of giving a table-spoonful of common salt in a pint of water every hour, until bile appeared in the evacuations, or there was a general amendment of the symptoms. He had 13 cures out of 15 bad cases; a practice eminently successful, if those bad cases meant persons in collapse, and if they meant cases of cholera with the pulse perceptible, still

sufficiently successful to compete with the calomel treatment, making only about 14 deaths in the 100. 15 cases, however, make too small a number to draw any such favourable inferences from. Dr. Ockel believes secondary fever seldom follows this treatment; a most important advantage, if the fact be as he conjectures, which I very much doubt. Drs. Ysenbaek and Brailow were also advocates of this praetice, and a Dr. Pidduck states that in his experienee of it he lost only 6 cases out of 40, or 15 in the 100: the extreme average lost under the calomel plan in many hundred cases. 12 of these reeoveries, he says, were out of the cold, blue, and pulseless state! a most extraordinary number certainly, if we recolleet that under the most successful treatment yet accurately ascertained and proved by large numbers, for every 2 or 3 reeovered out of collapse, 7 or 8 have died, so that for 12 recoveries from collapse there ought to have been, at the least, 30 deaths! Mr. Beaman, of Covent Garden, asserts that he eured 11 conseeutive cases of eonfirmed cholera by giving three table-spoonfuls of salt in half a pint of water, repeating the dose every five minutes when it did not vomit, and even when it did, repeating it to third time if the pulse began to flag. He afterwards completed the eure by the occasional exhibition of saline draughts.

Bleeding.

There are many reasons why bleeding should be looked upon with considerable suspieion or distrust in cholera. The disease, after the first few hours, becomes one of exhaustion and collapse, in which the spark of life fliekers so faintly that the detraetion of blood, be it ever so little, would seem to an inexperienced person to be fraught with certain danger. We must, however, in endeavouring to arrive at just views of the effects of any remedy in cholera, divest ourselves of all notions derived from theoretical eonjectures (for they are nothing more) as to its nature, and turn our attention entirely to the recorded experience of the profession. This, indeed, is unfortunately rather made up of opinions than faets, but such as it is it exercises too extensive an influence over medical practice to leave it unconsidered. As I believe much of the discrepancy of medical opinion on this subject, of which I have given a specimen in the introduction, has arisen from the different effeet of the remedy as it happens to be employed at an earlier or later period of the disease, it is of some importance to consider its influence with reference to its employment in the very early stage, in the stage which eommences with the first symptoms of collapse, such as the flagging of the pulse, the cooling of the skin, and alteration of the countenanee; and the stage of perfect collapse. We may, perhaps, in this way form some correet conclusion from the many opposite doctrines put forward on the subject, both in India and Europe, by physicians who have had the greatest experience of the disease.

In the first stage, it appears to me the testimony in proof of the successful employment of blood-letting in very numerous cases is beyond question. Dr. Craigie, of Edinburgh, who seems to have watched and weighed the effects of many remedies in his extensive eholera practice, with particular eaution, reeommends it strongly, and asserts, that "in open distinet eases, where the vomiting, purging, and eramps, are violent or frequent, but not of long duration, there is no difficulty; for a blood-letting of 20 or 24 ounees, followed by 10 grains of calomel, and half a grain of opium, repeated every six or eight hours, will infallibly remove the disease if any thing will." "So uniform," he says, "was the suceess of this method, that both the district surgeons when called in sufficient time, and the house surgeon when a case was admitted in the proper stage, invariably employed this remedy; and no case *in which it was admissible* was lost."

"The first effect of the blood-letting was to aggravate the eramps, if present, and to induee them, if not: but it was quickly followed by relaxation of the system; diminution and total removal of the epigastric-umbilical weight and distension; cessation of the vomiting and purging; and rapid and sure convalescenee. This mode of treatment required, indeed, two conditions;—one, *the early and preliminary stage of the disease before the approach of collapse;* the other, *that of the patient having otherwise a good and comparatively sound state of the different organs.*" Mr. G. Hamilton Bell, and Dr. Bigbie, of the Castle Hill Hospital, Edinburgh, found it equally successful; Dr. Ains-

worth, and Mr. Morday, of Sunderland; Dr. C. W. Graham, of Dalkeith; Dr. Gibson, in his report of cases in the north of England; and numerous other medical men who had experience of the disease, were warm advocates of the practice. Dr. Colclough, staff surgeon, as appears in a report to the Army Medical Board, bled in almost all cholera cases before collapse had begun, and asserts, that even those who go into collapse after blood-letting, are more likely to get through the consecutive fever; but all the cases extensively bled, he says, were prevented going into collapse, and were out of danger in from 6 to 12 hours. He states, moreover, that in no instance in which bleeding was resorted to in premonitory or bilious diarrhœa, did the patient go into regular cholera. It is essential to mention that in all these cases the auxiliary treatment was sufficiently energetic, the bleedings having been usually followed up by large doses of calomel, with opium, repeated according to circumstances. Even in the premonitory cases spoken of by Dr. Colclough as yielding to venesection, emetics were also made use of, succeeded by calomel and opium, in the proportion of 8 grains of the former to 1 of the latter.

The only physician whose experience I find reduced to numerals in reference to the effect of blood-letting, is Dr. Burrell, who states, that out of 100 patients 88 were bled and 12 were not bled, and that of the former only 2 died, while of the latter number 8 died.

All this seems so decidedly favourable to the abstraction of blood in the early stage of cholera, that it might be inferred no further encouragement or evidence of the advantage of the practice was necessary; but, setting aside the numerous opinions which are as decidedly opposed to it*, there are several considerations which should have their due weight in endeavouring to arrive at an accurate estimate of its value.

1st. It was not tried alone, or almost alone, as the calomel treatment had been, but was followed by very efficient auxiliary remedies, to which, from what we know of their influence, much of the success may be fairly attributed.

2dly. Even if the success was to be attributed to the blood-letting alone, it is a question whether it could be at all compared to the results of the calomel treatment alone. The amount of recoveries and deaths, in a given number of cases quoted from Dr. Burrell, is of little value, unaccompanied by a statement of those cases when they fell under treatment.

3dly. Its success is connected with conditions not always easily ascertainable: it must be often difficult to decide whether a patient is not too near the stage of collapse to make the depletion adviseable, or whether he may not be in unsound health, with respect to one or more organs; Dr. Mackintosh states that scarcely any of those who died of cholera, in Drummond Street Hospital, were found to have had sound viscera.

On the whole, I should say, there can be no reason to doubt that some cases of cholera, in the primary stage, may be saved by blood-letting, in conjunction with other active treatment, which without the depletion would be lost, and that in a still greater number the cure would be accelerated by it. There is, however, one consideration that should never be lost sight of:—its indiscriminate employment, in the way calomel was made use of at Limerick, would include its misapplication to so many cases, even on the admission of its warmest supporters, that it could not fail to be attended by a considerable mortality, and on the other hand, the conditions essential to its success would seem to require some further investigation, before we could venture, generally, to adopt those which have been suggested for our guidance in any discriminate treatment.

The great majority of the Indian practitioners believed that it was never too late to bleed, as long as it was possible to obtain blood; in England, on the contrary, the great body of the medical profession considered that the operation was less adviscable, in proportion as the disease had advanced, and was most dangerous when it had reached the verge of collapse. It certainly seems to have wholly disap-

* Drs. Clanny and Haslewood, of Sunderland, state that blood-letting was employed by them, but, unless in some cases, without much benefit: in several instances, in which oppression of the chest, severe cramps, and perceptible pulse, seemed to indicate, if not to warrant the evacuation, from 10 to 20 ounces were drawn, without relief, after which the pulse failed and never returned. In other instances in which cramps were the prominent symptoms, it was followed by decided improvement.

pointed many, who, imbibing confidence from the eastern doctrines on the subject, had commenced the practice with a sufficient degree of boldness. In Limerick, the physician who gave it the first, and I might say the only fair trial, was the late Doctor Carroll, and in a conversation which I had with him afterwards, on the subject, he told me he had persevered in it to a degree of rashness before he could bring himself to abandon it, but that he had no hesitation in saying it was a most fatal practice. I employed it myself only upon one occasion, in the case of a woman about 45 or 50 years of age, with a good pulse and warm skin, but who was evidently losing ground. After drawing from 12 to 16 ounces I found the pulse was flagging and the skin becoming cooler, upon which I instantly tied up her arm. In less than an hour, however, she was in collapse. I believe, indeed, not from this case, for single cases prove nothing, but from every thing I have heard or read upon the subject, that the abstraction of blood, when collapse is impending, is a dangerous measure. Perhaps there may be something in the nature of the disease in India, not obtaining here, which would account for the different sentiments entertained by practitioners in that climate.

The question still remains, when collapse has actually taken place, is it adviseable to bleed? If such a question were asked of a medical practitioner, who, for the first time, beheld a patient in the collapse of cholera, he would be very apt to doubt the sanity of the inquirer. He sees a human body before him—scarcely a living one; the arteries pulseless; the surface cold, and covered with a clammy death-dew; the skin puckered, and shrunk up; the countenance cadaverous; the voice whispering and sepulchral: he knows that all these frightful appearances were preceded by, and probably the result of profuse evacuations, and he naturally infers, that to attempt the recovery of such a case, by abstraction of blood, would be little short of folly or madness. The fact is neverthless certain, that in these very circumstances we are strenuously recommended, by most respectable authorities in India, perhaps by the major part of the British practitioners there, to bleed, if blood can be obtained, and as Mr. G. Hamilton Bell expresses it, "not to cease our efforts while life remains." They do not pretend, indeed, to rescue many patients from the dangers of such a condition, but believe that their only chance of life lies in the possibility of a due quantity of blood being drawn, and if that should not save them nothing else could.

If, in Europe, the general feeling of the profession was decidedly opposed to venesection, when collapse was impending, we may readily conclude it was still more absolutely so after it had taken place. It had its occasional advocates, however, in these countries as well as on the continent, and whether it for the most part proved a failure, or that the utmost success which ought to be expected from any remedy in that state, was too little to give it a reputation, I know not, but it certainly made no general impression on medical men.

Much as the condition of a patient, in collapse, may prejudice one against its use, its advocates are not without strong reasons in favour of the practice. The thick tarry state of the blood, in those cases, might very rationally be supposed to render its passage through the minute capillaries more difficult, and if we consider the weakened condition of the heart and arteries, almost impossible. The beats of the heart are sometimes so feeble as to be scarcely heard, and the ear applied to the chest can perceive only a dull sound, denoting that the ventricles do not expel even the little blood they contain: under such circumstances, must not the tarry state of that fluid present an extraordinary obstacle to its circulation? Must not its retardation, in the small vessels, and consequent congestion in the large, increase the difficulty, and still further oppress the failing and labouring heart? And might not the abstraction of blood from the extremities relieve this congestion, and enable the heart and arteries even with their diminished power to accomplish a circulation? All this has been said, and whether correct or otherwise, with much feasibility, but the truth is, we should be guided in our practice in reference to this remedy, as in reference to others, by experience alone, of which, however, any amount we yet possess is very limited. One consideration, and one only, has had great influence in inducing me to believe in its possible propriety. I have read some few cases, evidently correctly de-

tailed, in which the collapse was perfect, and the blood could only be obtained, stillatim, by means of frictions and fomentings. In proportion as the patients improved, the venesection was repeated to the third or fourth time with apparent advantage, and there were eventual recoveries. Of venesection it cannot be said, as it has been of calomel, opium, stimulants, salines, &c. that neither good nor harm was done, considering the condition of the patient at the time. Such a remedy, in such circumstances, could not be indifferent in its effects, and must, I think, of necessity destroy life, if it was wholly unadapted for relief. If it was not the chief means of effecting the cures in these cases, it is very hard to reject the belief that it materially assisted in them.

The most undeniable objection to the practice of venesection in collapse, and that which has chiefly interfered with a more general trial of it, is the difficulty of succeeding in obtaining blood. It is a question, indeed, whether this difficulty, however strenuous our efforts to overcome it, might not interfere with the practice to an extent which would make it a matter of indifference to what conclusion we came upon the subject.

Finally, I think it must be evident to the profession, that we do not possess any sufficient or accurate records of the results of the practice in any stage of cholera, to rest content with the best-considered inferences which can be drawn, even by those who have had a very extensive individual experience.

Cold Water.

There is much credit due to Dr. Hardwicke Shute for the accuracy of observation which led him to infer the necessity of watching the progress of cholera uninfluenced by remedies, in the hope that he might thus ascertain the natural efforts of the constitution, if any, to relieve the disease. The result of his cold water experiment, as justifiable under the circumstances as it was bold, has been, that we are now enabled with some degree of confidence to resist the absurd pretensions of remedies which are useless or mischievous, and to determine with something like an approach to correctness the comparative value of such as are truly influential. Dr. Shute does not assume, as has been done by many of his followers, any direct or controlling power over the disease for cold water, but considers its employment as merely satisfying one of those conservative instincts which may be observed in every disease in which a natural effort at recovery is made. He has shown that when a patient in the collapse of cholera is left to the ministry of nature, no amendment can be observed in the first five or six hours, except some diminution in the purple hue of the extremities; that in the next six or eight hours there is a manifest improvement in the countenance of the patient, and more disposition to sleep, but often no restoration of pulse nor increase of temperature; that in some cases the pulse may not become perceptible for twenty-four or even thirty-six hours; that from this period, the pulse, the animal heat, and the secretions, are very gradually restored; and that at the end of forty-eight hours, or the third day from the commencement of the collapse, the patient is convalescent. He also adds, what I think a more enlarged experience would have shown him was an erroneous conclusion, *that convalescence takes place in all cases without any consecutive fever.* I remember the case of a lunatic, aged about 40, who was brought into the Strand Hospital in collapse, and was allowed cold water *ad libitum.* His thirst was so great, that, dissatisfied with the cans of water placed near his bed, he often got up, and, taking the corks out of the tins which were lying full of the cooled water remaining since it was heated for application to the feet, drank it to vomiting. He continued drinking and vomiting in this incessant manner for nearly three days, after which the pulse and heat of surface returned, and he became apparently convalescent. In a few days after, however, the treacherous blush appeared in the face, and ushered in a description of consecutive fever which proved speedily fatal. This was an accidental case, occurring previous to the publication of Dr. Shute's papers, and the only one of cold water treatment which I have seen. The plan, however, has had repeated trials since in the hands of others, the results of which have not borne out Dr. Shute's conclusions respecting the absence of consecutive fever. I find, indeed, that some of the deaths which occurred under his own care, in the Gloucester Infir-

mary, in a subsequent experience, were occasioned by congestive fever, and one might be led to suppose it was even a frequent consequence of the cold water treatment, from the reports of Messrs. Garratt and Harrison, of the Isle of Man, who state, that although a few recoveries took place with them under this plan, they had reason to believe that when persevered in too far, it induces very high consecutive fever, and consequent congestion of brain. This statement might appear very strongly opposed to Dr. Shute's experience; but it has occurred to me that an explanation may possibly be found in a fact noticed by the physicians in Paris, that insatiable thirst is a bad sign in cholera; those who drink ravenously for days generally dying. If this be true, it could be no wonder that among numbers treated on the cold water plan, those only should die who drank most and persevered longest; not that they were injured by it, but that their excessive thirst denoted a severer form of disease from the commencement.

Dr. Shute clearly and decidedly states, that all his observations on the advantage of the cold water treatment, apply solely to the stage of collapse, and it is most essential to recollect this distinction. No mistake so fatal could be made as to rely on it in the previous stage; it would be, in fact, to leave the cases to nature, when, as I have already stated, they would inevitably run on into collapse. About half the cases admitted into hospital, or met with in private practice, are cases of collapse, and if the cold water plan could effect as much for these as calomel does for those in the primary stage, the disease would be stript of all its terrors, and be considered amenable to medicine, like any ordinary and well-understood malady.

Let us now endeavour to ascertain the comparative value of the medicinal treatment, and non-medicinal, in collapse. Dr. Shute's first cases gave twelve recoveries out of fourteen, and the twelve were consecutive; the pulse was almost imperceptible in all! I most fully believe that Dr. Shute has given a candid statement in this instance; but when we recollect how few the number of his cases, and how universally fatal the stage of collapse has been with the profession at large, whether actively treated or let alone, the deaths varying from seven in ten to eleven out of twelve, we must necessarily receive such a report with some qualification. We cannot but conclude the cases had assumed that milder and more protracted form which they do every where when cholera is declining, and that had they extended to a much greater number, and over a longer period of time, the results would have given a far less favourable average. In Dr. Shute's subsequent experience, he had fifty-two cases and fifteen deaths; but as he does not state that all the fifty-two cases were in collapse, the report is almost useless. As this was the entire number of cholera cases admitted up to the time of his report, it may perhaps be fairly concluded that not more than half of them were received with imperceptible pulse, under which circumstances we should anticipate the following mortality:—

Of 26 cases in the primary stage died 2 or 8 in 100. Of 26 cases in collapse died 18 or 7 in 10. Total deaths in 52 cases 20 or more than 1-3rd.

This is the most favourable result we could, under any circumstance, look forward to, calculating from the most successful practice in both stages; but Dr. Shute lost only fifteen out of the fifty-two; and if the cases of collapse exceeded twenty-six, his success was still more extraordinary.

I cannot, with the experience I have had in cholera, bring myself to believe Dr. Shute's success would be maintained when tested by large numbers, and during intense conditions of the epidemic. The testimonies in its favour, however, are quite sufficient to induce practitioners to give it a preference over every other method of treatment in collapse, and to adhere strictly to his suggestions with respect to those measures which should accompany the exhibition of cold water. It is not a little in support of Dr. Shute's opinion, that nearly similar ones were proposed by Mr. J. G. French, of St. James's Infirmary, Westminster, in a letter addressed to the Central Board of Health, at the same period, without his having had any knowledge of Dr. Shute's investigations.

ON PUERPERAL DISEASES.

To the Editor of the Medical Gazette.

Sir,

The accompanying translation of a paper published in the Austrian Medical Journal for 1837, if not too bulky for your pages, may, perhaps, be of some interest, as shewing the present state of opinion in Vienna concerning the nature of puerperal diseases.

The opportunities for observation in a hospital where from 4 to 5000 women are delivered annually, and in which, for many years, one or other of the more severe forms of puerperal disease has been almost constantly present, are, perhaps, greater than any other metropolis can afford. The mortality is, occasionally, very great; during the last summer from 50 to 60 deaths occurred in less than two months; and in the months of October, November, and part of December, when I frequented the hospital, 30 deaths occurred. The lying-in wards of the general hospital in Vienna are lofty, spacious, and tolerably well ventilated, and exhibit in the beds and other arrangements an average degree of cleanliness; they are, however, much too large for the purpose, containing each from 20 to 30 or more patients. Separate wards are appropriated to the sick, and to these they are removed immediately upon the appearance of serious indisposition. The treatment in cases of peritonitis is almost exclusively antiphlogistic; the internal use of mercury is seldom pushed to any extent, and when it causes purging, is generally suspended; the combination of calomel with opium being almost unheard of. Thus the constitutional effects of mercury are rarely brought into play: in two cases of peritonitis in which salivation was established, the results seemed to be beneficial, and recovery took place. The venesections rarely exceed from 8 to 10 ounces, but leeches are applied in vast numbers to the abdomen. A large issue is generally made on each thigh. The iodine ointment alone, or combined with mercurial ointment, seems to be very useful in promoting absorption of the exudation. In the later stages of uterine phlebitis, bark, in combination with ammonia, the sulphate of quina, and other stimuli, are employed, but with little if any advantage. In the earlier stages the mineral acids appear to be occasionally beneficial.

The examinations after death are conducted with extreme accuracy and minuteness by the professor of morbid anatomy (Dr. Rokitanski) and his assistants. The opinion that puerperal disease can cause death, without any perceptible change of structure having taken place, is not admitted by the Viennese pathologists; they are likewise opposed to the view of the contagious nature of the disease.

I have the honor to be, sir,
Your obedient servant,
Seth Thompson, M.D.

26, Duke Street, St. James's,
March 5th, 1838.

Beobachtungen uber puerperal krankheiten mitgetheilt von Theod. Helm, Dr. der Med. u. Chirur. und Assistenten am K. K. Gebärhause eu Wien.

Observations concerning puerperal diseases, however imperfect, are yet not altogether without value, inasmuch as they relate to a subject upon which sufficient light has not yet been thrown; and to contribute to the accomplishment of this, is the duty, and should be the endeavour, of every one to whom sufficient opportunity is afforded. Without occupying myself here with the question, whence it happens that such different diseases of childbed have been included under the name of puerperal fever, I shall endeavour to give a description of those which occur most frequently, and treat, as briefly as the subject will allow, of the morbid appearances, symptoms, prognosis, and treatment of each variety. In the sam manner that the different periods of life have their peculiar diseases, so also are there diseases which occur only to females during pregnancy, at the time of and subsequent to parturition; and it is only to those diseases which are peculiar to women in childbed that the term puerperal disease can correctly be applied.

The proximate cause of puerperal diseases consists, essentially, in the nature of the puerperal state. The change produced by parturition in the female system is for the most part so considerable, as completely to change her relation to surrounding influences both salutary and noxious; and thus those influences, which, under other cir-

cumstances, are innoxious, become morbid, or causes of disease. In lying-in hospitals many circumstances concur to favour the production of diseases, and thence it happens that in such institutions puerperal disease is of much more frequent occurrence than in private practice.

To enumerate all these would form the subject of a treatise on lying-in hospitals, or of a detailed history of the diseases of childbed; suffice it to say, that the patients received into these establishments are usually depressed by sorrow and care; that twenty or more, with their infants, occupy the same ward, which is entirely emptied and cleared but once in the year; that such patients have usually an aversion for their infants; that they are terrified by apprehension of disease, &c. But although puerperal disease is so easily produced in lying-in hospitals. and in large establishments of this nature is almost always in existence, it is in no-wise simply endemic, but is subject to epidemic influences, by which true puerperal epidemics are frequently generated. It might appear extraordinary that an epidemic should be confined to a particular group of individuals, but this objection vanishes on a closer investigation of the subject; for, as certain epidemics attack human beings, and spare the lower orders of animals, and, in some epidemics, adults, in others old people and children, are altogether exempt, why should it be denied that epidemic influences exist, of which only women in childbed, and more especially those placed in unfavourable situations, as in hospital, are susceptible? Why should not also certain diseases be more readily developed and modified in puerperal females by epidemic influences? Does not abortion, does not greater or lesser fertility occur, epidemically, in women? Experience does not fail to confirm these conclusions. Thus for a considerable period in a lying-in hospital there is but little sickness, and then, in a short space of time, without any apparent cause, many cases occur. The disease lasts for some months, and then without known cause, diminishes and altogether disappears. Were such puerperal diseases simply endemic, there would doubtless be a certain degree of fluctuation in the number of those affected; but it would not happen, that in one month three or four, and in the next fifty, should become victims of the disease. Moreover, the similarity of the cases which occur at the same period denotes an epidemic influence; thus during one epidemic there is almost constantly phlebitis and no peritonitis; in the next peritonitis is the prevalent form; and in a third epidemic, in almost every case after the existence of peritonitis for some days, acute pleurisy supervenes, which complication, at other times, is altogether absent, or of rare occurrence. Purely endemic diseases are not of such variable character; purely sporadic diseases, when occurring together, do not exhibit the same similarity of symptoms.

The more common primary forms of puerperal disease are,—1st, inflammation of the veins of the uterus, of its appendages, and surrounding cellular tissue (metro-phlebitis); 2d, inflammation of the mucous membrane of the uterus (methrymenitis puerperalis); 3d, inflammation of the ovaries (ophoritis puerperalis); 4th, inflammation of the peritoneum (peritonitis puerperalis); 5th, puerperal scarlatina (scarlatina puerperalis)—a peculiar form of puerperal disease, which has been altogether overlooked, or mistaken for common scarlatina.

Inflammation of the veins of the Uterus, of its appendages, and surrounding cellular tissue.

Upon dissection the veins of the uterus are found with thickened coats, and containing plastic lymph and pus; but these veins may be much dilated and filled with pus, whilst their coats are still uninflamed. When the disease has been of short duration, it generally happens that only those veins which correspond to the attachment of the placenta, or immediately surrounding this part, are thus filled; but when it has continued longer, pus is found in the veins both of the neck and body of the uterus, and also in those of the fundus uteri, in the broad ligaments, and in the fallopian tubes. The inflamed veins are surrounded by an exudation of lymph, in which they are imbedded in the substance of the uterus. Commonly, also, there is an infiltration of lymph or pus between the peritoneal covering of the uterus and the sub-peritoneal cellular tissue, which has either an uniform gelatinous appearance, or there are several cavities of variable

size containing pus. Both in the veins and cellular tissue there is frequently, instead of pus, a dirty-coloured, stinking sanious ichor (jauche) (phlebitis septica). Together with these appearanecs in the veins and cellular tissue, the uterus is frequently found shrunk, its tissue compact, dense, and of pale colour, its inner surface little vascular; or, on the contrary, it is twice or more its natural size, its substanee so soft as to be almost fluid, and containing much blood. The mucous membrane is also frequently inflamed, and appearances of peritonitis and ophoritis may be found, as these different forms of puerperal disease neither prevent nor cause the existence of each other.

These appearances of suppuration in the veins of the uterus are sufficient of themselves to account for the symptoms of any individual case; but they are not, especially after the disease has lasted some time, the sole morbid appearances, as we find other marks of disease caused by the primary affection of the uterine veins. Thus after long duration of phlebitis, there is often inflammation of the substance of the uterus, with condensation of the parenchyma, in consequence of infiltration of lymph, and more particularly around those veins which are filled with pus. In these spots the texture is very friable, frequently tending to become fluid, or sloughing (verjauchend). This inflammation occurs more frequently at the neck than in other parts of the uterus; in some cases there is on one side of the cervix uteri a swelling the size of a pigeon's egg, which, when cut through, appears to be traversed by veins containing pus. One or other of the spermatic veins is often found simply dilated and filled with pus, or its parietes are already thickened, and the inner coat lined with a layer of lymph: the same thing, but much less frequently, may be formed in the vena cruralis. It is less uncommon, however, to find the same appearances in the lymphatics which accompany the spermatic veins, and then also there is generally pus in the lumbar glands corresponding to these vessels. Also the substanee of one or both lungs may be pervaded by circumscribed spots of effused lymph or pus, which are found most commonly near the surfaee, and when seen through the pleura, have a reddish or pinky-white appearance. They are from the size of a millet seed to that of a hazel nut. (Pneumonia lobularis metastatica). They are compact, friable, of a yellowish tint, and often contain in their centre fluid pus. Less commonly these spots have not such defined limits, and run into a dirty black sanious ichor or slough (jauche), which has a peculiar gangrenous odour. (Pneumonia septica, gangræna pulmonum metastatica.) When such is the state of the lungs, we are sure to find similar ichorous matter in the uterine veins. Lymphatic and purulent infiltration occurs not unfrequently in the spleen, more rarely in the liver, and is of still rarer occurrence in the brain and kidneys; even in the heart and thyroid gland abscesses have frequently been found in these cases of metro-phlebitis.

In the external as well as in the internal organs matter is often found. The parotid glands are frequently considerably swollen: upon laying them open, pus or ichorous matter trickles from them. Very frequently there is suppuration within the different joints of the extremities, and most commonly in the elbow or shoulder-joint. In like manner there may be infiltration of the subcutaneous cellular tissue, and that which surrounds the muscles; more frequently of the extremities than the trunk. This is indicated by a more or less hard and tense swelling, which in some cases extends over nearly half an extremity, but is generally less widely diffused. Upon cutting into it the subcutaneous, or intermuscular cellular tissue, is found filled with pus or ichor (jauche), both of which substances, in other but less common cases, pervade the muscular tissue, which is thus converted into a soft pultaceous mass.

Course of the Disease.—Commonly on the second or third day after delivery, very rarely so late as in the third or fourth week, fever shews itself, sometimes preceded by shivering, sometimes not. Frequently at the same time pain of the abdomen is complained of, and upon examination the uterus is found to be the seat of this pain; it is often only upon examination that the patient feels pain, and sometimes no pain is felt under the hand. Both the pain and fever are frequently trifling; in other cases, however, they are very intense. The pulse also may be 140 or

150. The patient complains usually of headache, heat, and thirst. However, frequently, at the outset of the disease, the only symptom is increase of pulse, and if not sufficiently cautious in making a diagnosis, it is easy to confound the disease with ophoritis or peritonitis, which also may commence without marked pain, or with scarlatina puerperalis, or even the febris lactea: the latter mistake cannot last long. When thus phlebitis exists without local symptoms, it is only in its more advanced stage, and when the characteristic shiverings have taken place, that the diagnosis can be certain. Thus there may be for some days no other symptoms but fever and pain, and, indeed, pain is often altogether absent; but the existence of fever without any apparent cause must render us suspicious of latent mischief. The uterus is either large or small, hard or soft to the touch, the os uteri closed and not painful. The lochial discharge and milk continue to flow in the meanwhile well, the urine has no peculiarity of appearance, the skin is sufficiently moist, the appetite continues good, the countenance is as yet unaffected, the patient commonly without anxiety, and totally unaware of the existence of disease.

The further course of the disease now depends upon the supervention or not of peritonitis. In the first case the disease proceeds as peritonitis, the phlebitis recedes and disappears, as will be afterwards mentioned. Peritonitis supervenes more or less frequently upon phlebitis, according to the nature of the epidemic, phlebitis occurring at times quite alone, at others being constantly accompanied with peritonitis. When, however, phlebitis exists alone, there may be after some days, diminution of pain and fever, followed by their entire disappearance; or, the pain ceases and the fever continues, the appearance of the patient being as yet little changed; in the meanwhile the formation of pus in the veins proceeds rapidly, and the sudden occurrence of a more or less severe shivering fit, followed by great heat, alarms both patient and physician. A second and third shivering fit soon succeed the first, and the accuracy of the diagnosis is established. Not, however, that the shivering indicates the first commencement of suppuration in the veins; this may take place without any shivering, but the entrance of the pus into the general circulation appears to cause these phenomena of fever, by which the blood endeavours to free itself from the contamination.

There are few symptoms of disease which so decidedly confirm both diagnosis and prognosis as these shivering fits. Thus many a woman in childbed begins first on the tenth or twelfth day to become feverish, often still later; and were she not in childbed, one might infer the existence of febris gastrica rheumatica, or catarrhosa, but in such a patient all fever is suspicious. The characteristic shiverings, however, indicate only too soon the true cause of the fever. Generally there is no regularity as to the period of the occurrence of the paroxysm, so that often for a whole day there is an entire intermission, and then two or three occur in twenty-four hours; and again in other cases (which are less common), the symptoms have a true intermittent type. Such cases seem to have given rise to the denomination of febris intermittens puerperarum perniciosa. When the severe shiverings have once occurred, the disease proceeds uninterruptedly, with the peculiar metastases which shew themselves in dissection.

Of the metastases which occur internally, the greater number are hardly to be diagnosed. The more frequent and severe the paroxysms of fever [they commence often with a fearful shivering fit, which lasts fully an hour], the more extensive and intense are the different metastases. Thus inflammation of the lymphatics or veins may alone be suspected from the occurrence of these violent shivering fits, but metastases to the lungs, liver, or spleen, may also have taken place. The most easy of diagnosis is pneumonia lobularis. In some cases it is accompanied with bloody purulent expectoration and cough. That such small spots of hepatization escape detection both by percussion and auscultation, it is easy to understand. It is worthy of remark, that icterus and lobular hepatization frequently occur together.

The countenance of the patient, which at first was little affected, now denotes an important change—it sinks rapidly, and acquires an expression of deep suffering; there is a peculiar character of the features which might be termed the

puerperal physiognomy, as a variety of the abdominal physiognomy.

Although it is difficult during the life of the patient to determine the existence of the internal metastases, those which occur externally are easy enough of recognition. The metastatic parotitis differs from the more common forms of inflammation of this gland, by the sudden appearance of a tense circumscribed swelling, after short duration of pain and redness, which rapidly increases, and goes on to suppuration. Patients complain frequently of severe pains in the limbs, which are increased by every attempt to move them. This depends upon metastatic inflammation having taken place within the articulations, or in the cellular tissue surrounding the capsular ligament of the joint. After distressing pains in the extremities, a more or less considerable swelling, with well-defined limits, appears, which is tense and exceedingly painful, at first of a bright red colour, afterwards becoming paler. This denotes infiltration of the subcutaneous or intermuscular cellular tissue. The slightest touch cannot be endured, and often half an extremity is in this way affected. In less common cases the swelling begins gradually, becomes painful, of pale red colour, and after lasting some time disappears altogether. A great number of cases of the disease termed phlegmasia alba puerperarum appear to have been metastases of this description. The pain, the period of the appearance of swelling, its peculiarities, and the danger attending it—for these cases are almost constantly fatal—tend to confirm this supposition.

As a consequence of phlebitis a peculiar form of mania frequently occurs. It appears to be caused by the action of the blood, charged with pus, upon the brain, in the same manner that delirium potatorum results from the blood being charged with alcohol. This is preceded by much activity of the whole system of the patient; there is remarkable quickness of speech, &c. The attack of mania succeeds soon, and sometimes immediately, to these premonitory symptoms. The patients attempt constantly to escape, discourse in passionate terms of the most trivial and unconnected circumstances, or dwell constantly upon some particular incident or idea which has occurred to them during the last hours of sanity. They are sometimes furious, at other times the mania is of a gay character. The countenance is generally red and swollen, the looks wander, the pulse much quickened. Such paroxysms last commonly from five to six, rarely longer than ten to twelve hours. The patient either returns afterwards to entire consciousness, or lies, from exhaustion, in a half stupified state, from which it is difficult to rouse her. The attack rarely recurs. Upon examination increased vascularity within the cranium is all that can be found.

It often happens in cases of phlebitis, during the last days of its course, that there is a peculiar icteric discolouration of the skin and tunica albuginea. The yellow colour in these cases is exceedingly intense. Its first appearance is after the frequent occurrence of shivering fits, and it becomes more intense after each successive shivering fit. This symptom also appears to depend upon a diseased state of the blood; at least as yet nothing abnormal has been found either in the liver or biliary ducts; but lobular hepatization of the lungs almost constantly co-exists. Thus one or more metastases having taken place, the shivering fits recur with shorter or longer intervals, and the patient sinks with the symptoms of acute hectic fever. Some certainly do recover even when a metastasis has occurred, and this disappears during convalescence.

Prognosis.—The prognosis is generally unfavourable; even in the mildest forms of the disease the proportion of deaths is great, and in the worst epidemics four-fifths of the cases die. The more violent the outset of the disease, the worse the prognosis. When fever precedes the pain, the prognosis is more unfavourable than when the contrary occurs. Disappearance of the pain, with continuance, and perhaps increase of the fever, is a very bad symptom. Cessation of the fever, even when pain in the uterus remains, is of much better import. Frequent recurrence of the characteristic shivering fits gives a very unfavourable prognosis, and equally unfavourable is the occurrence of metastasis, especially when to the forearm. One of the worst symptoms is jaundice.

[To be continued in our next.]

HYDROCHLORATE OF MORPHIA.

To the Editor of the Medical Gazette.

Sir,

As hydrochlorate of morphia (muriate of morphia) is very largely used, and being at the same time a very powerful medicine, any thing respecting its properties, either in a medical or chemical point of view, will, I presume, be of interest to many of your readers. This salt is stated, in chemical works, to be *anhydrous*, and of course it is taken for granted by physicians that this is actually the case, and they prescribe accordingly; whereas, all the hydrochlorate of morphia sold contains a very large quantity of water, and, unfortunately, this quantity is not at all definite, but varies considerably in specimens obtained not only from different, but frequently from the same manufacturer, as the following experiments show:—

			Per Cent.
25 grs. lost	3·58 grs.	of water, or	14·33
25 —	2·92	——	11·66
25 —	4·01	——	16·04
25 —	2·95	——	11·80
25 —	4·50	——	18 00
25 —	2·40	——	11·40
25 —	2·30	——	9·20

The quantity of water in each experiment was ascertained by drying it over a water-bath at 212°.

The first two samples were obtained from the same manufacturer, some months elapsing between; the next three were obtained from different manufacturers, and the two last were some I recrystallized, and dried by exposure to the air, this being the mode of drying, I believe, generally adopted for this preparation.

As such discrepancies as these, in so powerful a medicine as the hydrochlorate of morphia, must be of the most serious consequences both to the physician and patient, I would recommend all chemists and druggists, on purchasing this salt, to insist upon its being dried at a temperature of 212° F.

The above being of some little importance, not only to the medical man, but to the chemist, will, I trust, be sufficient apology for my troubling you with it.—I am, Sir,

Your obedient servant,

S. A. Sandall.

St. Thomas's Hospital,
April 4th, 1838.

DILATATION OF BRONCHI.

To the Editor of the Medical Gazette.

Sir,

I am induced to send you the following cases of pneumonia with dilatation of the bronchi, from having just read, in one of Dr. Williams' admirable lectures now publishing in your journal, that this is not an uncommon complication, although pneumonia is omitted by Dr. Stokes, in his enumeration of the causes of dilatation, for the production of which, even in children, as far as his experience goes, the space of three months is required.—I am, sir,

Your obedient servant,

T. Ogier Ward, M.D. Oxon.
Physician to the Birmingham Dispensary.

Birmingham, March 20, 1838.

April 1st, 1836.—A. B., æt. fifteen months. About ten weeks ago she suffered from an extensive burn of the arms and neck, and since then she has never been very well. Three weeks ago she was seized with cough, dyspnœa, fever, &c., and died yesterday. On examination the lungs were solidified throughout, particularly their lower lobes, which were very friable. The trachea and bronchi were very red, and the latter, in the lower lobes, where the lungs were most inflamed, were deprived of their mucous membrane, and seemed excavated out of the solid lung. They were filled with tubercular matter in a softened state, that adhered to their ragged sides, and they were so much dilated as to admit a director to their very ends, close under the pleura; whereas, although dilated in the other less inflamed portions of the lung, they preserved their lining membrane entire, admitting the director but a short distance: here, also, they were filled with the same cheesy matter. Heart gorged with blood, firmly coagulated in both ventricles. There was a patch of recent lymph on the lower lobe of the right lung, just opposite the inflamed mark of a blister that had been applied for eight hours, two days before death.

In this case the degree of bronchial dilatation was evidently proportionate to the extent of the inflammation of the lung, which had even proceeded to the destruction of the mucous membrane.

This is the only instance in which I have observed ulceration of the more minute ramifications of the bronchi; but it closely resembled those chronic ulcerations of the larynx and trachea in which, except from its ragged and roughened state, it is difficult to determine where the attenuated mucous membrane ceases to exist. It is probable that the tubercular secretion into the bronchi, the lymph effused opposite the blistered surface, and the firm coagula in the ventricles, might be all manifestations of a diphtheritic tendency, and that the bronchial exudation, having become adherent and partly organized in the more inflamed portions of the lung, could not be scraped off without leaving the mucous membrane (already deprived of its consistence by inflammation) ragged and wasted beneath. This is not the first time that I have remarked that blisters, when suffered to remain applied too long upon the chest, have increased the inflammation they were intended to divert. That their effects extend pretty deeply is evident from the firm adhesions between the skin and subjacent parts, where blisters have been kept open, so often met with in dissections; and it has always appeared to me, that it is by the support they thus give to relaxed and weakened parts, rather than by their revulsive power, that they are of so much use in veterinary surgery.

The next case occurred in a boy, æt. 8, who had had a cough for some time, when he was attacked with measles of a malignant kind, the eruption being of a bright purple colour, and attended with vomiting, purging, delirium, and symptoms of inflammation of the lungs, from which he died in eleven days.

Sectio cadaveris 30 hours after death. — Body and limbs purplish; lining membrane of pharynx much wrinkled and puckered, and the upper part of larynx in the same state. Pharynx, larynx, trachea, and bronchi, of a deep red, but the mucous membrane not softened. Left lung engorged, particularly at the base, where it was very friable; and right lung still more so, and filled with miliary tubercles at its upper part. Bronchi generally dilated throughout the engorged and softened parts; but the right were not so deeply coloured as the left; they, with the trachea, were filled with thick white mucus. No disease of par vagum, phrenic, or sympathetic nerves; venous blood fluid; clots of lymph in both ventricles; left rather thickened.

In this case the dilatation of the bronchi does not appear to have arisen from any morbid state of the vessels themselves, as none was detectible except the change of colour; but it was probably induced by the yielding of surrounding engorged and softened tissue. Neither is it likely that the dilatation was the result of the previous cough, for besides the healthy state of the bronchi, they were only dilated in those parts of the lung that were diseased acutely, and not in the upper lobes, where they usually suffer this lesion, and in which the tubercles existed. I think, therefore, that we are warranted in concluding that this is a case of dilatation of only eleven days standing. Had the pulmonary disease any influence upon the colour of the eruption and fluid state of the blood, or was the blood the *point de depart?* I think the latter was the case, for the eruption did not shew itself for five days after the general and pulmonary symptoms, and three days before death it had become red.

The third case was also connected with tubercular disease of the lung, but depended upon acute inflammation:—

Feb. 27th, 1837. — Caroline Roper, æt. 4, was brought to me with swelled glands on each side of her neck, enlarged and aphthous tonsils, and with other marks of a scrofulous habit. She was very fat, but her flesh was flabby; abdomen large; cough and rapid pulse; appetite good, and general health not much amiss. The local disease was treated by leeches and astringents, and attention was paid to her general health, but without much effect. If she improved one week, she fell away the next, and the aphthous ulcers spread forwards and backwards till they lined the mouth with a yellow pellicle, which extended over the velum, uvula, and back of the pharynx. Her tongue was furred throughout.

March 24th.—The glands had subsided, and the throat and mouth looked much cleaner; but she had fever, sunken features, hot skin, pulse and respiration very rapid, with constant cough, or attempts to clear her throat of the saliva

and discharge from the ulcer. Her chest did not sound dull on percussion, but respiration was very loud and bronchial on the left side, with mucous rattling on the right. Diagnosis: hepatization on left side; suppuration or congestion on right. As she was very fretful I could not examine her chest again. She lived in this state four days, and her breath became horribly fœtid two days before death. Her voice had been extinct for some time.

Sectio cadaveris, 17 hours post mortem. — Mucous membrane of palate, fauces, and upper part of larynx and epiglottis, thickened and partly removed by ulcerations, that were covered with a dirty white sloughy matter. Slight inflammatory redness of trachea, increasing in depth towards the bronchi, which in right lung were dilated and filled with puro-mucous fluid in the inflamed portions of the lung. Both lungs generally engorged, and filled with tubercles in every stage, from the semi-transparent form at the base, to the crude and softened state at the apex, where cavities had already formed. Apex of the right lung was gangrenous round the cavities; and at the back and base it was infiltrated with pus, but it did not present the granulated appearance so distinct in the inflamed lung of the adult. The liver was healthy; mesenteric glands enlarged.

The same remarks are applicable in this as in the two former cases, with regard to the origin and seat of the dilatation, and it is needless, therefore, to repeat them. In all the cases the dilatation was general, the bronchi resembling the fingers of a glove. I would merely add, in confirmation of the opinions of M. Louis respecting the baneful influence of the tubercular diathesis on chest disease, that I have hardly had a single fatal case of pneumonia in which tubercles in some form were not present.

LARGE SCIRRHOUS TUMOR,

CONNECTED WITH THE LIVER BY A PEDICLE.

To the Editor of the Medical Gazette.

Sir,

Having read with much interest the lectures of Mr. Hawkins on Tumors, recently published in your journal, I am induced to transmit to you the following case (the notes of which have been recently sent me by my friend, Mr. Hugh Birt, M.R.C.S., of Storrington, in Sussex), and should it appear to you worthy of publication, I shall feel favoured by its insertion in the pages of your valuable periodical.—I am, sir,

Your obedient servant,

Gurney Turner.

General Dispensary, Aldersgate Street,
April 10, 1838.

Mr. Parsons, æt. 48 years, of a nervo-bilious temperament, and particularly abstemious habits from youth, applied to me, May 11, 1837, respecting a constant pain felt in the epigastric, right hypochondrial, and subscapular regions, accompanied by a sensation of weight and extreme distension, although no tumor was perceptible to the eye. He complained of frequent vomitings, eructation, and hiccough, and could not bear the slightest degree of pressure, not even that caused by buttoning the waistband. His skin was dry, and of a yellowish hue; the urine high coloured, with a whitish sediment; the tongue covered with a yellow fur in the centre, whilst its edges were of a florid red colour. The bowels were constipated; the fæces displayed absence of the hepatic secretions; his breathing was oppressed, and its odour very offensive; his pulse full and slow; the lower extremities œdematous, covered with an erythematous blush, and always feeling cold.

The treatment pursued was as follows:—Mercurials, conjoined with oleaginous purgatives, to improve the biliary secretions, and keep up the peristaltic action of the bowels, which were torpid; effervescing mixtures, with antimonials, blood-letting, counter-irritation, and muriatic pediluvia, for the purpose of increasing the capillary secretions, and diminishing the momentum of the circulation. I availed myself of the blisters, not only for the purpose of counter-irritation, but also to form a surface for the free introduction of the deutiodide of mercury, with a view to diminish the morbid increase in size of the liver, which at this time (July) became quite perceptible to the touch. Stimulant aromatics, with anodynes, were used also, for the purpose of allaying the pain, restoring the tone of the stomach, and rousing the patient's nervous sys-

tem. From the 15th of October he gradually sunk, and death put an end to his sufferings on the 11th of December. After his death, I learnt that two years previously he had a severe attack of jaundice, and since that time had suffered much from dyspepsia, and that his countenance had frequently presented an icteroid hue.

Post-mortem examination.—On opening the thorax, slight effusion was found; the pleuræ were healthy, and free from morbid adhesions; the lungs slightly congested, and tuberculated towards their apices. The pericardium contained a small quantity of fluid; the heart was normal, and its structure quite healthy, and the valves free from ossific deposit.

The muscular parietes of the stomach were thickened, and its mucous surface healthy in the cardiac portion; whilst, towards the pylorus, the rugæ were numerous and unnaturally large. The pylorus itself was very much diseased, its structure scirrhous, and a portion of the mucous membrane was ulcerated, extending in length from the upper surface of the pylorus into the duodenum. On cutting through the pylorus it displayed the well-known glistening and striated appearance of schirrus. The remaining portion of the intestinal canal was highly vascular, although perfectly free from disease. The liver was preternaturally enlarged, and the gall-bladder distended with black inspissated bile, with which the whole structure of the liver seemed engorged, save the lobulus Spigelii, which was denser than the other portions, and presented a laminated grey appearance.

The most striking feature of the case was a large tumor, growing by a pedicle from the "pons hepatis," and weighing two pounds and a quarter avoirdupois. This tumor was extremely hard, and of a fibro-cartilaginous structure; it rested on the pylorus, but no attachment existed between them, although the under surface of the tumor appeared to present a slightly different structure from the rest of the mass, but still not of the same nature as the disease pervading the pylorus.

The true scirrhous degeneration of the liver was well marked, and not to be doubted; and this is an interesting fact, as many pathologists (Mackintosh amongst others) have denied the fact of its ever taking place.

URINARY CALCULI.

To the Editor of the Medical Gazette.

Sir,

As no descriptive account has hitherto appeared of the valuable collection of urinary calculi in the museum of St. Bartholomew's Hospital, may I request the insertion of the following, should you deem it of sufficient interest to merit a place in your pages. For the opportunity of examining this collection I am indebted to Mr. Stanley, by whose care it has been almost wholly formed. The entire collection consists of 129 specimens, of which about one-third had not been examined: the composition of the others was pointed out some few years ago by Dr. Hue. It was, however, necessary to re-examine many of these, as from there not having been divided their internal structure was not described. In this, as in most other collections, chemical composition has been taken as the basis of arrangement. This plan would be sufficiently simple and accurate if the calculi were always homogeneous; but as by far the greater number consist of layers differing in composition, some additional method is necessary. In the present instance, the alternating calculi have been classed according to the number of layers which are present, and these are subdivided with reference to the composition of the nucleus. I am aware that many objections might be urged against this plan, and it would no doubt have been more scientific to have grouped together all those in which the layers observe a similar order of succession; but it was found that an arrangement of this kind would have introduced so many subdivisions as completely to destroy that simplicity which, in a museum continually subject to increase, it was necessary to preserve. The following table exhibits the relative frequency of each species, with the order of succession of the layers in the alternating calculi:—

Uric acid, nearly pure	11
Urate of ammonia, intimately mixed with variable proportions of oxalate of lime and the phosphates	8
Oxalate of lime, nearly pure	8
Phosphate of lime	4
Phosphate of ammonia and magnesia	1

Mixed phosphates	10
Ditto ditto deposited on foreign bodies	3
Cystic oxide	2

ALTERNATING CALCULI.

Uric acid—urate of ammonia	4
——— Oxalate of lime	3
——— phosphates	6
Urate of ammonia—uric acid	2
——— ——— oxalate of lime	7
——— ——— phosphates	13
Oxalate of lime—uric acid	3
——— ——— urate of ammonia	1
——— ——— phosphates	13

Uric acid—urate of ammonia—phosphates	3
———-oxalate of lime do.	1
Urate of ammonia—uric acid—phosphates	1
———-oxalate of lime do.	13
——— ———-ditto, uric acid	1
——— ———-ditto, urate of ammonia	1
Oxalate of lime—uric acid—urate of ammonia	1
Oxalate of lime	1
Calculi consisting of several layers	8

Uric acid. — Of these calculi the greater number has been taken from adults; the finest specimen, which is remarkably compact and crystalline, was extracted by Mr. Lawrence from the bladder of a man aged 72. Some few contain a little urate of ammonia, and in these a minute quantity of oxalate and phosphate of lime may be detected.

Urate of ammonia —In no table that has been given by writers on this subject, has urate of ammonia been regarded as forming an independent concretion, and its existence as such has been much disputed. This difference in opinion has arisen from the difficulty of separating that salt in an unexceptionable manner from the triple phosphate with which it is frequently mixed. The chief chemical evidence adduced in favour of its presence is the evolution of ammonia, when these calculi are heated with a solution of caustic potass. It has been contended on the other hand, that the ammonia is derived from a decomposition of the phosphate of ammonia and magnesia, or of urea accidentally present. When the former is the case, I am not aware of any method by which this objection can be set aside, for every plan which I have hitherto tried of separating these salts, so as to avoid their reaction on each other, has failed. There are, however, many calculi in which the presence of urate of ammonia can be satisfactorily shown, and which contain none of the triple phosphate, or in so small a proportion as to be inadequate to account for the quantity of ammonia combined with the uric acid; and as these calculi possess the same external appearances, and, in their general chemical characters, correspond exactly with those containing the phosphate of ammonia and magnesia, I think it is fair to infer that their composition is similar. Notwithstanding, therefore, the deservedly high authorities by which the contrary opinion has been maintained, I must fully concur in the observation of Dr. Prout, that all those calculi which present, when broken, an amorphous and earthy-looking fracture, consist essentially of urate of ammonia. That the ammonia in these calculi is combined with uric acid, may be shewn in the following manner:—

Let a small quantity of boiling water be poured over a few grains of the calculus, placed in a small paper filter. The solution will, on cooling, deposit a copious flocculent white precipitate of urate of ammonia, which, from its appearance alone, may be easily distinguished from the scanty crystalline precipitate which takes place when uric acid calculi are similarly treated.

Should too much water have been added, it will be necessary to evaporate the solution a little, before precipitation of the urate of ammonia will take place.

Free uric acid is very frequently present in these calculi, and may be observed in the form of minute crystals mixed with the amorphous precipitate of urate of ammonia, or adhering to the sides of the vessel. When, however, the triple phosphate is present, this test is of no value, for the mere affusion of hot water over a mixture of pure uric acid and phosphate of ammonia and magnesia, will give rise to the formation of urate of ammonia, and thus vitiate the result. This fact appears to add considerable weight to the opinion of Dr. Prout, that uric acid is seldom, if ever, deposited in a free state together with the phosphates. There is another distinctive character by which these calculi may always be recognized—viz. their decrepitating on the application of heat; in no well-marked specimen has this property been wanting, and I consider it as perfectly characteristic of the species. To what this peculiarity is owing it is not easy to determine; for

pure urate of ammonia does not decrepitate when heated; it has been generally referred to the small quantity of oxalate of lime contained in these calculi, but this is hardly probable, as oxalate of lime calculi undergo combustion silently, and the same property is possessed by those specimens in which the phosphates form the predominating admixture. It may possibly arise from the sudden extrication of ammonia, and its degree of force depend upon the compactness of the body; for in those calculi which, from the predominance of the earthy phosphates, are porous and friable, this property is considerably impaired, or altogether lost. As far as my observation has gone, the urate of ammonia in these calculi is never in a state of purity, all of them containing variable quantities of oxalate of lime, the phosphates, uric acid, and, in some few instances, of urate of lime. The quantity of earthy matter, however, in the compact varieties, is very small, seldom exceeding a few per cents. Fifteen grains of a specimen which was rather disposed to crumble, and in which the characters of urate of ammonia began to pass into those of the phosphates, gave on analysis—

Uric acid	9·1
Phosphate of lime	1·5
Phosphate of ammonia and magnesia	3·1
Animal matter, ammonia, and loss	1·3
	15·0

The ash which is left when these calculi are burnt, is almost always infusible, and alkaline; only in three cases were the phosphates in such proportions as to render it fusible. By reference to the table, it will be seen that urate of ammonia, so far from being rare, as is generally stated, forms, in the present collection, the most frequent primary deposit; as, out of 82 alternating calculi which have been divided, the proportion, as to nuclei, is as follows:—Uric acid, 18; urate of ammonia, 41; oxalate of lime, 23: and in those which are homogeneous, the proportion of urate of ammonia, though not so great, is still very considerable. It has been remarked, by Dr. Prout, that this species of calculus generally occurs in children; and the truth of this observation is fully borne out by the histories attached to these calculi, for although, unfortunately, they are not so perfect as to enable me to institute a strict comparison of the relative frequency of each variety at the different periods of age, yet, in the present case, by far the greater number are expressly stated to have been taken from persons under puberty.

Phosphate of lime.—Under this head are arranged some small calculi from the prostate gland, and three large irregular concretions from the kidney: two of them contained carbonate of lime and some urate of ammonia, the latter being apparently in separate layers. In one, a small quantity of the phosphate of ammonia and magnesia was likewise present: whether the phosphate of lime in these calculi was primarily secreted, or merely coated a nucleus of some other substance, is uncertain, as, on account of their figure, it was not considered adviseable to divide them. The other specimen was examined by Dr. Hue, and consisted of phosphate of lime, with a large quantity of animal matter.

The term bone earth, which is frequently applied to these calculi, is faulty, as it conveys the idea that the lime and phosphoric acid are in the same relative proportions as in the earthy matter of bones, whereas it has been shown by Dr. Wollaston that the calculi from the prostate gland contain a much larger proportion of acid, forming what is usually termed the neutral phosphate, or, more correctly speaking, the diphosphate. From several facts which I have observed, I am, however, convinced that the relative proportions of acid and base in the phosphate of lime surrounding other calculi, whether alone or mixed with the phosphate of ammonia and magnesia, varies considerably. Whether this arises from a mixture of two or more of the already known compounds of lime and phosphoric acid, or whether they are definite compounds of which we have at present no knowledge, I am unable to decide, although I believe the latter occasionally to be the case. In a calculus which consisted of urate of ammonia and oxalate of lime, surrounded by the mixed phosphates, was observed among the latter a layer, which had an imperfectly fibrous structure, and was much harder in texture and more compact than the rest. On digesting a portion of this in dilute

acetic acid, effervescence took place, and some lime was dissolved; the insoluble matter left had a crystalline appearance, and was found to be phosphate of lime. When dissolved in stronger acid, and the solution neutralized by ammonia, a gelatinous precipitate fell, which, after standing about four-and-twenty hours, was wholly converted into a number of small crystals, having undergone similar changes to freshly precipitated uric acid. If these crystals are left for a few days in the solution from which they have been precipitated, they gradually disappear, and are wholly converted into an amorphous precipitate, differing only from the former in not being quite so gelatinous. The nature of the changes which take place I am unable at present to explain, although I find that when the diphosphate of lime (prepared by dropping a solution of phosphate of soda into one of muriate in lime, the latter being in excess) is precipitated from its acetic solution, the same appearances present themselves; the conversion is, however, only partial.

The calculi which contain this phosphate usually partake more or less of the external characters before mentioned. In some of them it appeared to be mixed with the bone earth phosphate, properly so called. If it be identical in composition with the diphosphate, which I believe to be the case, the property alluded to is not noticed in any of the chemical works I have consulted. I am informed, however, by Dr. Prout, that he has remarked the same. Only in one instance have I seen the radiated structure noticed by Dr. Wollaston: it formed a thin layer among the mixed phosphates, surrounding a calculus of oxalate of lime, phosphate of ammonia, and magnesia. This specimen has not been divided: it probably contains a nucleus of uric acid, and therefore should have been arranged among the alternating calculi.

Mixed phosphates.—The calculi arranged under this division are composed throughout of the phosphate of lime and phosphate of ammonia and magnesia, mixed in variable proportions. Some of these contain thin layers of urate of ammonia, and this salt is frequently present in the fusible calculus.

The number of calculi of this description is rather above the usual average, as the phosphates seldom form the primary deposit. It is probable that some of these calculi were formed by the decomposition of urine which, from some cause or another, could not escape from the bladder. Such appears to have been the case in two of these specimens; one having been extracted by Mr. Stanley from a cyst which communicated with a fistulous passage leading from the bladder to the perinæum; the other occurred in a patient in whom, on account of an enlarged prostate, lithotomy had been performed above the pubis, and through which opening the urine was subsequently expelled.

Cystic oxide.—Of this rare species of calculus I had the satisfaction of finding two specimens. The largest weighed 740 grains: it was of an oval shape, measuring one inch nine-tenths through the long axis, and respectively one inch five-tenths and one inch one-tenth through the two short axes. When sawn through, it exhibited the confusedly crystalline structure characteristic of this species. The crystals radiated from the centre, their summits appearing at its external surface. These were not, however, sufficiently defined to render their form evident. It had a light-yellow colour, and its sp. gr. = 1·13. When burnt, it left a small ash, which was partially fusible. Ten grains, on analysis, gave—

Cystic oxide	9·	10·
Phosphate of lime	0	38
Phosphate of ammonia and magnesia	0·	10
Animal matter and loss	0·	42·
		10

This specimen is probably the largest and most perfect in the kingdom; it is supposed to have been taken from the bladder of a young man after death, in whom there had been, during life, no symptoms of any affection of the urinary organs. Unfortunately no record of the case exists. Of the other specimen the Museum possesses only one-half, which weighs 155 grains. It has a lighter colour, and its structure is not so decidedly crystalline as the other. The crystals appeared to diverge from three points. Its external surface was coated in parts by a thin layer of the fusible calculus. When burnt it left a very minute ash, which had an alkaline reaction.

Carbonate of lime.—This salt rarely

forms the principal constituent in calculi from the human subject, and no specimen of the kind exists in the Museum; it is, however, very frequently present in small quantities, and generally mixed with the phosphates.

Purpurate of ammonia.—Of this singular substance it is not easy to obtain decisive chemical evidence, partly on account of the small quantities in which it occurs, and partly on the facility with which it undergoes changes, by which its colour is destroyed. I believe I am correct, however, in stating that I have detected it in three instances In one it formed flesh-coloured layers, alternating with the phosphates; in the others it merely coated the calculus. In all of them it was mixed with urate of ammonia.

With regard to the alternating calculi, the table that has been given expresses nearly all that is worthy of notice. It may be observed, that in no one instance have the phosphates, either pure or in a state of mixture, formed the nucleus; indeed, this circumstance is so extremely rare, that it has been laid down as a general rule, by the highest authority on the subject, "that a decided deposition of the mixed phosphates is not followed by other depositions." There is, however, one specimen in the Museum which must be regarded as an exception to this statement. The calculus in question consists, at its centre, of urate of ammonia, containing a little oxalate of lime; around this is oxalate of lime nearly pure; a white layer three-eighths of an inch in thickness follows, and is succeeded by a thin stratum of oxalate of lime, of a very dark colour. Upon this is deposited crystalline uric acid, marked with the irregular concentric lines peculiar to oxalate of lime calculi, although it contains but a mere trace of that substance. The whole was coated by urate of ammonia, uric acid, and oxalate of lime, irregularly deposited. As in the Museum Catalogue the white layer was merely described as fusible, and as Dr. Prout (to whom, with the permission of Mr. Stanley, I had the pleasure of shewing this specimen) suggested that it might contain urate of soda, it was carefully examined for that substance, and the result was, that in addition to the mixed phosphates, with some carbonate of lime, a small quantity of uric acid and soda was present. The quantity of the latter was, however, very minute. Oxalate of lime could not be detected.

It is highly probable that, in this case, the deposition of the phosphates had been caused by the use of alkaline remedies, and that, on the discontinuance of these, the former diathesis had returned. If this were the case, it can hardly be considered as a fair exception to the law above mentioned.

By most writers on this subject a species of calculus has been noticed, consisting of the different ingredients mixed indiscriminately together, from which circumstance it has been termed mixed, or compound. The only specimens which appear to me to deserve this appellation are the mixed phosphates, and the less pure varieties of urate of ammonia. As, however, there is no calculus which is absolutely pure, and it would be exceedingly difficult to decide what proportion of the dissimilar ingredients should constitute a mixed calculus, this class has not been included in the arrangement. I may, however, remark, that with the exception of those layers which occasionally intervene between two different deposits, and which, as has been observed by Dr. Prout, usually consist of a mixture of the old and new layers, only two specimens have come under my notice at all approximating to the characters of the so-called mixed calculus, or in which the slightest hesitation occurred in assigning their proper place. Of one of these I have given the analysis, under the head of urate of ammonia; the other contained a much larger relative proportion of the mixed phosphates, and surrounded a nucleus of uric acid; it has, therefore, been classed with the alternating calculi.

Although, sir, I have endeavoured in the foregoing observations to confine myself to points of general interest, or on which some difference of opinion existed, yet as I am afraid they have already extended to an unwarrantable length, I shall no longer intrude upon your notice, merely requesting their insertion at your earliest convenience.

I am, sir,
Your obedient servant,
THOS. TAYLOR, M.R.C.S.

New Bridge Street,
April 11, 1838.

A MODE OF RELIEVING PATIENTS LABOURING UNDER ENLARGEMENT OF THE VEINS OF THE TESTICLE.

By Thomas Wormald, Esq.,
Assistant Surgeon and Demonstrator of Anatomy at Bartholomew's Hospital.

(*For the London Medical Gazette.*)

When cases of varicocele are allowed to proceed without any active means being adopted for their relief, the patients may experience much inconvenience from pain in the loins and spermatic cord, and frequently are incapacitated from walking any considerable distance.

P. W., aged 19, applied to me in the year 1832, in consequence of a circocele of very large dimensions, which had existed two years, and had been progressively getting worse.

The veins were distended to the size of a large apple; so much was he inconvenienced, that a walk of half a mile produced great pain in the back and spermatic cord. After a consultation with Sir A. Cooper, cold lotions, suspensory bandages, &c., having been employed without affording the slightest relief, Sir Astley recommended the removal of a portion of the scrotum. To this proceeding the patient would not consent. I therefore adopted the following mode of treatment:—

A ring, about an inch in diameter, made of soft silver wire, of a suitable thickness, was padded, and covered with wash leather. Through this I drew the lower part of the scrotum, whilst the patient was in the recumbent position, and the veins comparatively empty. I then pressed the sides of the instrument towards each other with sufficient force to prevent the scrotum escaping.

The use of this instrument every morning before the patient rose from his bed, enabled this gentleman to walk nineteen miles on the third day after the first application; and although he has for six years worn an instrument of this description, he has never experienced the least inconvenience.

And I may add, that other patients (and amongst them medical friends) labouring under varicocele, have found the greatest relief from this simple contrivance.

April 23rd, 1838.

MEDICAL GAZETTE.

Saturday, April 28, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

ON THE SCHOOL EXAMINATIONS.

As the Session approaches to its termination, we are reminded of a system which, though but lately introduced into the schools, we regard as one of the greatest improvements which has been made for many years in their scheme—we mean the establishment of examinations in the various classes, and the public distribution of prizes among the successful students. We believe that the credit of having first adopted this part of the plans of the larger Universities, in the medical schools of the metropolis, belongs to University College; but the readiness with which the improvement was received and followed in all the other principal schools, deserves almost equal approbation. Its value, indeed, must be evident to every one acquainted with the kind and quantity of work which was accomplished by students before and since the adoption of this system. A few years ago, the only standard which the student was required to attain, or which (except for conscience sake, or the satisfaction of an unusual desire after knowledge) it was worth his while to aspire to, was that which would enable him to pass creditably the examinations for the diplomata necessary for his practice. This standard, too, being of course adapted to the average, and a low average, of knowledge, there was too little stimulus for the exertion of the more talented, because there was no opportunity for exhibiting their superiority over their contemporaries. The case is now very

different: an opportunity is afforded to every student of obtaining considerable honour, by the demonstration of his having attained more knowledge than others who had enjoyed equal advantages. No standard being fixed, each is induced to strive after the highest acquirements that his time can possibly permit, by the fear lest his should come short of those of the others; and the consequence is, that the amount of information possessed by those who contend in the school examinations, is far greater than that which used to be possessed by those who could even most creditably pass those of the College or Hall.

In many points, however, the system is at present defective; and we say this, not with any intention of finding fault with those who administer it, for in one so lately established such success as this has met with is unusual, but that we may point out some important particulars in which it might be improved. In the first place, all the students should either voluntarily, or by compulsion, engage in the examinations. At present but a small portion of them contend in any of the classes, and in some there are occasionally not more than two or three competitors. This arises partly from the novelty of the system, and the dislike which the pupils entertain of being beaten, the disgrace of which they seem erroneously to think exceeds the honour of victory, forgetting that they are as much disgraced by not going in to the examination, and so tacitly admitting their inferiority, as by being positively defeated. This feeling, however, is not likely to cease at present; and the very great benefit of the system will remain proportionally limited, unless some greater inducement, or some method of compulsion, be employed to increase the number of the candidates.

The former object might be attained by increasing the value of the rewards held out—by making them more public and permanent, and at the same time more exclusive; and this would be best effected by giving honorary testimonials to none who had not engaged in the school examinations. At present, the system of giving certificates of qualification to candidates for public situations is utterly absurd. It is well known that any student who has not been most unusually notorious for idleness or ignorance, can obtain from his teachers testimonials of his fitness for almost any office he thinks himself able to secure: indeed, it often seems to be regarded as a *right*, which was purchased with the admission to the lectures or practice. To be sure, with those who know any thing of the plan on which these things are bestowed, the long list which every candidate for a dispensary or any other medical appointment presents, full of assurances from the optimates of the profession, that they "know no man more capable of discharging the important duties of the office he solicits," is only so much soiled paper; but the majority of the charitable voters in such cases believe all they read, and are only embarrassed to decide which of the "very talented gentlemen" they ought conscientiously to choose, and so, very frequently, the most meritorious is rejected. We know that it is a common plan for students to enter to the lectures at several schools, merely that the number of testimonials to be obtained afterwards may be greater, because many governors are in the habit of counting them, and voting for him who has most; and no wonder, for all being expressed in the superlative degree, no other mode of comparison is possible. Now, this most absurd and unfair system, which, with but a few honourable exceptions, is admitted by all the medical teachers, might be set aside, and the greatest benefit be done to the students generally, if it were strictly agreed that (except in

extraordinary cases) no such testimonials should be given to those who had not engaged and fairly acquitted themselves in the school examinations.

We fear, however, that this plan alone would not be sufficient to induce all the students to submit to them; for to many these higher honours seem to offer but little attraction, as the Council of the College of Surgeons have discovered, by the indifference shown to the reputation to be obtained by voluntarily passing the more severe examination, which they have made compulsory on future anatomical and surgical teachers—an indifference so complete, that we believe, though backed by the promise of special testimonials, &c., not a single candidate has yet availed himself of the opportunity of obtaining them.

It becomes a question, therefore, whether the school examinations should not be made compulsory. It is well known that the greatest evil of which the present examinations for diplomata of every kind permit the existence, is that of the system of grinding or cramming, as amongst many equally elegant synonyms, that of storing a pupil's mind with just the required quantity of knowledge (?) for one particular evening in his life, is called. A single examination, such as should be able to prevent this, and yet not require a higher amount of information than the average of students could fairly attain to, is yet, we suppose, a desideratum, for we know of no university, school, or college, in which grinding, in some form and extent, is not practised. Now we think that the school examinations might be employed to obviate this evil, the consequences of which have at all times been incalculably mischievous in our profession. Suppose, for example, it were required that before he could be admitted to examination for his diploma at the College, or his license at the Hall, each student should have previously passed at his school an examination in each of the subjects on which he is now only required to bring certificates of having attended lectures. In these examinations, let the lowest amount of knowledge demanded be determined by the Boards of Examiners, and let some one appointed by them, and unconnected with any school, decide whether or not it has been attained. But let the highest amount be, as now, limited only by the emulation and talents of the students, and let the teacher, or any whom he may choose to appoint, determine who deserve honorary distinctions, and in what order they should stand: of this last class a record should be kept, and made public; and the testimonials now valueless, because universally bestowed, should be given to no others, and should contain the statement of his place in his school examinations as part of the evidence of qualification of him on whom they are bestowed.

The least good that could flow from the adoption of this system would be that each pupil would be obliged to get up each subject twice in his life instead of once, an advantage which it would be worth ensuring at some expense of trouble. But we feel certain that much greater benefits would follow: there are few students who would not, for the school examination, get up as much as would be afterwards required for the diploma, that they might save the time and trouble of studying the same thing twice; for it would at once suggest itself to the mind of every one, that while necessarily engaged on the subject he might as well learn, once for all, as much as he should ever want. Now the kind of knowledge that he could remember for a year or more, and which would alone be of any use to him, is very different from that on which so many now rely for passing their only

necessary examinations; it could not be obtained by an unfair system of grinding, and it would be less likely to be forgotten in a month after it was required. Nor would the good stop here; a large majority of those compelled to obtain a certain amount of knowledge in every subject, would go a step further, and strive to obtain in some of them the honourable distinctions offered to those who passed a certain limit, and which are not only desirable to all, but are actually essential to some; and thus the number of those who would endeavour to acquire all the knowledge which their time allowed, would be increased to a most desirable degree: nay, we should expect that there would be scarcely one who would not then exert himself far more than under the present system.

Our space will not now allow us to enter into the exact details of the mode in which these examinations would be best conducted; on some future occasion we may recur to them: we would only say that they should be, as they now are, carried on principally in writing; that the lowest standard fixed should be a few degrees below that now required for the diploma, and that which should entitle to honorarary distinctions, about as much above it; and that the order in which the examinations were engaged in, should be left to the choice of the student.

We will only add, that in suggesting this plan for insuring a greater degree of *certain* knowledge in candidates for examination, and for rewarding the most meritorious students in the manner which they fairly deserve (an object which has never been fairly had in view), we are supporting no unnecessary or radical innovation. If the certificates now received by Boards of Examiners mean any thing, it is that the student who presents them, and whom they require to make a solemn assurance of their truth, has studied certain subjects deemed necessary, to such an extent as entitles him to his diploma. The school examinations would only be certain proof that he had studied them to some extent; they would be only evidence of what they are now required to give assurance, and would, we confess, be scarcely necessary if that assurance were always true—if it was not notorious that certificates do not always mean in fact what they express in words. Nor is the plan we have suggested new, or untried; it is but little different from that which every graduate at Cambridge is required to pass, before he can be admitted to his final examination: it will scarcely be denied that it would be most desirable if the perfection which the system of mathematical and classical examinations has nearly attained there, could be only in fair part reached in medicine. Some of the boards are, we find, gradually adopting those plans, and we earnestly hope to see, before long, this system of intermediate examinations, which forms a most important and essential feature in the scheme of that University, adopted in all our medical schools.

ACCOUNT OF ADDITIONAL MESMERIC PHENOMENA.

To the Editor of the Medical Gazette.

Sir,

With Dr. Elliotson's permission, and as the details may interest some of your readers, and as the attempt to state them perspicuously to others will enable me to see them myself in a truer light, I draw up and offer for insertion in your journal an account of some additional mesmeric phenomena, which were witnessed by several this afternoon at the North London Hospital.

I. One of the two sisters who have been the principal subjects of Dr. Elliotson's observations, being now in the state of sleepwalking, was seen to be capable, as usual, of being alternately

rendered comatose and restored to the state of somnambulism, by blowing upon her eyes. A pair of bellows was then used, instead of the breath; and her eyes were blown at through the bellows repeatedly, both while she was comatose, and while she was in the state of somnambulism. In neither case, however, was any effect produced, although blowing *once only* with the breath was at any time sufficient to change the mesmeric trance from the existing form to the alternative one.

When this patient was in the state of somnambulism, and so susceptible that a single motion of the hand rendered her comatose, a motion was made towards her with the bellows reversed; this was repeated several times; and although, being in front of her, it was seen by her, and the body moved was of considerable bulk, no effect whatever ensued. The instant afterwards, on waving the hand before her, she sank unconscious.

Her sister being in the state of sleep-walking, and in the highest degree of susceptibility, a large cat, with which she had before been playing, was lifted up at the distance of a foot from her, and then lowered before her, as the bellows had been in the last experiment. No sensible alteration was produced in her condition. But moving a single finger towards her directly afterwards, rendered her comatose.

The same experiment was repeated, a little child being substituted for the cat, and lifted, and lowered immediately before the sleepwalking patient; no alteration of her condition ensued.

So it appears that *the mental intention to affect the somnambulist, joined with the motion immediately before her, of a considerable mass, and that not only if dead matter, but if a living animal, or even a human being (itself not gesticulating), are insufficient to bring on the state of coma;* exactly as, in the preceding communication, it was shown that *the sleepwalking movements are not excitable by the motion of a mock hand guided by the operator.* The same principle is further confirmed by the following instance:—

One of the sisters being in the state of somnambulism, was desired to push with both her hands against the wall, which she did, with strong and repeated efforts, as she said, to push it down. No effect ensued. She was then desired, her hands being placed against those of her sister, to endeavour to push her away; the other sister opposed her, and for a few seconds they pushed against each other. They *then (each having directed a muscular effort against the other)* sank back comatose, having neither of them made half the exertion which was made by the first in pushing against the wall.

II. *An intention* on the part of the operator *is unnecessary to the production of coma in a mesmeric somnambulist; or coma will supervene when a motion is made towards the somnambulist by a person who has no thought of producing any effect on the somnambulist.*

The latter half of the preceding instance might be satisfactorily adduced in support of this conclusion. But the following is still more striking:—

One of the sisters being in the state of somnambulism, was desired to raise her hand with the palm towards her, and then to depress her fingers in a direction *towards herself.* There was some difficulty in explaining to her what was meant; but at length she understood the gesture she was to make, by seeing another make it. Upon trying it, that is, upon making towards herself the motion by which others render her comatose, and certainly with no idea of what was likely to ensue, or desire to produce it, she sank unconscious.

III. It may be further inferred from the preceding observations, that nothing is required for the production of the mesmeric coma or the phenomena of mesmeric sequence in *somnambulists*, beyond the muscular motion of a part either towards, or from, or in a direction crossing the party mesmerised, the same being *free motion, or motion independent of efforts made to sustain or move any substances supported by the moving part.*

The conditions which heighten this influence are, magnitude of the part used, rapidity of motion, and proximity. Of these conditions the influence of the first has been already exemplified.

The effect of any gesture in producing mesmeric coma in a somnambulist is, cæteris paribus, *in proportion to its rapidity.* When a finger is held up towards one of the somnambulists, and then moved downwards very slowly, she becomes, if before speaking, silent,

her voice ceasing gradually; if singing, her voice becomes fainter, and as if more distant, and then stops. She remains, as to her person, fixed cataleptically; she is capable, however, of self-support, being in a stupor half way only towards coma. Being awakened to the state of somnambulism, if the finger of the operator again is raised, and then moved downwards with rapidity, she drops into coma.

It is evident that the two conditions last adverted to, namely, the magnitude of the surface or part used to mesmerise, and the rapidity of the motion employed, admit of being conveyed in one expression, namely, *quantity of motion*. Again, it will be recollected that the common means of throwing a *waking* person into coma, is simply to hold the hand out, pointed to the patient. The influence of this means is greatly less than that of the hand in motion; if one may judge from its effect *on the somnambulists*, who often take several minutes of this operation to be thrown into coma, into which a motion directed towards them throws them at once. But the existence of this source of influence renders it necessary to modify the above expression still further. The influence exerted seems, *cæteris paribus*, to be in proportion to the quantity of free muscular action in a part that has the proper direction in reference to the patient.

So an influence is exerted when the fingers are simply kept pointed, more when the hand is moved, more when it is moved rapidly, more when the whole hand, than when one finger alone is moved.

IV. *The influence of any gesture in exciting mesmeric phenomena is* cæteris paribus *in proportion to the proximity of the operator to the party mesmerized.*

I saw Dr. Elliotson to-day excite mesmeric movements followed by coma at distances considerably exceeding sixty feet; and this, not only when the patient had her face turned towards him, but likewise when her chair was turned the other way, and she sat with her face to the wall, nobody at her side, or within the range of vision, and no one within several feet of her but a visitor, (who was at hand to prevent her falling from her chair). The effects, however, did not take place as instantly, two or three seconds intervening, and more gesture being necessary, before they ensued.

I had before witnessed sensations of pricking and muscular twitchings, which are sometimes the only mesmeric effects that can be produced upon persons who are sensible to this influence, excited at a distance of ninety feet, seemingly as promptly as when the operator was close to the patient.

It has been mentioned that most of the mesmeric effects are not prevented by the interposition of sheets of pasteboard, wooden screens, and doors; but they are to a certain extent interfered with by these means; and are less sure and constant, and more feebly pronounced, in proportion to the thickness of the intervening medium. The effect of the intervening medium, again, is most evident in the more delicate of the experiments narrated; when, for example, a small extent of surface, and very slow motion, are employed by the operator.

V. *Different individuals possess the power of exciting mesmeric phenomena in different degrees.*

A child of seven years of age, herself subject to epilepsy, was placed before one of the somnambulists, and instructed to raise her hand, and hold it out horizontally towards the somnambulist, and then to move it downwards, and to go on repeating this gesture. The child laughed, and so did the somnambulist at the experiment, in which they were to be the only parties. At first no effect was seen. When, however, the child had repeated the prescribed motion six or seven times, the somnambulist became dull and silent, and her countenance lost all expression: —she had been thrown into the half stupor recently spoken of. The motions were continued, and at each repetition we expected the somnambulist to become comatose. But instead of this, when the motions had been repeated nine or ten times more, the half stupor disappeared, and she returned to her previous state of lively somnambulism.

VI. *The power of producing mesmeric phenomena, proper to an individual, may be heightened through his contact with others.*

The last experiment was repeated, with this modification:—Dr. Elliotson placed his hand on the shoulder of the child who had just failed to render the

somnambulist comatose. The child then was told to recommence the movements it had before made. It had not repeated them more than six or seven times, when the somnambulist dropt forward in the state of perfect coma. At Dr. Elliotson's request another gentleman now placed his hand on the other shoulder of the mesmerizer; and as soon as the patient had returned to the state of somnambulism, the experiment was recommenced. Coma now supervened after three passes. Three other gentlemen were afterwards joined in the group, each taking hold of the mesmerizer; that is to say, there was the child of seven years old prepared to begin to mesmerize, and the hands of five adult persons had hold of its shoulders and unengaged arm. At the first motion which it made, the somnambulist dropt her head on her bosom insensible.

The child of seven years old that has been spoken of, has been recently admitted for epilepsy. The attempt to subject it to the mesmeric influence had as yet only produced drowsiness, followed by short naps, like common sleep, from which it has awakened just as a child ordinarily wakes from sleep. And this is a common first stage for those who afterwards exhibit the genuine mesmeric trance. The child had exhibited, however, one peculiar feature; on awaking from the common sleep artificially produced, for several minutes it appears to labour under a remarkable weakness, so as to be unable to support itself on its legs. Two new phenomena made their appearance upon combining this child *in a system* with the somnambulist sisters. The first was, that when it was in contact with the others, they being comatose, it could be thrown into its ordinary seeming sleep by a single gesture (whereas, when disconnected with them, some little time and a repetition of motions are necessary). The second new phenomenon was this:—The three holding hands, rendering one of the somnambulists comatose threw the new little patient *out of her state of common waking* into sleep.

I will conclude this brief account of the additional phenomena which presented themselves to-day, by describing one that far transcends the most marvellous that had yet come before me.

It has been mentioned that in the state of somnambulism Dr. Elliotson's little patients move their hands and feet, and their entire persons, in the direction in which the operator move his hand, and sways his person. But al the variations which I had witnessed o this experiment did not prepare me fo the following:—

Dr. Elliotson sat down behind one o the little somnambulists, who was sitting like a Persian, on her legs, towards th foot of her bed, and conversing witl two or three who were before her. Dr Elliotson's head was directly behin her's, and at the distance of a few inches, and he looked towards us, as sh did. He then made a grimace, draw ing his mouth towards the right side This is a very considerable muscu lar exertion; for it involves the motion of the lower jaw, the large muscles o which are in action to carry it to the same side to which the mouth is drawn *The little patient simultaneously made the same movement of her face towards the same side.* When Dr. Elliotson varied the experiment by a motion o his face made while he fronted the somnambulist, she made the same motion but now on the reverse side to his—that is, on the side of her face directly opposite to that on which his features moved; the left side of her face moving to his right. Of many other motions of the face, made, as the first, behind the patient, some failed to produce any motion in the child's face; none were followed except by a similar one.

The observations of which this is a summary, were made in a ward of the North London Hospital, seven or eight gentlemen being present, between the hours of five and seven this afternoon. They were repeated many times, and with every variation that it occurred to Dr. Elliotson and the others present to suggest, so as to leave no doubt on my mind that the phenomena which have been narrated were not accidentally connected with, but really dependent on, the conditions to which their production has been here ascribed. It would be idle, in these early days of the philosophic investigation of this extraordinary subject, to frame conjectures as to the probable limits and direction of its speculative and practical bearings. Time will not fail to show them—I am, sir,

Your obedient servant,

HERBERT MAYO.

19, George-street, Hanover-square,
April 24th, 1838.

CLINICAL CLERKSHIPS.

To the Editor of the Medical Gazette.

Sir,

In offering some remarks upon the subject of medical and surgical clinical clerkships, I would have it to be distinctly understood, that my object is not to find fault with the manner in which they have hitherto been conducted, but to inquire into the possibility of their being better managed, both for the benefit of the clinical clerks themselves, the hospital pupils generally, and all members of our profession.

Any person in the slightest degree acquainted with the subject will be aware that the time which is necessary for the investigation of a disease in order to its treatment, bears no comparison with the time that is necessary for compiling a calm unbiassed statement of all the facts connected with its history. To say nothing of other difficulties, a hundred negative and positive symptoms are almost instantaneously conveyed to the mind of the experienced practitioner, through the medium of sight and of touch, which alone it would be a work of some time to commit to paper; it cannot, therefore, reasonably be expected that hospital physicians and surgeons should give up the time which would be requisite for taking notes of all the symptoms attendant upon every case which may fall under their care; but when it is considered what an immense mass of useful evidence might be gained from such carefully collected histories, the importance of the office of clinical clerk will not be thought slight; and any endeavour to point out a better mode for the regulation of such office may not be deemed unworthy of consideration.

I would beg with great deference to suggest, that no pupil should be allowed to become a medical or surgical clinical clerk until he has diligently attended for some time (six months might, perhaps, be deemed a sufficient period) to the practice of medicine or surgery; and as examinations seem the order of the day, he might, in addition, be made to undergo some examination as a test of his competency for such an office; that the office should be generally understood to be one of merit and distinction; perhaps even some of the numerous prizes which are now so generally bestowed upon industrious and deserving pupils (at most schools of medicine) might not be ill bestowed upon such of the clinical clerks as may have distinguished themselves by their zeal and industry in performing the duties of their office; that the clinical clerks should have early information of the admission of each new patient, and that they should take notes of every case admitted (I say every case, because those often which appear least interesting and instructive in the commencement, prove most so in their progress and sequel); that either each clinical clerk should be required to conduct the post-mortem examinations of such of his cases as may prove fatal, or at least to be present at them, and to take full notes of all the appearances found on dissection, not always confining himself to those viscera which either really are, or are suspected to be, the seat of disease.

I would further suggest, that the clinical registers thus kept should be considered the property of the medical officers, but should be kept in the hospital library (and most of our large hospitals have a library attached to them) for the benefit of the pupils; it being distinctly understood that no gentleman should have any right to make public any of the cases, unless by the especial permission of the medical officer under whom they occurred.

Many arguments might be urged to shew the great importance of the clinical clerkships, but I am unwilling to trespass further on your valuable pages at present; I may, perhaps, say something more on this head at some future period.—I remain, sir,

Your obedient servant,

Philomathes.

London, April 9, 1838.

MR. WAKLEY AND THE IRISH MEDICAL CHARITIES BILL.

To the Editor of the Medical Gazette.

Sir,

How Mr. Wakley can reconcile his opposition to the Medical Charities Bill for

Ireland, without specifying the grounds of his hostility, must be rather perplexing to many of his medical brethren, not only in the sister kingdom, but in Great Britain also. More or less Mr. Wakley has been advocating a *principle of superintendence* and control ever since his journal has been established; it has been his "meat indeed, and his drink indeed." But when a motion is brought before the legislature for the furtherance of the above-named objects, he not only does all he can to retard its progress, but to bring it into contempt.

No doubt but the present bill has some defects, which might be, and will probably be remedied in its further progress through the House; but to offer opposition for the mere sake of opposition, is what few of the wellwishers to the profession will admire.

Mr. Wakley is a talented man, and has done much good to the profession; and may, and will, I hope, do more; but his writings and his speeches are often intemperate and ill-advised; and were he to be less talkative in his place in the House, would do more good, and gain the confidence of his medical brethren in a higher degree, than by fifty phillipics against a bill which has, after all, much in it that is likely to prove a real service to the profession in Ireland.

I am aware, Mr. Editor, that our views in many matters are widely different, but I trust to your candour for a place in your journal for the present letter, because, in the first place, the Lancet would probably not be open to strictures such as I now put forth; and, secondly, because I do not wish to run the risk of a refusal.

Mr. Wakley, from the prominent situation he holds, carries with him great weight and influence, and is much looked up to by a great portion of the profession; but it must be allowed that in some instances he is in error, in attempting to deprecate what cannot but prove beneficial, if rightly administered; I mean, of course, the bill which I am now more especially discussing; and without hesitation say, that if any thing ought to be done, it is to be done in a manner similar to what Mr. French has proposed.

Can Mr. Wakley deny that some method of control or inspection, or call it what we may, is not called for? Has he not been continually, in the pages of his journal, writing in favour of it? And can any man say that a measure framed in a right spirit is not likely to promote the welfare of the public, and faculty too? How can we so readily put an end to the system of quackery now so rife amongst us with druggists and advertising mountebanks, as by men immediately under the sanction of government taking official cognizance of such proceedings, and reporting in the proper quarter the injury thus sustained both by the profession and public? And how can an end be put to the vile system of traffic which now prevails amongst us, with respect to hospital and other appointments, if we have not some such plan as the "Medical Charities Bill" for Ireland contemplates? I deny, in the name of my medical brethren, the truth of Mr. Wakley's statements. I am satisfied that the present bill is likely, with certain modifications, to be highly useful and beneficial; and I only hope that a similar measure will be introduced for England—where as much requires to be done, or nearly so, as any where else. Look at the jobbing and purchasing of place; look at the infringement which the private practitioner is continually exposed to; see the swarm of irregulars which set down next door or so to him, and take the bread out of his mouth with impunity, and without any prospect of redress on the part of those who have worked for years and spent hundreds of pounds to get a respectable livelihood.

It may be all very well for Mr. Wakley to object to this and the other thing; but why, if he is so earnest in advancing the interests of the profession, does he not bring forward a bill himself for remedying such abuses? Not a bill, however, framed in such a spirit as has no chance of passing the legislature, but a temperate and judicious one, such as would confer a substantial boon upon those who need it.

Whether Mr. W. is sincere in all this opposition I cannot pretend to say; but if he is, I know not how to reconcile it with his vaunted liberality; for that something is needed all parties allow; and I know, that if a proper system of medical superintendence be established, and that superintendence be rightly and wisely acted out, it will prove not only a bar to the iniquities in high places, and the numerous other drawbacks which the private practitioner is exposed to, but will prove an invaluable blessing to the profesion generally.

Lastly, why does not Mr. W., if he can spare the time, consult with Mr. French, and get the obnoxious clauses of the present bill expunged; and further, something at least in the shape of relief to his brethren. Hoping the above will be received as intended,

I remain, sir,
Your most obedient servant,
PHILO.

MEDICAL CLUBS.

To the Editor of the Medical Gazette.

Sir,

Although much has been written on the subject of "medical clubs," and more on "medical relief," as administered under the authority of the Poor-law commissioners, I am yet induced to trouble you with a few observations, which have emanated chiefly in consequence of the perusal of an excellent letter, by "Theta," published in the Medical Gazette of the 14th inst.

Every surgeon in the Bradfield Union, it is true, is compelled to establish a "medical club" in each district that he undertakes. This circumstance alone must be exceedingly humiliating to the feelings of every respectable and well-educated member of the medical profession. But when I add, that he is likewise called upon by the functionaries of that union to frame his club upon terms similar to the following, the public may easily imagine that to him his contract must be entirely useless and profitless, and to the medical profession both arbitrary and oppressive.

"The contracts of the Bradfield Union medical club to include medical and surgical assistance to the agricultural classes in cases of sickness or accident, by their own small periodical payments during health; midwifery alone excepted. Each single member will be required to pay the sum of *three shillings* per annum; man and wife, without family, *four shillings;* with family, *five shillings*. Individuals above 16 years of age, to enter as single members, at *three shillings* each"

Notwithstanding the very degrading terms offered in the above contract, I understand that there were *nine* applicants for the appointment of medical officer to the districts of the union, most of whom produced good testimonials as to their qualifications, &c. &c; but the election terminated in the re-appointment of the resident medical men.

It becomes, however, a question as to the most prudent course to be adopted by the resident surgeons, under this dreadful system of insult and oppression; but, although an established practitioner myself, I confess that I do not place that implicit confidence in the public, to which your correspondent alludes in his excellent letter; for I know, to my cost, that many there are, and opulent families too, who may profess much kindness and friendship towards their accustomed medical man, (especially at the period that they may find the most need of his important services), yet they frequently think nothing of wounding his feelings, should it suit their own convenience and pockets, by sending for a nearer surgeon, provided he be at hand, to attend their establishment, particularly in sudden and emergent cases, and eventually the services of the long-resident practitioner are altogether dispensed with.

I attribute to this circumstance *alone*, the desire and inclination that is manifested by the resident surgeons to continue their parochial districts, even under the most disadvantageous circumstances.

At this moment there are numbers of established medical men of the first respectability, compelled (if I may use the term) to undertake parochial appointments, who have a deep interest in their profession, but who know too well that a deeper interest lies in the welfare of themselves and their families.

Although much has been attempted to ameliorate the present system of medical relief, by parliamentary petitions, by associations, and combinations of professional men, I yet hope that the medical profession may be protected from those disgraceful attacks to which crude legislation may have exposed it.

A Country Surgeon.

April 19, 1838.

PHYSICAL SOCIETY, GUY'S HOSPITAL.

April 7, 1838.

Dr. Guy in the Chair.

Mr. Gorham read an essay containing some remarks

On the Respiration of Infants in Health and in Disease;

the object of which was to point out some peculiarities in the subject, which had not hitherto been sufficiently investigated; and great stress was laid on the number of the breathing in health, by which, the author stated, accuracy could be obtained, and disease better understood.

Tables of the number of the respirations were added; and the conclusions to which Mr. Gorham had arrived were to the following effect:—

1st, That the normal respiration during the first few weeks of existence is quicker than at any after period of life.

2d, That this, however, is subject to great and appreciable changes, from alterations in the condition of the infant; and of these, the most remarkable is that of the sleeping state. Thus, during the waking period, and when the infant is in the horizontal posture, the average number of the

respiration is 58; but during sleep it amounts to 41 only, a difference of 17 being found to obtain.

From the second month to the end of the first year the numbers indicating this difference are expressed thus:—Sitting, 47; asleep, 32; difference, 15. No observations having been made on infants who were either horizontal or standing at this age.

3rdly. The respiration is quicker while in the sitting than in the erect posture; thus, during the second year, if the child be sitting, the respiration is 47; if standing, 38; and if asleep, 26.

During the third year the numbers indicating this change from change of position are as follows:—Standing, 30; sitting, 33; asleep, 22; and during the fourth year thus:—Standing, 27; sitting, 30; asleep, 25.

4thly. Precisely the same results obtain under the several conditions mentioned, as the infant proceeds in growth.

5thly. The effect produced on the number of the respirations during sleep is much greater as the individual is younger; or in other words, the change produced is in an inverse ratio with the advance in age.

The author reduced all these matters to a demonstration by an original chart, which he had drawn up from experiments.

Some other matters connected with the respiration in *health* were then described. These, the author stated, although known to many experienced men, had never been accurately pointed out before. One was very important, when considered in reference to the diseased condition: he alluded to that wonted irregularity which obtained here, in common with many other of the functions of the infant. In the diseased condition, the respiratory function, however, was carried on with great regularity. During the first few weeks of existence, as also in the sleeping state, the respiration was generally very regular. This Mr. Gorham explained by stating, that in these conditions the actions going on in the body are chiefly spinal or organic. The author lastly noticed the changes which occur in respiration in disease: he spoke of bronchitis, hydrocephalus, pneumonia, and snuffles*, and concluded by expressing a conviction that more extended observations would render the state of respiration a valuable pathognomonic sign.

In the course of the debate which followed this paper, Drs. Ashwell and Whiting, and Mr. Roberts, expressed their dissent with the opinions expressed by Dr. Marshall Hall, at a former meeting of the society, when the gentleman had declared laryngismus stridulus to be a disease admitting of easy cure, and generally produced by reflected irritation. It was the opinion of Drs. A. and W., and Mr. R., that the disease had its seat in the brain, was always obstinate, and frequently fatal in its character.

Thanks being voted to Mr. Gorham for his valuable communication, the meeting adjourned to Saturday, April 21st, when Mr. Coates will read some cases of injury to the brain. Mr. C. A. Key in the chair.

* In bronchitis the pulse was quickened, but regular: in hydrocephalus very irregular, varying from 18 to 60 in a minute.

HOPITAL DE L'ECOLE.

The three following cases occurred among the patients of M. P. Dubois:—

Case I.—*Spina Bifida.*

About three weeks ago, a child, labouring under spina bifida, was born in the wards of the midwifery *clinique*. The mother enjoyed good health, and had not experienced any thing during her pregnancy to intimate the rise of this disease.

The spina bifida showed itself as a tumor, situated at the lower part of the lumbar region, and covering the greater part of the sacrum. The tumor was red, soft, and of the size of an egg. With this exception, the child was of a good constitution; its functions were regular; the legs moved perfectly well, and they had the same sensation as other parts. Besides the division of the vertebral column, a spina bifida makes us suppose an accumulation of fluid in the part where this division exists; but though these two circumstances are very often united, they may also exist separately.

In this child, death took place in an unusual manner. The common way is, that the tumor inflames, ulcerates, and becomes perforated, so that the fluid runs out; and, the air entering, the lining membrane of the tumor is inflamed. This inflammation is propagated by continuity in the cavity of the membranes, causing acute pain, and attacks which are almost convulsive.

In this case nothing of the kind happened; but there was progressive emaciation, and the child seemed to die of inanition. The interior of the tumor was examined: it partly lay open by a small hole, and had already sunk down at the moment of death.

Behind the tumor there was a division of the spinous apophyses of the vertebræ. After opening the spinal canal posteriorly, the dura mater was first removed, and then the parietal lamella of the arachnoid,

which was not adherent. On coming beneath the visceral lamella, a considerable quantity of pus was found between it and the spinal marrow. This pus was also present in the fourth and in the lateral ventricles.

Now if we call to mind that the child almost died of inanition, it seems very singular that it should have expired in this state. The inflammation, no doubt, spread from the tumor to the place where the pus was found; and, in fact, a probe, by passing from the tumor into the sub-arachnoid cavity, showed that this communication had taken place. The spinal marrow was bathed in pus.

The child had neither suffered paralysis nor coma.

CASE II.—*Mammary Abscess in a Woman recently delivered.*

A woman in one of the clinical wards had an abscess in the left breast; it was opened, and first pus flowed out, then milk and pus. M. Donné examined the fluid with the microscope, and found that it contained both milky and purulent globules.

This case may lead to important deductions; for it shows the existence of a communication between the reservoir of pus and the lactiferous tubes, which in this instance, indeed, appeared only from the pus flowing out mixed with milk; but this communication might possibly take place in a different manner, so that the pus passing out through the nipple should be sucked by the infant.

Some facts might be easily explained by this circumstance. M. Dubois has observed, that when women are attacked by inflammatory symptoms, the infants whom they are suckling become weaker, pale, sickly, and afterwards sink under erysipelatous inflammations.

It is not that these inflammations do not occur except among children exposed to these causes; but there has been such a coincidence between the two facts, that it is impossible to avoid viewing them in the light of cause and effect.

M. Donné's microscopic observations afterwards confirmed this supposition.

CASE III.—*Phlegmasia Alba Dolens.*

A dressmaker, aged 25, was delivered on the 12th of January, and there was nothing unusual in the labour. She tried to suckle her infant, which was small and weak, but, either from weakness or want of experience, she was not able to go on with the attempt, and the child died of cold and hunger in the ward*.

She was much exhausted while endeavouring to suckle the child, and perhaps took cold. However this may be, on the 20th of January she felt pain about the left crural arch, which, she said, extended to the thigh. These pains had lasted several days, when, on the 25th, she complained of more acute pain in the calf; which was swelled, hardened, and very sensitive. The treatment was confined to the application of poultices.

On a more accurate examination, moreover, it appeared that pressure was painful on the left iliac fossa, below the crural arch, and along the course of the vessels of the lower extremity.

The fever was not violent, and re action was hardly perceptible. The swelling afterwards extended, and in two days occupied both the thigh and the leg; at the same time there was neither redness in the parts nor induration along the course of the vessels. The finger left no trace of its pressure. It was now an undoubted case of phlegmasia alba dolens. On the 26th, thirty leeches were ordered to be applied to the upper part of the thigh. Nineteen of them took well; yet the pain at the top of the thigh became so acute that slight syncope followed. The next day the patient was a little better; but on the morrow, the pain still continuing, leeches were again applied. Twelve grains of calomel were prescribed, and the patient had one stool.

On the 30th the pain returned, so that leeches were again ordered. This time the relief procured was well marked, and on the 31st the disease had begun to decline. The thigh was smaller and less tense, the pain not acute; and the reaction, which had never been strong, was diminished.

In this state the patient might well think herself cured. But, on the evening of February 4th, she complained of pain in the lower part of the abdomen, towards the right iliac fossa, and twenty-five leeches were applied to the spot.

When she was seen on the 5th, the pain was very acute at the bend of the groin, and along the crural vessels. The slightest pressure made the patient cry out. The pulse was 100.

The disease proceeded on the right side, as it had done on the left, but with this difference, that the reaction was more intense than it had been the first time. The treatment was nearly the same as before, consisting of leeches (which were applied three times), emollient cataplasms, and purgative clysters. In addition, a liniment, composed of an ounce of oil of sweet almonds, and the same quantity of Rousseau's laudanum, was used to rub the limb, beginning two days before the use of the poultices.

* The majority of these poor litt e creatures share the same lot in the *Clinique*: this arises, in part, from the inadequate number of wet-nurses kept there by the government.

The limb now begins to lessen; the pain, nevertheless, continues, but is not so acute as on the preceding days. According to all appearances, the disease will terminate as it did on the left side.

What is the cause of this affection? The œdema, the local infiltration, and the effusion, arise from a disturbance of the circulation. In this case, the local œdema arose from a disturbance in the circulating system of the thigh. The crural vein was inflamed; the disorder being propagated from the vessels above. The inflammation took its origin in the uterine vessels, thence spread to the hypogastric vein, then to the external iliac, and thence to the femoral vein.

This is shown by the disease being always the result of an inflammation. Besides, pressure was always painful along the whole course of the vein; and the pain complained of by the patient followed this direction. Indeed, it has been demonstrated of late years, that this affection depends upon inflammation of the crural vessels.

Before the patient complained, she had felt pain in the left iliac fossa, and it afterwards increased. But late researches have shown evident traces of inflammation in the uterine, hypogastric, and other veins. Hence, the crural vein was the seat, not of a primary, but of a secondary inflammation.

The prognosis in phlegmasia alba dolens is not serious. When the leg has recovered its functions it will still remain a little swelled, but this will gradually go off. It is probable that the disease in this case will last a week or two longer. The treatment has been antiphlogistic.

In this patient the disease showed itself a week after delivery; there was no evident cause, unless we refer it to taking cold, of which there is no proof. Generally speaking, the disease appears towards the second or third week after delivery. We then most frequently find that the symptoms of local inflammation in the pelvis precede those of inflammation of the crural vessels. In this case the first appearance of swelling on the left side was in the calf; in general, it begins in the thigh, and thence extends to the lower parts; which was the case here with the right side. The reaction is usually stronger than it was in this patient. This disease may produce death; but when this is the case, post-mortem examination shows that the affection had been preceded by an inflammation of the abdomen, or of the symphyses of the pelvis; and this explains the death better than the mere existence of phlegmasia alba. The œdema sometimes continues several years after the disappearance of the acute symptoms, the inflammation having produced a permanent disorder—an obliteration, namely, of the veins of the limb.

M. Dubois does not consider phlegmasia alba as confined to women in child-bed: he says, that it occurs in women who have not been delivered, where it arises from ulceration or inflammation of the cervix uteri, or of the uterus itself. Men, too, may be attacked with it, especially if labouring under an inflammation of the hemorrhoïdal veins*.—*Gazette des Hôpitaux*, Feb. 27, 1838.

CONDITION OF VESSELS IN PHTHISIS.

M. Guillot has determined that in phthisis the pulmonary arteries and veins are obliterated near the cavities, or large collections of tubercles, and that there is always a corresponding and compensating enlargement of the branches of the bronchial arteries going to those parts, or of those of the intercostal arteries which pass through adhesions between the surfaces of the pleuræ.—*Comptes Rendus des Seances de l'Academie*, No. 2, 1838.

ON THE NITROGEN IN PLANTS.

M. Boussaingault analyzed, on a large scale, and with the greatest possible care, quantities of wheat and trefoil, at different periods of their growth. He kept the plants for two or three months under glass bells, air tight, but filled with atmospheric air, constantly replenished and carefully washed, in siliceous earth, watered with distilled water.

On comparing the quantities of nitrogen which each contained on being put into, and on being removed from the earth and from beneath the bell, he found that the trefoil had gained a large amount of nitrogen (of course, from the air), but that the wheat had remained just the same, as regarded the proportion of nitrogen which it contained. He thinks, however, that at an earlier period of its growth it too abstracts some of that gas from the atmosphere. Hence he accounts for the production of nitrogen in such large quantities in herbivorous animals—a subject on which he promises to throw considerably more light in a special work which he is about to publish.—*Comptes Rendus des Seances de l'Institut*, No. 5, 1838.

In the same work, M. Payen shows that the quantity of nitrogen in each organ of plants diminishes as they become developed. The cambium, he says, contains an abundance of it, in a substance which is removeable by water.

* Two cases of phlegmasia dolens occurring in men are given by Sir Henry Halford in his Essay on this disease (Essays and Orations, 2nd edition, p. 112, *et seq.*)—*Translator's Note.*

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns, April 24, 1838.)

	Price.			Duty.		Duty Paid	
						In 1838 to last week	Same time last year
	£ s. d.		£ s. d.		s. d.		
Aloes, Barbadoes, D.P. c	12 0 0	to	30 0 0	B P. lb	0 2	33,161	31,165
Hepatic (dry) BD..... c	5 0 0		14 0 0	F. lb	0 8		
Cape, BD. c	1 16 0		2 0 0				
Aniseed, Oil of, German, D.P.... lb	0 9 6		0 9 6	F. lb	1 4	——	251
E. I. ..... lb	0 7 0		0 7 6	E. I.	1 4	254	392
Asafœtida, B.D. c	2 10 0		5 0 0	c	6 0	35	——
Balsam, Canada, D.P. ..... lb	0 1 3		0 1 4	lb	0 1	1,732	401
Copaiba, BD..... lb	0 7 6		0 8 6	c	4 0	42	76
Peru, BD. ..... lb	0 4 3		——	lb	1 0	637	224
Benzoin (best) BD..... c	25 0 0		50 0 0	c	4 0	41	32
Camphor, unrefined, BD..... c	9 0 0		——	c	1 0	203	236
Cantharides, D.P. ..... lb	0 5 0		——	lb	1 0	6,885	9,012
Carraway, Oil of, D.P. ..... lb	0 9 0		——	lb	4 0	——	585
Cascarilla or Eleutheria Bark, D.P.C.	1 15 0		——	lb	0 1	3,670	2,096
Cassia, Oil of, BD..... lb	0 7 0		0 7 6	lb	1 4	1,508	9,20
Castor Oil, East India, BD ..... lb	0 0 6		0 0 10	c	1 3	2,110	2,219
West I. (bottle) D.P. 1½ lb	0 2 3		——		——		
Castoreum, American ..... lb	1 15 0		——	lb	0 6	422	147
D.P. Hudson's Bay ..... lb	1 0 0		1 4 0				
Russian..... lb	none						
Catechu, BD. Pale c	1 5 0		——	c	1 0	4,267	10,751
Dark	3 0 0		——				
Cinchona Bark, Pale (Crown).... lb	0 2 0		0 3 6	lb	0 1	28,224	65,296
BD. Red ..... lb	0 2 0		0 4 0				
Yellow ..... lb	0 2 3		——				
Colocynth, Turkey ..... lb	0 2 6		0 4 0	lb	0 2	1,806	3,551
D.P. Mogadore ..... lb	0 3 0		——				
Calumba Root, BD. c	1 4 0		2 5 0	lb	0 2	9,611	7,304
Cubebs, BD. c	5 0 0		——	lb	0 6	11,828	12,408
Gamboge, BD. c	5 0 0		15 0 0	c	4 0	44	37
Gentian, D.P. c	1 4 0		——	c	4 0	223	301
Guaiacum, D.P..... lb	0 1 0		0 1 8	c	6 0	13	16
Gum Arabic, Turkey, fine, D.P... c	8 0 0		9 0 0				
Do. seconds, D.P. .. c	5 0 0		7 0 0	c	6 0	2,227	1,498
Barbary, brown, BD. c	3 0 0		3 3 0				
Do. white, D.P..... c	4 15 0		——				
E. I. fine yellow, BD. c	3 0 0		3 10 0	c	6 0	1,416	708
Do. dark brown, B.D. c	1 15 0		2 5 0				
—— Senegal garblings, D.P. c	4 15 0		5 0 0	c	6 0	3,194	173
—— Tragacanth, D.P. c	8 0 0		12 0 0	c	6 0	148	124
Iceland Moss (Lichen), D.P. .... lb	0 0 2½		0 0 3	lb	0 1	4,425	810
Ipecacuanha Root, B.D. ..... lb	0 1 9		0 2 0	lb	1 0	7,354	5,340
Jalap, BD. ..... lb	0 2 4		——	lb	0 6	11,914	18,669
Manna, flaky, BD. ..... lb	0 4 0		0 5 6	lb	0 3	2,713	12,900
Sicilian, BD. ..... lb	0 1 7		——				
Musk, China, BD. ..... oz	1 0 0		1 8 0	oz	6 0	556	475
Myrrh, East India, BD. c	5 0 0		14 0 0	c	6 0	60	61
Turkey, BD..... c	2 0 0		11 10 0				
Nux Vomica, BD..... lb	0 8 0		0 9 0	lb	2 6	272	509
Opium, Turkey, BD. ..... lb	0 14 6		——	lb	1 0	10,107	11,759
Peppermint, Oil of, F. BD..... lb	0 19 0		——	lb	4 0	186	461
Quicksilver, BD. ..... lb	0 3 6		——	lb	0 1	79,504	79,705
Rhubarb, East India, BD..... lb	0 2 6		0 3 3	lb	1 0	11,904	11,155
Dutch, trimmed, D.P. lb	0 3 6		0 4 0	F. lb	1 0	2,475	3,182
Russian, BD. ..... lb	0 8 3		——				
Saffron, French, BD. ..... lb	0 18 0		——	lb	1 0	1,306	2,206
Spanish ..... lb	0 19 0		——				
Sarsaparilla, Honduras, BD..... lb	0 1 0		0 1 9	lb	0 6	32,301	33,460
Lisbon, BD. ..... lb	0 2 0		——				
Scammony, Smyrna, D.P..... lb	——		——	lb	2 6	2,810	3,600
Aleppo ..... lb	0 12 0		0 15 0	E.I. lb	0 6	23,396	31,582
Senna, East India, BD. ..... lb	0 0 3		0 0 4				
Alexandria, D.P..... lb	0 1 9		0 1 10	Other sorts	0 6	24,021	25,584
Smyrna, D.P. ..... lb	0 1 0		0 1 3				
Tripoli, D.P. ..... lb	0 1 0		0 1 3				

‡§‡ BD. In Bond.—c. Cwt.—B. P. British Possessions.—F. Foreign.—D. P. Duty paid.

INSTRUMENT FOR EXTRACTING CATARACT.

In reference to Mr. Middlemore's letter, in our last number, we have to state that our anonymous correspondent of the previous week has sent us Sir James Earle's "Account of a new Mode of Operation," &c.; and we find that his quotation is "correct to the letter." There is a delineation of the instrument, which is about one-tenth of an inch in breadth. It may, perhaps, be worth while to make the following additional extract from Sir James Earle's pamphlet:—

"If, however, any difficulty should occur with regard to the extraction, or if from any reason it should be thought more advantageous to leave the whole or any part of the cataract within the eye, this instrument, capable of acting in a double capacity, becomes the best of all possible couching needles, as being blunt it cannot wound the iris, or do any mischief; being broad, it has great command over the lens, and, either shut or a little open, will readily transport any portion of the cataract to any part of the eye where it can lie out of the passage of light, till it is absorbed, which, when freed from all its connexions, takes place in no great length of time."

Our correspondent further requests us to add Quadri to the list of writers given by Mr. Middlemore, on the removal of the lenticular cataract by extensive division of the sclerotica.—Ed. Gaz.

PARALYSIS OF THE GUT IN HERNIA.

M. Tessier believes that in those cases in which, after the stricture of a strangulated hernia has been divided, and the intestine completely returned, evacuations from the bowels do not take place, there is no mechanical obstruction, but that the portion of intestine above that which has been strangulated is paralysed, so that it cannot act on its contents, to force them through the part of the gut which was strangulated, and has closely contracted, as intestine will do, under the stimulus of the pressure. On the post-mortem examination of such cases, he has found the superior portion of the intestine much inflamed and distended; and he notices, that when tympanitis takes place in these cases, the intestine above the strangulated part, which is so distended as to present its outlines evident through the abdominal walls, is never seen to move. Hence he says that, at the operation, the contents of the intestine should always be made to pass through the constricted part, and that after it active purgatives should be given.—*Archives Générales de Médecine*, Mars 1838.

[M. T.'s cases, as far as their pathology is concerned, afford considerable support to the opinion expressed by Dr. Abercrombie, in his work on Diseases of the Stomach and Abdominal Viscera, that in ileus, the actual disease is not spasmodic constriction of the part found contracted, but paralysis of the upper part, which, after death, is found so distended.—Translator.]

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, April 19, 1838.

Richard Francis Burton, 12, Wade's Place, Hackney Road.—James Williams, London.—John Hunter, Islington.—George Weekes, Hurstperpoint, Sussex.—John Matthews, Burton, Somersetshire.—Roger Rankine, Accrington, Lancashire.—James Mack, Handsworth, near Birmingham.

WEEKLY ACCOUNT OF BURIALS.

From Bills of Mortality, *Apr.* 24, 1838.

Age and Debility	22	Heart, diseased	1
Apoplexy	4	Hooping Cough	5
Asthma	5	Inflammation	10
Childbirth	1	Bowels & Stomach	1
Consumption	33	Brain	2
Convulsions	17	Lungs and Pleura	8
Croup	1	Measles	3
Dentition or Teething	2	Mortification	3
Dropsy	3	Rheumatism	1
Dropsy in the Brain	12	Small-pox	10
Dropsy in the Chest	1	Sore Throat and Quinsey	1
Erysipelas	2	Tumor	1
Fever	15	Unknown Causes	27
Fever, Scarlet	3		
Fever, Typhus	2	Casualties	4
Hæmorrhage	1		

Decrease of Burials, as compared with the preceding week . . . 224

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

April.	Thermometer.	Barometer.
Thursday . 19	from 27 to 44	29·68 to Stat.
Friday. . . 20	27 48	29·68 29·70
Saturday . 21	27 49	29·65 29·50
Sunday . . 22	25 48	29·20 29 28
Monday . . 23	32 56	29·43 29 30
Tuesday . 24	33 53	29·40 29·60
Wednesday 25	26 50	29.64 29·72

Winds very variable. N.W. prevailing.

Except the afternoons of the 21st, 23d, and 24th, generally cloudy; snow on the mornings of the 19th and 20th, and rain on the 22d and two following days.

Rain fallen, ·3875 of an inch.

Charles Henry Adams.

Errata.—In the article "Leicestershire Twins," page 110, line 17, *for* "sigmoid flexure of the colon," *read* "cæcum;" also, same page, column 2, line 30, *for* "partly," *read* "hardly."

Wilson & Son, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 5, 1838.

LECTURES ON BLOOD-LETTING,

Delivered from time to time,
At the General Dispensary, Aldersgate Street,

By Henry Clutterbuck, M.D.

Formerly one of the Physicians to that Institution.

Lecture IV.

Of the Medicinal Use of Blood-letting in general; with the Theory of its Operation.

I remarked at the outset, that blood-letting ranks among our most effective medicinal agents; and I may now add, that, like all others of great activity, it is powerful for good or for evil, according to the judgment and discrimination with which it is administered. It behoves us, therefore, to use it at all times with great caution and circumspection, lest, in our endeavour to do good, we unwittingly inflict a great, and perhaps irremediable injury on the patient. There is no doubt of this remedy having been sometimes resorted to without absolute necessity; that is, in cases that might have done well without it; but much more frequently, I believe, it has been withheld, where its use would have been followed by great and decisive benefit. Where it is employed, however, it is not always well-timed, nor judiciously administered in respect to quantity, repetition, and manner of drawing; all of which, we shall find, are points of no small moment.

In considering blood-letting as a therapeutic agent, one cannot but be struck with the wide differences of opinion entertained by practitioners with regard to it. Few, in the present day, are so hardy as to deny altogether the usefulness of this remedy, nor even the absolute necessity for it on many occasions; for to do this would be to fly in the face of almost universal experience. Yet by those who are favourable to its employment it appears to be held in different degrees of estimation, even in the same disease, and under apparently similar circumstances. Now, considering that its utility is determinable in all cases by careful and sufficiently extended observation, one is surprised at the discrepancy of opinion that still subsists on the subject. This can only be accounted for, I apprehend, upon the supposition that physicians, in their use of this remedy, have been more guided by theory, or preconceived notions, in regard to the nature of disease and the presumed mode of acting of the remedy, than by real experience, or a careful observation of its effects under actual trial. If we knew intimately and perfectly the nature of disease, as compared with the healthy state, we should probably comprehend the way in which blood-letting, as well as other medicinal agents, effects its purpose; and, at the same time, be enabled to apply it with something approaching at least to certainty as to the result. Such, however, is not the state of our knowledge at present. We are under the necessity, therefore, of placing our chief reliance on observation and experience, as the only guides that can be safely and implicitly followed.

Blood-letting may be considered in three points of view—as curative, as palliative, and as preventive; and in all these respects it is important.

In regard to its curative powers, blood-letting is capable of removing, with more or less facility, though never, perhaps, with absolute certainty, a great number of diseases which, but for its aid, would endanger or destroy life, and which cannot be effectually combated by other means. Considered in the light of a palliative merely, it is still of no small value. There are a great many diseases that are, from the first, and in their nature, as it were, incurable by art; others, that become so

during their course. But there are scarcely any that do not admit of more or less of palliation; and blood-letting is often the best means we have of effecting this purpose. An instance of this is afforded in the case of phthisis pulmonalis, where the acute pain that occasionally arises in the chest, even in an advanced stage of the disease, and when the case is altogether hopeless, seldom fails to be relieved by a small bleeding; and (provided this be done under proper limitation) without any increase of weakness, or other inconvenience. On the contrary, not only is the pain relieved, but the hectic and night-sweats also; while the appetite is usually improved by it, and sleep rendered more refreshing. The same remedy is also, on many occasions, preventive in its effect; by lessening, if not destroying, the tendency to certain diseases, of which, apoplexy, hæmorrhage, and inflammation, may be cited, as examples sufficiently well known.

Before, however, you have recourse, in any case, to blood-letting, as a means of cure, you ought to satisfy yourselves on the following points:—First, whether the remedy is adapted to the nature of the existing malady; and, secondly, (supposing this to be the case), whether the circumstances are altogether such as to justify its employment.

As to the nature of the disease, I may observe that there are some diseases to which blood-letting is commonly considered peculiarly adapted; such, for instance, are apoplexy, hæmorrhage from the lungs, and violent inflammation in general. Now, from the predilection that exists in favour of blood-letting in those affections, you will feel, as it were, predisposed to resort to it, perhaps without sufficient consideration, and at the risk of carrying it to too great a length. But it is proper you should know, that even in these diseases, blood-letting is not always admissible; on the contrary, there are many instances in which this remedy may be safely dispensed with; and others, to which it would be wholly inapplicable. On the other hand, the use of blood-letting is as generally condemned in what are termed nervous disorders, dropsies, intermittents, and others; it being considered not only useless in such, but in opposition to the very nature of these affections. All this, however, is by far too general, and admits of numerous exceptions. In fact, general rules on this, as on all other subjects, are very apt to mislead. Blood-letting may be better adapted to some diseases than to others—for example, to inflammation; but still, there is no form of disease which, at all times, and under all circumstances, admits of its employment. On the other hand, it may with equal truth be affirmed, that there are few which may not occasionally justify its use; so far, at least, as an auxiliary towards the cure. What these circumstances are, can only be fully and satisfactorily indicated when we come to treat of the application of the remedy to individual diseases.

Blood-letting is found useful in a great number of diseases that have little or no similarity, and such, indeed, as are generally considered to be of different natures, and to require different and even opposite modes of cure. Thus it will occasionally remove spasm, as well as inflammation. It is often anodyne, or a means of relieving pain. It sometimes acts as a narcotic, and is followed by tranquil sleep—at others, it occasions watchfulness. It is capable at times of removing obstructions, as they are termed, and thereby of restoring suppressed discharges; and, on the other hand, of restraining them when in excess. Nor is its use by any means confined to habits of strength or fulness; for it is not unfrequently needed by the weak as much as by the strong.

With regard to the other point I mentioned, namely, its fitness for adoption under the particular circumstances present, you should inquire whether the call for the remedy be so urgent that it cannot with safety, or even common prudence, be dispensed with; or whether there may not be found substitutes for it, that are equally safe, prompt, and effectual; for, in that case, they ought, no doubt, speaking generally, to be preferred. The reply to this, however, involves many considerations. Blood-letting, though well enough adapted to the existing disease, may nevertheless be not absolutely indispensable to the cure; for the disease may, perchance, subside without it, and yet its use may be attended with advantages that will more than compensate for any inconvenience to which it is liable. It may, for example, be more certain of attaining its object than other remedies, or it may be more prompt in its effect, so as to shorten the duration of the disease, thereby relieving the patient from protracted and unnecessary suffering. It may chance also (not a very improbable supposition) to be really milder in operation, and the cause of less suffering, both immediate and remote, than the other means usually resorted to as substitutes for it; such, especially, as violent and frequently-repeated purging, blistering, and the use of mercury, which are often so mercilessly inflicted on the patient.

Having thus decided on the propriety of drawing blood, you have next to determine the extent to which the evacuation is required, and in what mode it may be most

advantageously effected, in the particular case before you; for these, you will find, are not matters of indifference. The remedy must be carried to a certain extent, in order to ensure its purpose. This purpose, again, is sometimes best answered by a single large bleeding; sometimes by a repetition of it to a smaller amount. Our object, for example, may be to produce a sudden and marked impression on the system at large, as the surest means of accomplishing the end we have in view. In order to effect this, it is requisite that the blood should be drawn freely and rapidly. At other times, the purpose may be as well or better answered by a more moderate and quiet use of the remedy; as where the disease is of considerable standing, in which case it is hardly possible to cure it quickly, to whatever extent the remedy be carried.

Thus you perceive that there are various points requiring attention, and which must be treated of in turn. We will first, however, inquire into the probable mode of acting (the *ratio medendi*) of blood-letting, constituting the *theory* of the subject.

Theory of Blood-letting.

The *ratio medendi*, or manner in which medicinal agents effect their purpose in the removal of disease, is an important branch of medical study. Had we a perfect knowledge of this, we should be able to administer remedies with greater precision, and, probably, with greater certainty as to the result, than is at present the case. We should, at the same time, be better judges of the ill consequences to be apprehended from an injudicious or mistaken employment of them, and thereby learn greater caution in their use—a thing particularly necessary at present, when the most active and virulent poisons are daily, and, I might add, almost indiscriminately, applied to the purposes of medicine. This kind of knowledge, however, is of very difficult attainment. Therapeutic reasoning in general is of the most vague and unsatisfactory nature; little that has been brought forward on the subject bearing a scrutiny, or making an approach even towards demonstration.

A variety of opinions have been promulgated at different periods, in regard to the supposed mode of acting of blood-letting; founded on the particular views that happened to be entertained at the time, of the intrinsic nature, or proximate cause, of disease. Many of those opinions are altogether hypothetical, and also inconsistent with the principles of the animal economy; and accordingly they have been successively abandoned, in proportion as the extension of physiological science has served to shew their futility. Without dwelling on such, it will be enough to direct your attention for a short time to the existing doctrines, for the purpose of inquiring how far they merit our confidence.

When the circulation of the blood was first discovered, it was expected, and not without reason, that the mode of acting, as well as the effects, of blood-letting, would be clearly understood: but it turned out far otherwise; insomuch that it is a matter of doubt whether the subject did not become involved in greater obscurity than before. The effects of blood-letting were judged of upon mechanical and hydraulic principles; as if the blood were moving in inanimate, or, at least, in simply elastic tubes. The contractility of the vessels, a vital property, and nearly allied to muscularity, if not identical with it, was wholly overlooked, although, without reference to this, it is impossible to comprehend a variety of phenomena that present themselves in the movement and distribution of the blood.

The opinions most generally entertained at present, in regard to the mode of acting of blood-letting in the removal of disease, may be reduced to the following heads:—

1st. That it acts upon the principle of depletion simply, or, in other words, by diminishing the general mass of blood.

2dly. By weakening the system altogether.

3dly. As a sedative, or by diminishing vascular action, this in a great number of diseases being in excess.

The first of these opinions, namely, that blood-letting operates upon the principle of depletion, is founded on the supposition that there is often a superabundance of blood in the system, constituting what is called plethora; and that such plethora is a cause of various inconveniences, and even, at times, of immediate danger to life. It was thought, for instance, at one time, that such an overcharged state of the vessels would impede the general circulation, the heart being thereby rendered unable to move forward so unusual a load; and for this state of things, blood-letting naturally suggested itself as an appropriate remedy. Thus Boerhaave observes, in his *Institutes**, that "blood-letting (within the limits of not reducing the general strength) diminishes the quantity of blood in the vessels — the *moles movenda*—and thereby indirectly increases the moving power in the function of circulation." This notion was not unreasonable, as long as the heart was imagined to be the sole propelling power in the function of circulation. The diminution of the general mass, would, on such a supposition, enable

* Institutes of Medicine, No. 1228.

the heart to perform its office with greater ease. But when it came to be understood that the blood-vessels themselves contributed largely, by their action, to the movement of the blood, and that this action, as well as that of the heart, is liable to be diminished by any considerable loss of blood, it became clear, that the employment of blood-letting, for the mere purpose of promoting the general circulation, in the way here supposed, could not be supported.

There is, nevertheless, one case in which blood-letting may be said to promote, though indirectly, the general circulation; and that is, where the brain is suffering oppression from the violent action of its own arteries. This condition of brain, as it occurs in intoxication, and in many apoplexies, renders the heart and blood-vessels torpid, as indicated by unusual slowness, and sometimes feebleness, of the pulse. In such cases blood-letting often promotes the general circulation, by diminishing the action of the cerebral arteries, and thus relieving the organ from the state of oppression alluded to. With this exception, blood-letting tends to lessen rather than to augment the force of circulation. The propelling power, in fact, is diminished in a higher ratio than the resistance to be overcome.

Another idea was, that a plethoric state of system might, by distension, act as a stimulus to the heart and arteries, so as to excite them to inordinate action, and thus give rise to disorders of a febrile and inflammatory character. It is a question, however, whether these disorders, when they do occur, are really produced in the manner here supposed. Changes in the distribution or determination of the blood, from one part of the system to another, and consequent distension of vessels, are frequently taking place, and that to a great extent (as from the application of cold to the surface), without any bad effect following. It is, no doubt, true that many disorders of a febrile and inflammatory nature are quickly and effectually subdued by taking away blood; but the quantity required for the purpose is often too small to produce any appreciable effect in lessening the fulness and tension of the vessels altogether. It is in some other way, probably, than by simple depletion, that the salutary effect is produced in such cases.

It has been thought again, that, on some occasions, the vessels are in so plethoric a state, as actually to burst from over-distension; and the occurrence of spontaneous hæmorrhages was so explained. Upon this principle blood-letting would naturally enough be resorted to, as an appropriate means of cure. But admitting, as one must, that the remedy in question is found on many, though by no means on all, occasions, both to prevent and put a stop to hæmorrhage, the effect is explicable on other grounds, as will be shewn hereafter.

But the existence of plethora altogether admits of a question, and certainly is a thing not easy of proof. There are no means of estimating correctly the quantity of blood in the system, at any time; while the signs usually pointed out as proofs of existing plethora are, at best, equivocal. The plethoric state is supposed to be indicated chiefly by the following signs:—General fulness of habit; a florid hue of the skin; redness of the tunica conjunctiva of the eye, and of the inside of the nose, mouth, and lips; a distended state of the veins; an augmented temperature of the whole body; a sense of fulness or distension, especially in the head and chest; drowsiness, and disinclination for all exertion; and lastly, a strong, full, and slow pulse, with a disposition to occasional returns of hæmorrhage.

Now there is hardly one of these that is not equivocal, and that may not be more satisfactorily accounted for. I hardly need observe to you, that fulness of habit, or corpulency (which consists in an unusual accumulation of fat in the different textures appropriated to its reception), is quite distinct from plethora, or superabundance of blood in the vessels. It is a common observation, that corpulent people generally take but little food; and you will recollect what I before stated, that frequent bleeding tends rather to induce than to diminish corpulency. It is a well-known fact, also, that fat persons do not generally bear bleeding so well, nor to the same extent, as those of spare habits.

The florid countenance, the redness of the eyes, and other similar signs, indicate only an increase of arterial action, and consequent determination of blood to particular parts; while a full and strong pulse, and tumid veins, with augmented temperature, are merely the result of a general increase of action in the sanguiferous system. The sense of weight and fulness in the head—the drowsiness, and disinclination for exercise—and the slow pulse—which are so often met with in corpulent persons, may be attributed to pressure on the jugular veins, by which the return of blood from the head is impeded, and thereby a degree of stagnation in the circulation of the brain produced; thus giving rise to a lethargic, or slightly apopleetic state. The same mode of reasoning may be applied to the chest, where a large accumulation of fat cannot fail to embarrass the movements of the heart and lungs.

The causes assigned for the production

of plethora,—namely, luxurious living, without sufficient exercise, and too great indulgence in sleep, are far from satisfactory. They are causes, indeed, of obesity; but this, as already observed, does not necessarily imply a super-abundance of blood in the system.

Another cause of plethora has been suggested,—namely, the removal of a considerable portion of the body, as by amputation of the larger limbs; it being thought, that in this case, the organs of nutrition would continue to form as much blood as before the body was so mutilated. This does not appear a very probable supposition, judging *à priori*. The actions of the stomach, and other organs that co-operate with it in supplying new materials to the body, are regulated principally by the wants of the system. Thus it is observed, that when blood is drawn to a moderate extent only, and so as not to disturb the general health, the desire for food is increased, and blood is the more rapidly formed, in order to supply the loss. The case of menstruation in females serves to illustrate this; for just as much blood appears to be formed in the intervals, as had been lost by the previous menstrual discharge. And it may be further observed, with respect to amputation, that there is commonly more or less of general disorder following the operation, and which usually continues for many weeks; during which nutrition is imperfectly performed, as is evident from the emaciation that takes place in such cases; and in this interval, the organs would be likely to accommodate themselves to the new circumstances of the system.

From what has been now stated, the existence of plethora can hardly, I think, be considered as sufficiently proved. The quantity of blood in the system may be easily diminished; but it is not so clear that it is ever materially in excess. Blood-letting, no doubt, is very capable of relieving most of the symptoms ascribed to plethora; but it probably acts upon another principle than that of depletion simply, namely, by diminishing arterial action, from the excess of which such symptoms appear, for the most part, to proceed. The effects of over-stimulation have probably been mistaken for those of actual repletion. By taking food in too great quantity, or of too stimulant a kind, the stomach in the first instance, and, subsequently, the rest of the system, are preternaturally excited; and a tendency to, if not actual, disease is thus generated. But this, it is evident, may be the case, without the supposition of plethora. And, moreover, when you reflect upon the small quantity of blood that is commonly required to be removed, in order to relieve such symptoms, and compare this with the entire mass, (of which, probably, it does not, in any case, constitute a thirtieth part), it is difficult to imagine the curative effect to depend simply upon the diminished fulness of vessels thus occasioned. If depletion simply were the cause of the benefit derived from blood-letting, the effect might be expected to correspond with the quantity of blood lost; whereas experience shews, that in many cases as great, and as beneficial, an effect may be produced by the loss of a few ounces of blood, taken in a certain way, as by a much larger quantity differently drawn.

But even allowing plethora to exist, which, for the reasons stated, appears to be at least questionable, it is still, as was observed by Vanswieten, "never of itself a disease, but only gives a predisposition to it: it is not a disease till it is accompanied by some disorder of functions*." Upon the whole, then, you may, I think, discard the idea of general plethora, or over-fulness of vessels, as a foundation for the employment of blood-letting.

There is one modification of plethora that has of late been much insisted upon by pathologists, in which, not general, but partial, over-distension of vessels, is supposed to take place, and to which the name of local congestion has been applied. This term appears to me to have been used without much precision, and applied to cases to which it has no proper reference. Literally speaking, local congestion means an undue accumulation of blood in the vessels of some part of the body; and, in this sense, is as applicable to arteries as to veins. But, in general, the allusion has been to the latter only; and accordingly, you read a great deal about venous congestion, especially in the liver, lungs, or brain; which organs are said, by a barbarous construction of language, to be in a congested state; while the most formidable, and even fatal symptoms, have been referred to this supposed source.

Thus, the stupor of apoplexy, the coma that attends certain states and stages of fever, oppressed breathing, and an impeded or disordered function of the liver, are frequently ascribed to venous congestion; and that with as much confidence, as if it were an established fact, and one that admitted of easy proof. But, in reality, there is no proof offered of the existence of such a state as venous congestion during life, that is at all satisfactory; nor, if

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* *Commentaries*, i. 277. This excellent practical writer is by far too much neglected in the present day. I know no work that contains such a fund of valuable practical matter (the result of actual experience), or the study of which is so well calculated to check the prevailing fondness for speculation, as the one here quoted.

proved, would it afford an adequate explanation of the symptoms ascribed to it. The subject is of some importance, whether theoretically or practically considered; and therefore I shall offer no apology for dwelling on it for a short time.

Venous congestion, as it is called, or a preternatural accumulation of blood in the veins, ought to be clearly proved, before it is used in argument, for the purpose of explaining the phenomena of disease; and more especially, before it is made a ground upon which to form our indications of cure. Now, in neither of the cases mentioned, apoplexy, coma, oppressed breathing, or liver disease, has the existence of such a state as venous congestion been satisfactorily established; nor, in fact, can it be so during life; and I need not tell you that the evidence derived from dissection is quite inapplicable to the point. It is true that the veins are often found gorged with blood after death; and that, perhaps, in a greater degree than ordinary in the particular cases mentioned. But the arteries are, at the same time, observed to be empty; the expulsion of their blood into the corresponding veins being, in them, the last act of life. The more active the arteries are while living, the more enlarged they gradually become, and the greater is the quantity of blood they carry; the more, consequently, they will have to discharge into the veins at the moment of death. A preternatural fulness of veins, as appearing on dissection, is to be regarded, therefore, merely as a proof of the greater activity of the arteries during life. So that venous congestion, as observed after death, in general only implies previous arterial fulness; as this, again, implies increased arterial action.

The existence of venous congestion has been inferred (but certainly not proved) from the imperfectly performed or interrupted function of the organ concerned. Thus, in the comatose state of fever, it has been thought that there was congestion of blood in the brain, especially in the veins and sinuses; and it has been advised, in consequence, to take blood from the jugular veins, as the readiest way of getting rid of the superabundance of blood supposed to be congested within the skull. But although it is undoubtedly advantageous, in some of these cases, to take blood from the jugulars, there are many others in which such a remedy would be injurious rather than beneficial; though, upon the principle of venous congestion, the practice alluded to should be applicable in all such cases.

Any mechanical obstruction to the return of blood by the veins, in their course towards the heart, might undoubtedly give rise to a distended state of these vessels, and which might be termed venous congestion. But this does not apply to the cases here alluded to. In ordinary apoplexy, and in the comatose state of fever, nothing of this kind is observed: there is no known impediment to the return of blood from the brain in such cases, unless it be that which is occasioned by the pressure of the distended cerebral arteries on the veins within the skull. Such pressure must of necessity produce more or less of stagnation in the circulation of the brain; and upon this stagnation, and not upon simple compression of the substance of the brain, as is commonly supposed, the impaired state or suspension of functions probably depends. It is no argument against what has been just stated, to say, that the principal arteries and veins within the cranium are remotely situated from each other; for the effect would be as readily produced through the medium of the incompressible cerebral substance, as if the arteries and veins were in immediate juxta-position.

Another way in which blood-letting has been supposed to operate in the removal of disease, is by inducing weakness; it being thought that, on some occasions, there is a preternatural degree of vigour of the whole system, constituting what was termed the sthenic diathesis, in opposition to the asthenic, or state of general debility. Upon this ground, diseases in general were arranged in two great classes, the sthenic and asthenic, and a mode of treatment inculcated in conformity with this distinction. Blood-letting, of course, presented itself as the principal debilitating power. All this, however, is entirely fanciful. There is no such thing as a general excess of strength in living bodies. The strength may be diminished in various ways, as by abstinence, evacuations, and long-continued over-excitement of any kind; but there is no reason for supposing it to be ever in excess, or beyond the degree of perfect health, a state in which every function is duly and perfectly performed; so that health and strength may be considered as nearly synonymous terms.

Diseases, therefore, neither consist essentially in, nor are derived from, excess of vigour in the system at large. I do not deny that weakening the general system, whether it be effected by blood-letting or any other means, conduces at times, and under certain circumstances, to the removal of disease, and especially where the habit is strong; but the same remedy is often found equally effectual for its purpose, where the body is already weak. In both cases, the remedy is uncertain in its effect, and frequently fails to cure. This would seem to shew, that it is not simply

by inducing weakness that the object is attained, but in some less direct way—probably by lessening the disposition to disease.

Lastly, blood-letting has often been supposed to effect its purpose by a sedative operation, that is, by diminishing vascular action, without regard to the general strength. A great number of diseases in which this remedy is found useful are accompanied by much vascular excitement; febrile diseases in general. But such diseases do not consist simply in increased vascular action, nor do they yield, with any thing like certainty, to this or any other sedative remedies. Blood-letting is not required in all cases in which vascular action is in excess; on the contrary, equally good effects are often found to result from remedies of a totally different nature, as will be seen hereafter.

It must appear, then, I think, from what has been said above, that it is neither as an evacuant merely; nor by lessening tension; nor by weakening the system altogether; nor by a sedative operation on the vascular system, that blood-letting effects its purpose in the removal of disease. Let us next inquire whether a more plausible explanation of the matter can be given, and one that at the same time will serve us as a better guide through the intricate paths of practice; for unless this is to be the result of the inquiry, it would be a waste of time to pursue it further.

Now when we consider that the same agent (blood-letting) is found capable of relieving, at different times, a great variety of morbid states that differ widely from one another, not only in their general characters but in their intrinsic nature—that, in innumerable instances, it proves a sovereign remedy for inflammation, and that under the greatest diversity of circumstances; that it is, at times, a powerful antispasmodic; that it often relieves pain; that it sometimes promotes, sometimes restrains, the different natural discharges; that it either conciliates sleep or induces watchfulness, according to circumstances; one is forced, I think, to conclude, either that it possesses different and even opposite qualities to enable it to accomplish such various purposes, or, which is far more probable, that it acts upon some very general principle that is applicable to all. The most intelligible explanation of the matter appears to be this: that by any considerable loss of blood, however occasioned, a kind of shock is given to the system, in consequence of which, all vital movements, morbid as well as healthy, are more or less disturbed. In this respect, therefore, blood-letting resembles, in its effect, other sudden and powerful impressions on the system, whether made on mind or body, and whatever be the cause producing them. As instances in proof, I may mention the cold-bath—extreme pain—various noxious substances swallowed, as alcohol, and many others—and terror, as well as other violent mental emotions: all these are capable of exciting great commotion in the system, so as to influence and disturb, in greater or less degree, the most important functions of life; and with the further effect, on many occasions, of modifying, suspending, or even putting a stop altogether to various forms of disease.

That blood-letting possesses a similar power, and that in the highest degree, cannot be questioned. There is no one function, either mental or bodily, that is not more or less under the immediate influence of this agent, according to the manner in which it is applied, and the extent to which it is carried. It quickly and powerfully disturbs the heart and whole vascular system; as is evident not only from the changes induced on the pulse, but from the capillaries, in extreme cases, suddenly ceasing to contract, so as to allow their contents to escape in the form of cold sweats; respiration is disordered by it—the alimentary canal and urinary organs often discharge their contents involuntarily—and, lastly, the cerebral functions of sensation, voluntary motion, and thought, are impaired, disordered, or even wholly suspended, by a sudden and copious abstraction of blood. Nor are morbid actions by any means exempt from this influence (for disease is only a modification of healthy action, and is more or less under the influence of the same agents.) In short, blood-letting, in checking or suppressing violent diseases of any kind, appears to act upon a principle very analogous to, if not identical with, what is called counter-irritation, but which in this case is, perhaps, better termed counter-impression.

Upon the principle now stated, namely, that of counter-impression, there is no difficulty in comprehending the superiority of venesection, in most instances, over the slower modes of drawing blood by leeches or scarification; and we at the same time readily understand why the same remedy should prove effectual for the removal of so many various forms of disease, as is found to be the case.

If, then, blood-letting really acts in the manner here suggested, and which there seems good reason for believing, it is obviously necessary so to administer it as to make the desired impression on the system at large, as the medium of influencing the local disease. It is also requisite that we should be well acquainted with the cir-

cumstances that give this a preference, or otherwise, over other modes of producing counter-irritation; for blood-letting is not always advisable for such a purpose, on account of the weakness of the patient at the time, although this oftener serves to modify, than altogether prohibit its use.

On the other hand, in cases, and under circumstances, to which blood-letting is well adapted, it is far more efficacious than the other modes of producing counter-irritation usually resorted to, such as blistering, the use of mercury, and the like; while it is free from many serious inconveniences to which these are liable, and which are often such as to render them wholly inexpedient.

ON PUERPERAL DISEASES.

Translated from the German of
THEOD. HELM,
By SETH THOMPSON, M.D.

[Concluded from p. 185.]

Treatment of Phlebitis.—Phlebitis, as a very dangerous and deep-seated form of inflammation, requires antiphlogistic treatment; and that this be efficient, it is neeessary that both generally and locally it should be actively employed. We must endeavour to cut short the disease as soon as possible; for when this is not accomplished in the first few hours, it is for the most part not to be afterwards effected. With the exception of those cases in which from the first all the functions are at so low an ebb that the worst consequences must be anticipated, full venesection should not be omitted. Locally the assiduous application of ice is frequently the only remedy which ean eheck the progress of the inflammation. Leeches act in these cases, in consequence of the deep seat of the inflammation, only in proportion to the quantity of blood which they abstract. With a view to cause a derivation to the intestinal canal, calomel and jalap (the latter in doses of a scruple or half a drachm) are preferable to all other purgatives. When these remedies, employed either together or separately, according to the degree of inflammation and its duration, do not arrest its progress, and the characteristie shivering fits occur, all rational treatment is unhappily at an end, and the attention of the practitioner must then be directed towards the alleviation of symptoms and the local treatment of metastases. The truly fearful shivering fits can in no wise be prevented. Bark is quite inefficient for this purpose, and generally augments the fever when it is retained on the stomach. The internal metastases are not less out of the reach of treatment than of diagnosis. The violent pains which accompany matastases to the cellular tissue, or into the articulations, are most relieved by the application of unguentum opii. Also the mania puerperalis is most easily tranquillized by opium. Venesection and leeches are generally of no avail. When the disease is drawing towards its fatal termination, opium is the only remedy which is efficient in procuring tranquillity both of body and mind, and in such cases which set all curative treatment at defiance, it is a most valuable resource to the practitioner.

Inflammation of the Mucous Membrane of the Uterus.

Dissection shews us—1st. A layer of greyish plastic exudation of lymph on the mucous membrane of the uterus, which is sometimes a line in thickness, and with difficulty separated: it is either spread over the whole surface of the membrane, or more eommonly attached only to certain parts, and most frequently to the neck; the uterus is also found in various states of disorganization, and traces of phlebitis, or some other fatal form of puerperal disease, are likewise present. 2ndly. A dirty brown or blackish glutinous or fluid exudation, which has a peculiar gangrenous odour, (inflammatio septica): the uterus is considerably enlarged, frequently appears of a brown-red colour through its peritoneal covering; its texture is soft, easily torn, and as if soaked with much uncoagulated blood.

Inflammation of the mucous membrane of the uterus is one of the most common forms, but occurs rarely alone when neither epidemic nor endemic causes favour the development of the septie character of the disease; when this is the case, however, it may be very prevalent. Those epidemics which have been attributed to putrescence of the uterus were in reality a septic inflammation of the mucous membrane.

When it appears eombined with phlebitis and peritonitis, it is not possible to

make a decided diagnosis, if the inflammation be not of the septic character; in which case the nature of the discharge is sufficiently characteristic. When it occurs alone, the symptoms are as follows:—Generally two days after delivery, there is slight fever without any pain, or with dull pain in the region of the uterus; the lochia are not bloody, but serous, and scanty; the os and cervix uteri rarely painful; the patients are hardly aware that they are ill. These symptoms continuing for some days without manifest increase, the fever becomes lessened, the lochial discharge more considerable, generally containing purulent mucus, and convalescence commences on the fourth or fifth day. In other cases the fever and pain increase, the lochia disappear almost entirely, in consequence of the extension of the inflammation, and phlebitis or peritonitis generally supervene, and modify the further course of the disease; or in some cases diaphoresis occurs, and the uterus expels, with a contraction like that which takes place in labour, coagulated and fatty-like lymph, which is frequently repeated; the result of which is decided improvement, and rapid recovery follows.

Very different are the symptoms and course of the disease when the inflammation is of a septic character. The fever is from the first much more violent; the pulse small and contracted, from 140 to 160 in the minute; the patient very restless and anxious; complains of great heat and thirst, and excessive weariness; the perspiration is suppressed, and skin dry and hot; the uterus remains large, compressible, not painful, unless strong pressure be made upon it; the os uteri gaping, hot, very tender to the touch; the lochia are dirty, bloody, discoloured, of a bad smell. The course of the disease is often very rapid. The different symptoms increase, the features become nearly cadaverous, the patients lie in a restless but half stupified state; the abdomen is distended, but not painful; the uterus so soft and compressible, that it is often difficult to ascertain its size; it bears pressure in every direction; the lochia have the characteristic smell of gangrene, are of a dirty black colour, and if the finger be introduced it brings away a similar stinking slimy fluid, mixed with broken-down membrane. This state of things does not often last long; generally on the third or fourth day the patient falls into a state of sopor or muttering delirium, and gradually sinks. In particular mild cases, in which, although they bear a certain resemblance to the above description, the fever, weariness, and change of countenance, are not so marked, and the septic inflammation has affected only the lower portion of the uterus, separation of the gangrenous part ensues, followed by suppuration and cure. In such cases, however, the cervix uteri is frequently detached from the body, or there is obliteration of the os uteri by adhesion, or a rupture into the bladder or urethra takes place, and fistulæ are produced, which are exceedingly difficult or impossible to heal.

Prognosis.—Generally speaking, this form of the disease is the least formidable; but, however, when the prevailing form of the epidemic, the prognosis is unfavourable. A gangrenous smell of the lochia and gaping os uteri are not of much importance in determining the prognosis; but a pulse of 140 or more, with an altered expression of countenance, either from the first or occurring during the progress of the disease, give a bad prognosis. When the uterus is so large and soft that its volume cannot be ascertained, the worst is to be expected.

Treatment.—When the dilute mineral acids and solution of chlorine have been employed internally and externally, as injections and lotions, without avail, bark, camphor, and vinous preparations, will be equally useless.

Inflammation of the Ovaria.

Morbid appearances.—One or both ovaries are surrounded by numerous flakes of lymph, or coated with a solid layer of lymph, and more or less strongly attached by it to the neighbouring parts. Their peritoneal covering is generally pervaded by injected capillary vessels, of a bright red colour. The inflamed ovary is enlarged, its texture very soft, friable, and contains much infiltrated lymph or serum. There may be also one or more abscesses, of the size of a linseed to that of a bean, in the substance of the ovary; its lymphatics also are generally distended with pus; or, instead of pus, the same sanious ichor as in metro-phlebitis may be contained both in the substance of the ovary and the lymphatic vessels. When the ophoritis has been acute, some

veins of the broad ligaments, or of the uterus itself, will be found to contain pus. It is much more common to find pus in the lymphatics leading to the lumbar glands, and in the glands themselves, in this disease, than in metro-phlebitis.

Course of the disease.—Inflammation of the ovaries is one of the most obscure, and at the same time most dangerous, diseases of the puerperal state. There is rarely any deep-seated pain in the pelvis posterior to the uterus; generally violent continued fever, without any discoverable local affection, is the only apparent symptom. The fever continues many days, even more than a week, in the same degree of intensity; the milk and lochia flow as usual, and the patient complains but little. Suddenly, however, great confusion of ideas occurs, and in the course of a few hours the patient is moribund.

The absence of all other symptoms during the progress of the disease should lead us to look for inflammation of the ovaries, especially when death has occurred suddenly; for peritonitis, phlebitis, and plastic inflammation of the mucous membrane, when not sufficiently acute to produce local symptoms, are not so rapid in their progress to a fatal termination; septic inflammation, moreover, is characterized by the lochial discharge. As in the greater number of cases it is not possible to form a certain diagnosis, little can be said as to the comparative frequency of recovery in these cases. But as many patients who have no other symptom than fever of several days' duration do recover, and in only a portion of those who die is ophoritis to be found, it may with some probability be deduced, that a certain number of those who recover have had ophoritis; and this must be our conclusion until our diagnosis becomes more precise.

Prognosis.—When there is no peritonitis, or other marked form of puerperal disease, the fever must determine the prognosis; and fever is always a suspicious symptom in the puerperal state, especially when of some duration, and when no local affection is to be discovered. When coma or delirium supervenes, death is not far distant. Besides, however, the immediate danger attending the disease, manifold are the sequelæ of inflammation of the ovaries.

Treatment.—The difficulty of diagnosis, and facility of confounding this with other and less dangerous forms of puerperal disease, are not of so much consequence, as we have as yet no specific treatment for the different varieties of the disease. Thus in the more acute cases, whether or not the existence of ophoritis be quite certain, active antiphlogistic treatment is necessary; in less acute cases a more negative mode of treatment may be adopted. General and local blood-letting, combined with the exhibition of calomel, are the means which experience point out.

Inflammation of the Peritoneum.

Morbid appearances.—Immediately on opening the greatly dilated abdomen, the intestines, which are frequently distended to three times their natural volume, escape forcibly from the incision. The diaphragm is commonly pushed upwards, and the liver with it; in some cases they can be found as high as the fourth or even the third rib. When the abdomen is freely opened the exudation shews itself, consisting principally of lymph or serum. In most cases there are many pounds of a thick yellow fluid, the colour of which may, however, vary from an orange to a greenish or brown tint, which, when the proportion of the serum to the lymph is small, is proportionably thick; in other cases the serum contains flakes of lymph, of variable quantity and size. Frequently the exudation has a truly gangrenous smell, is dirty and discoloured, and contains very few flakes of lymph (peritonitis septica.)

When the exudation consists principally of lymph, it is found covering and deposited between the different abdominal viscera, of a lemon or straw colour, semi-liquid and lumpy from the mixture of serum, or mixed with blood, of a reddish yellow colour, and purulent. It may also be nearly solid, and covering the different organs, more especially the uterus and ovaries, liver, and spleen, with a layer of lymph, which is sometimes easily separable, but in other cases is so tenacious, that it is only with difficulty to be detached from these organs in the form of filamentous strips. Between the serous exudation, containing but few flakes of lymph, and that which consists of layers of lymph of firm consistence, there are many intermediate states, the peculiarities of which depend partly upon the original quantity and

quality of the effusion, and partly upon the duration of the disease and the commencement of reabsorption.

On the peritoneal lining of the anterior abdominal parietes, and in the various prolongations of this membrane, especially those which envelop the uterus, the tubes and ovaries, and neighbouring portion of intestine, are many spots of fine capillary injection, more or less thickly scattered; and the comparative abundance of them has reference more to the nature than the quantity of exudation. Thus they are more numerous and more extensive when the effusion consists of lymph mixed with pus, than when principally serum containing flakes of lymph. When there is a tendency to a septic character, there are few such spots of injected vessels. Those portions of the intestinal canal which are sometimes found distended to three times their natural volume, consist chiefly of colon; the ileum is much less frequently in this state. The peritoneal covering of these portions of intestine is very vascular, and this is the more evident from the thickening of their muscular coat, and œdematous state of the mucous membrane. In some rare cases a plastic exudation is found upon the mucous membrane. The vessels of the omentum are much injected; and newly formed vessels in the effused lymph occur in different parts of this membrane, more especially those which are situated over the uterus.

When the exudation is partial, the effused fluid is rarely found between the convolutions of intestine; much more commonly between these and some other organ, and especially between the uterus and neighbouring portion of intestine, and is thus contained in a separate cavity, the parietes of which are formed by these organs. This exudation is of variable quality: when the disease has lasted from eight to twelve days, the fluid is mixed with much lymph, which is easily separated from the parietes of the cavity; but after a longer duration of inflammation, the quantity of fluid is much diminished, the sac containing it becomes smaller, and filamentous or membraniform adhesions are formed between the different organs.

In many individuals who, during convalescence from peritonitis, die in consequence of a relapse, dissection clearly shews the various ages of the exudation. In three cases there has been rupture or disturbance of one of these sacs during the process of healing, which has occasioned an attack of acute peritonitis. Combined with the appearance of peritonitis, there are generally traces of the other forms of puerperal disease to be met with, which are either older, or more recent, or of simultaneous production with the peritonitis; and this can in most cases be determined with tolerable certainty.

One finds in most epidemics, and generally in a great proportion of cases, besides the marks of peritonitis, also those of pleurisy, of pericarditis, or meningitis. Pleurisy is of the most common occurrence. The pleura pulmonalis is either covered with a thin glutinous layer of lymph, or between the pleura pulmonalis and costal pleura there is a variable quantity of coagulated lymph. This effusion into the cavity of the chest is as variable, both in quality and quantity, as that which is met with in the abdominal cavity. The lungs are in most cases compressed, their texture dense, and void of air and blood.

In pericarditis, which occurs much more rarely, there is a turbid yellow or reddish serum, containing flakes of lymph in the sac of the pericardium; the inner surface of the membrane is lined with an exudation of variable thickness and consistence.

More frequently a thin yellow exudation is found in the dura mater, or between it and the inner membranes; in some cases, also, a large quantity of turbid serum in the lateral ventricles.

Course of the disease.—After the occurrence of a shivering fit, often also without, and generally but a few days after delivery, the patient begins to be feverish, and has pain in the abdomen, for the most part confined to the uterine region, which is constant, and augmented by deep inspiration, moving in bed, or by examination with the hand; or it is only felt in consequence of these different exciting causes. Often there is fever without pain; and in such cases the absence of pain may continue during the whole course of the disease. When the pain is violent, the fever is also considerable. The pain is often so intense that the application of the hand cannot be endured; and this excessive sensibility is generally extended beyond the uterine region, over the abdomen. The lochia, milk, and functions of the

skin, continue unaffected; the urine is fiery red, and the bowels almost always constipated. The uterus generally remains high in the abdomen, is large, hard, and very tender on its whole surface; generally in the early part of the disease, the abdomen is not distended, except in very acute cases, and percussion gives a full sound over the whole abdominal surface. The further progress of the disease is always rapid. In some cases (but rarely, however) there is a sudden amelioration of the symptoms, insomuch so, that after twenty-four or forty-eight hours, the danger is much diminished, and in three or four days convalescence is established.

In the greater number of cases, however, the acuteness of the pain continues and increases; the abdomen becomes much distended, the respiration difficult, short, and high; the pulse is 150, or more; the features have a peculiarly anxious expression, and effusion into the abdominal cavity takes place. This continues commonly rapidly, and in the space of five or six hours there are as many pounds of effusion. The belly is still more distended, and has a doughy feel; and from the presence of a quantity of fluid, the posterior part of the abdominal parietes, in the space between the false ribs and pelvis, affords a dull sound on percussion, and as the quantity of exudation increases this dulness of sound extends over a larger surface of the abdomen. By pressing quickly and repeatedly on the abdomen, so as to shake its parietes, it is very possible, by the extension of this motion to the effused fluid, to cause a splashing sound, and thus indicate its presence: for this purpose it is necessary that the quantity of exudation be great, and there still remains a source of error, for if there be much fluid contained in the intestines the same sound will be produced. The discovery of the effusion in many cases which begin suddenly with fever, is of use in forming a diagnosis, as it is certain that peritonitis puerperalis, as well as the other forms of puerperal disease, can take place without any marked pain.

In most cases, after effusion has taken place, the fever continues in the same degree, although the pain diminishes, and in some cases ceases altogether; in others, on the contrary, it is augmented. The belly becomes still more distended, and the difficulty of respiration amounts almost to suffocation; the countenance is much changed. At this period of the disease there is often an appearance of scattered spots, the size of a pin's head, on the chest and abdomen, which are of a deeper red than the surrounding skin, and are afterwards converted into small white vesicles, filled with a purulent fluid (miliaria puerperalia alba et rubra*).

There is generally frequent vomiting of a peculiar, dirty, verdigris-coloured fluid, mixed with slimy matter, which is characteristic of effusion in the cavity of the abdomen; the ingesta are all rejected. The patient retains her full consciousness at this stage of the disease, and complains of the troublesome vomiting and weariness.

Under these circumstances, death takes place sometimes a few hours after the characteristic vomiting has commenced, when the disease has lasted but thirty hours; other patients lie in a state of torpor, with the respiration labouring, high and short, their extremities becoming repeatedly cold, and although every hour death seems to be approaching, they may remain five or six days in this moribund state. The vomiting in some cases ceases entirely, in others it continues, but, instead of the peculiar verdigris-coloured matter, it consists of a dirty brown fluid, having a faint smell, and which at last becomes black (a symptom of softening of the stomach). Sometimes, but rarely, there is vomiting of liquid fæces (ileus, caused by the great distension of the intestines). A fatal termination is not constant, however; even when the effusion is very copious, recovery may take place.

This happens generally with great increase of some one of the secretions. There is rarely a critical sweat; more commonly diarrhœa, or copious secretion of urine. When the latter secretion begins to be affected, it is at first scanty, thick, and very red, little increased in quantity. After some days, much thick purulent sediment (frequently a third of the whole quantity of

* These vesicles are generally distinct; sometimes, however, they occur in very dense groups, and in some cases portions of skin, the size of a dollar, are covered with confluent miliary vesicles; the individuality of the different vesicles being so completely lost that the spot has much more the appearance of a lichenous eruption; but the existence of less confluent groups, and of others, in which, although the vesicles are placed very close, no confluence has taken place, destroy all doubt as to the nature of the rash.

fluid) is deposited; the quantity of urine suddenly becomes very copious, from eight to ten pounds daily, and it is then quite clear, colourless, and without sediment. This may continue for seven or eight days. Frequent watery stools generally cause great fatigue, but this never happens from increased action of the kidneys; also when the discharge takes place from the intestinal canal, the exudation is more slowly reabsorbed than when it is accompanied with increase of the urinary secretion. In some cases the fever ceases entirely a few days after the exudation has taken place; but the abdomen remains large, there is a dull sound on percussion, the pulse becomes slow, the face swollen, pasty, the urine is scanty, and a hydrops anasarca is developed, which generally leads to a favourable termination, although frequently after the lapse of many weeks.

A peculiar form of erysipelas, resembling much the purulent infiltration of the cellular tissue which takes place in phlebitis, may accompany puerperal peritonitis. A portion of integument, the size of the palm of the hand, and generally situated over a joint, becomes of a deep-red colour, very tender to the touch or on motion of the subjacent muscles: it is hot and tense; not so hard, however, as from metastatic infiltration of pus, as in this case there is serous infiltration of the cellular membrane. The pain generally diminishes towards the second day, the other symptoms gradually disappear, and frequently in three or four days all traces of it vanish. Soon afterwards, even before the first has entirely subsided, the same thing appears in another spot, generally corresponding to the first—viz. in the same part of the right extremity which was first attacked in the left: this is succeeded by a third attack, and others follow successively. This form of erysipelas occurs rarely, but generally in cases which recover.

When the exudation is partial, recovery often happens; and then the fluid being reabsorbed, the walls of the sac contract, and, in proportion to the quantity of effused lymph and the rapidity of the absorption, are more or less firmly united. When this process has commenced, there remains for a long time after the cessation of fever, pain in the uterine region, confined to a particular spot, which is hard and swollen, and some rare traces of fluctuation can be detected. The absorption requires generally from five to six weeks for its accomplishment, and during this time there are repeated but slight attacks of fever. The urine at the same time generally deposits a heavy purulent sediment; and the diminution of the swelling, hardness, and pain, is proportionate to the quantity of this sediment.

Prognosis.—This is generally unfavourable, but is on the whole better than in phlebitis, and cure is more frequently effected than in that of disease. The prognosis is especially bad when the prevailing character of the epidemic is a tendency to a rapid, copious, and quickly fatal exudation. The violence of the attack, or existence of fever without pain, is by no means so suspicious as in phlebitis. The quantity of exudation does not much affect the prognosis. When fever and pain cease after abundant effusion, the prognosis is considerably improved, and the contrary when they continue. Fever without pain, and exudation followed by diminution or cessation of the fever, give a good prognosis. When fever precedes the symptoms of peritonitis several days, and is increased upon their appearance, the prognosis is decidedly bad. Sinking of the countenance and cold extremities are bad. Vomiting of verdigris-coloured matter—a symptom equivalent to the serous vomiting in phlebitis—puts an end to all hope. The fæcal vomiting is also a very bad symptom.

Appearances of dropsy during convalescence are not alarming. A partial exudation leaves great reason for hope, when the patient's strength is not too much exhausted, and when extreme quiet is observed. The appearance of sediment in the urine is a very good sign.

Treatment.—The nature of the disease requires active antiphlogistic treatment, and it must be endeavoured to cut it short at its outset.

If the fever is considerable, and the pulse strong, even when local symptoms are wanting, a venesection of from 12 to 15 ounces is indicated (there is less to be apprehended from a too free than from a too timid employment of the lancet.) When the pain is acute, 20 or 30 leeches should be applied over the region of the uterus, and then should be repeated if the pain continues. Joined

to them, nothing moderates so quickly both pain and fever, the first almost immediately, as cold applications to the seat of pain; but they cannot always be endured. The principal indication, moreover, which in acute attacks should never be omitted, is the energetic employment of mercury, both internally and externally. Large doses of calomel, and the rubbing in of a sufficient quantity of unguentum mereuriale on the thighs and belly, are often of great serviee. Diarrhœa is seldom occasioned, and still more rarely salivation; but their evils are not to be weighed against those dangers which their exhibition may avert.

When exudation cannot be prevented, the further treatment must be guided by symptoms. When fever and pain gradually disappear, as in those cases whieh are attended with anasarca, the employment of cream of tartar, eontinued for a long time, is frequently the best means of exciting and keeping up the secretions of the intestinal canal and kidneys. When copious diarrhœa takes place after effusion, with relief of symptoms, it is very necessary to be careful that it does not become excessive, otherwise it may lead to a rapid sinking. In such cases it must, if possible, be suppressed. Opium is here of little service: it is augmented by bark. However, rhatany and alum are very efficacious. When the inflammatory symptoms continue, even after effusion has taken plaee, local and general antiphlogistic treatment, according to its degree, are indicated. When the verdigris-coloured vomiting has once commeneed, all rational treatment is at an end. Effervescing draughts and opium, in large doses, are most to be relied upon for the relief of this symptom. When there is amendment after partial effusion, very spare diet, remaining in bed, and complete quiet, both of body and mind, are quite necessary to effeet a cure. An aceidental ehill, any vexation, or error in diet, may at onee destroy all hope of reeovery. Repose from the use of medicines is equally necessary: the stimulantia and roborantia are deeidedly hurtful; nothing more than the local employment of mercury, in small doses, and a eonstant and unwearied applieation of emollient fomentations, should be permitted. When convalescence is established, the principal indication is nourishment in proper quantity.

(5.) *Puerperal Scarlatina.*

This is a form of puerperal disease which has been always overlooked or confounded with common scarlatina, but differs in many respects from that disease. Thus, 1st, Scarlatina puerperalis is not contagious; 2d, it appears always only three or four days after delivery, like the other puerperal diseases; 3d, it occurs frequently without any affection of the mucous membrane; 4th, there is no regularity in the succession of the fever and eruption, for they either occur simultaneously, or the eruption may precede the fever: the manner of the appearance of the rash is also different, as it shows itself at the same time in different parts of the body without any order; 5th, scarlatina puerperalis frequently appears only on particular parts of the body, whieh does not happen in eommon scarlatina; 6th, its more or less frequent oceurrence does not seem to depend at all upon the prevalence of common scarlatina.

Hence it may be deduced, that it is a true disease of child-bed; and there are sufficient grounds for ineluding it among puerperal diseases.

Course of the disease.—Generally on the second or third day after delivery, there is a smart attack of fever, preeeded by a well-marked cold stage. The pulse is very peculiar and characteristic, being exceedingly quick, hard. and full, and the skin very hot and dry, The patient complains of little else than heat and thirst; the lochia and milk continue as before. Often there occur trifling pains in the uterus, which commonly cease when the rash shows itself. Generally the day following the commencement of the fever, the whole surface of the body becomes of a purple colour; or sometimes this appears first on the extremities, sometimes on the ehest. The fever continues undiminished, and in some cases the cervical glands are swollen and painful. Frequently, however, puerperal scarlatina is not aecompanied with fever, or it may oceur the day after the rash has shown itself. Sometimes the fever is trifling, and but a portion of the surface exhibits the exanthema; then the upper extremities alone, or the chest and one arm, may be affected. Towards the close of the second or third day, the exanthema is eommonly fully developed, and its eolour is that of the most intense form of scarlatina.

Frequently it is of a dark red, or bluish tint, like that of cyanosis; or in other cases it is very pale. In the milder cases the redness of skin and fever disappear on the third or fourth day: the desquamation generally takes place in minute scales, more especially in delicate females of fair complexion; but in other cases the epidemic is detached in larger scales or strips. In some cases the desquamation is the first positive confirmative of the diagnosis, when the eruption has been so pale as to be scarcely visible. Immediate recovery does not always follow desquamation. In many cases, without evident cause, peritonitis, splenitis, or anasarca, or even pleurisy with copious effusion, may supervene. In other cases desquamation does not take place; the fever remains considerable; the pulse suddenly, however, begins to sink; and this continuing rapidly, there is a sudden invasion of maniacal symptoms, leading to a fatal termination.

Prognosis. — Although this is the mildest form of puerperal disease, yet it requires always very careful watching. Violent fever is a less unpleasant symptom than intense colour of the rash. During the existence of the rash, headache is an unpleasant symptom; and when the rash has disappeared, symptoms of peritonitis or pleuritis give much cause for alarm. Splenitis, which may also ensue, is rarely to be detected.

Treatment.—The timely employment of antiphlogistic remedies will be of much service. Almost every case requires venesection. The neglect of this may frequently lead to a fatal termination. Small doses of calomel (from six to eight grains) are of much use at the commencement of the disease*. When there is want of action on the surface, and the development of the rash is slow, a wash of dilute mineral acid is of service. Such applications, and the internal employment of the mineral acids, are, during the course of this affection, the most useful remedies. Mania requires cold applications; when violent, leeches should be applied; but generally in these cases all means of relief fail. A better result may be hoped for, when peritonitis or pleuritis supervene, from the employment of the means before recommended.

* Viz. six to eight grains, given in divided doses in the twenty-four hours.

CONTRIBUTIONS

TO THE

PHYSIOLOGY OF VISION.

By D. Griffin, M.D. Limerick.

(*For the Medical Gazette.*)

The controversy as to the cause why an inverted image on the retina gives us a perception of the object looked at in its erect and natural position, has, it is well known, been terminated by the discovery of certain properties in the retina itself, which were said to be resolvable into a remarkable law, that afforded a perfect explanation not only of this, but of many other circumstances regarding vision. These properties were believed to be an original endowment of that membrane, and impressed on it for the purpose of connecting our perceptions of the objects that surround us with their real positions, directions, and other qualities appreciable by sight, in a uniform and simple manner. This law is expressed by saying, that "whenever rays proceeding from any point of an object are brought to a point on the retina, the whole of these rays, or any one of them, will represent that point of the object in the direction of a line perpendicular to the part of the retina on which they fall, without any regard to the degree of obliquity with which they may have fallen on it. It had its origin in the observation of the following circumstances, which I give particularly, that, in denying its existence, I may not be thought to underrate their importance:—

1. If we make moderate pressure with a blunt point on the ball of the eye, a black spot, surrounded in most cases by a luminous ring, will be seen; and this spot will take up a position opposite to the point impressed.

2. As the pencil of rays by which any point of an object is seen has its greatest breadth at the cornea, converging from thence in two cones towards the object and the image on the retina, we might expect that, by shutting out all of the pencil except a few rays which pass in near the margin of the pupil, the object would seem to shift its place, since in this case we see it by means of rays which come to us by a circuitous course, and of which no portion that pierces the eye points directly from the part of the retina impressed towards the object.

We shall find, however, under these circumstances, that the object keeps its position, and is seen in its real direction as perfectly as if we had admitted the whole pencil.

3. In following the course of a pencil of light through the eye, it will be found that pencils entering it at angles of 45° and upwards from the axis, do not contain a single ray which points directly from the part of the retina impressed towards the object; yet it is well known that objects at such angles, though indistinct from other causes, are seen in their true directions.

4. Another argument in favour of this property may be drawn from considering the situation of the punctum cæcum, or blind point of the retina. Objects below a certain size become invisible some distance to the right of the axis in the right eye, and to the left of the axis in the left; whereas the entrance of the optic nerve, on which this defect depends, takes place to the left of the axis in the first case, and to the right of it in the second.

Other facts are mentioned of the same nature and tendency; but the most remarkable, and—simple and beautiful as it is—by far the most important of all, in confirmation of the existence of this property, is the following experiment of Scheiner:—

If we take a small object, the head of a pin, for example, and bring it before the eye some distance within the best point for distinct vision, we shall see it enlarged, but hazy and indistinct, the rays from it being too divergent to be brought to a point on the retina. If we now place a card, perforated by a small pin-hole, between our eye and the object, a portion of that divergent pencil will pass through the hole, the rays of which are so nearly parallel that they will be refracted to a point. We shall now, therefore, see the head of the pin magnified and perfectly distinct; but the remarkable circumstance is, that, keeping the pin steady, if we move the card up or down, or to the right or left, which movements, of course, change the place of the image on the retina, the object will appear to move in an opposite direction in each case, though it shall not actually have changed its place at all.

Thus the first-mentioned facts prove that obliquity of incidence in the rays does not effect any apparent change in the place of the object, provided they still fall on the same point; while Scheiner's experiment proves that when this point is varied, there *is* an apparent change of place, and that this apparent change is in a direction opposite to that in which the image on the retina is made to move.

An interesting variation of the experiment, and one which leads to the same conclusion, is the following:—If we make two additional pin-holes in the card, one at each side of the former, and just so near it that the rays from the pin head through the whole three may pass through the pupil together, by holding it between our eye and the object we shall now see, as it were, three pins' heads. If any one of the holes is stopped, one of these will disappear; and by trials we shall find that, when the hole at the right side is stopped, it is the left pin that disappears, and *vice versâ*.

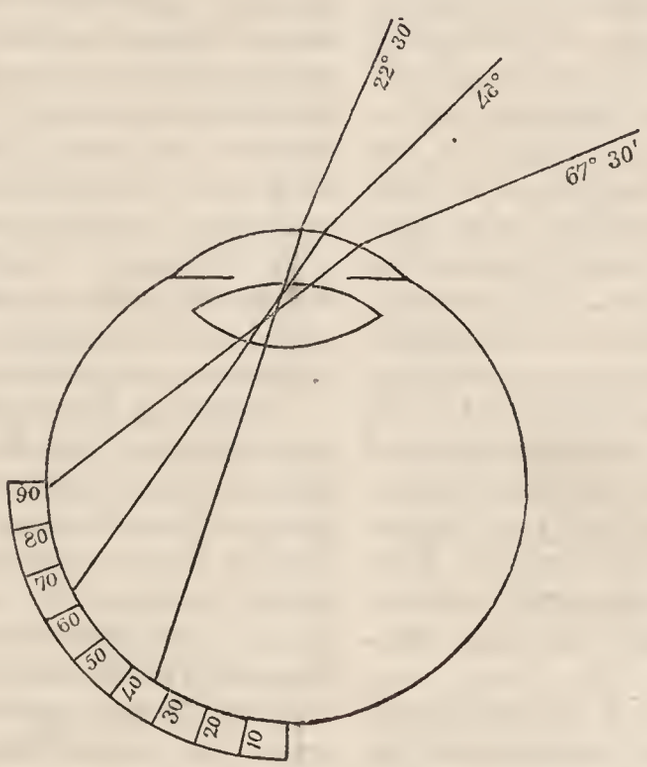
I have been thus particular in stating the law as at present received, with some of the principal facts on which it rests, because I think some circumstances have been overlooked which are utterly incompatible with its existence. A few years ago, being engaged in delivering some public lectures of the most elementary kind, on optics, at the Limerick Institution, it fell to my lot, during part of the course, to describe the structure of the eye, and its action as an optical instrument. In drawing the necessary diagrams, some circumstances occurred to me, which impressed me strongly with the conviction that this law could not be true. As, however, any discussion on the question would be out of place before an audience, many of whom were not familiar with the subject, I gave the explanation which is now generally received—a very unpleasant task to execute while I had the feeling of its fallacy so strong upon me. My attention has been drawn to the subject from time to time since; and though during that period I have seen many writers receive it as if it was well established, yet of this at least I am now certain, that the above law cannot be true under the conditions specified. These conditions are, that the concave surface on which the retina is expanded is spherical, or nearly so, and that the curvatures of the different humours of

the eye, and their indices of refraction, as given by the best authorities, do not differ widely from the truth.

The first of these conditions has, I believe, never been disputed; with regard to the second, the refractive indices and curvatures of the different humours have been examined by M. Petit, Dr. Young, Brewster, Gordon, Wollaston, and others; the following are the results; such of them, at least, as are necessary for our present purpose:—

Interior transverse diameter of the eye	·90
Horizontal cord of the cornea	·49
Radius of external surface of cornea	·33
Radius of anterior surface of lens	·33
Radius of posterior surface of do.	·24
Distance of iris from cornea	·11
Distance of lens from do.	·12
Refractive index of aqueous humour	1·337
Refractive index of lens (mean refraction)	1·384
Refractive index of vitreous humour	1·339

Let us suppose the interior of the eye, on which the retina is spread, to be graduated from 0 to 90°, and so on, beginning from the point where the visual axis strikes that membrane, and marking this point zero. If now, using the above curvatures and refractive powers, the progress of a pencil of light be traced graphically, following the usual law of the sines, it follows that if the above law of "visible direction," as it has been called, is true, the number of degrees marked on the point of the retina where it is found to fall, ought to indicate the degree of inclination of the pencil to the optic axis before it entered the eye; that is, in fact, ought to indicate the actual direction, in space, of the object from which it came, or at least, according to Sir D. Brewster's version of the law, ought to indicate the position of a line parallel to it, which would come to the same thing. In tracing the ray thus, the mean refractive index of the crystalline may be taken, instead of its varying one, without affecting the course of the pencil in any degree worth speaking of. Indeed, it will be found that by far the greatest portion of the whole refraction that takes place in the eye is produced at the cornea, and that when this is accomplished, the refractions produced by the lens and vitreous humour have but little influence further on the direction of the ray. The accompanying figure is a copy of a much larger one, drawn in conformity with these conditions; and it will be found that, tracing the course of the ray in the manner described, a



pencil inclined 22° 30′ to the visual axis, will fall somewhere about 34° on the retina; one inclined 45°, will fall on a point marked 63°; and a pencil inclined 67° 30′, will fall on a portion of the retina which, if it possessed the property above mentioned, must represent it as standing nearly 89° from the axis of vision. Pencils at intermediate stations will be found to deviate in in-

termediate degrees; but in every case there is an error, which increases with the inclination of the pencil, though not exactly in the same ratio.

Now it is well known that exteriorly the field of view extends even 90° from the axis—that is, when the eye is directed straight forward, we can see objects in any position, from 0 up to 90°, on the outside of the eye, and see them in their true positions. But it will be found, by trials on a diagram such as I have described, following the same rule as for the other rays, that a pencil of light from an object removed 90° from the axis, cannot possibly be brought to a point marked 90° on the retina. The question is not here, whether we can see them distinctly or not, but whether we can become sensible of their presence, and if they are represented in their true positions; and neither of these points, I believe, has ever been disputed.

Thus it is evident that this law, stated, as it has been, on what I cannot help calling a very loose examination of the circumstances, is not true, nor even nearly true; and we do not get out of the difficulty by supposing that the indices of refraction of the different humours have been under-rated; for (not to speak of the improbability that such a large error could be committed in ascertaining them as this supposition would require) it is clear that the refractive power necessary for bringing pencils entering the eye at angles of 45°, or 70°, to points at the same angular distance from the extremity of the visual axis, would make the eye myopic for pencils lying near the axis, and would thus render the very best part of the retina utterly useless. We cannot, therefore, put the facts we have collected under any general expression, but must rest satisfied, for the present, with the simple statement, that when rays of light fall on any point of the retina, that point has the property of representing the object from which they come *in its true direction*, without any regard to the obliquity of their incidence.

I have examined, with the assistance of another person, the positions of the spectra when pressure is made on different parts of the ball of the eye, in order to see how far facts of this kind support the view which is sought to be maintained. Extreme accuracy cannot be expected in such examinations, but the following are the results, as near as possible:—When the pressure is made 90° from the axis on the outside, the spectrum appears anterior to the bridge of the nose; when the axis is directed towards this latter part, and pressure is made as deep as possible on the outside of the eye, the spectrum appears a little within the bridge of the nose; when the axis of the eye is directed outwards as much as possible, and pressure is made as deep as one can at the inner canthus, the spectrum stands about 30° on the outside of the point to which the axis is directed; and generally speaking, I find that whatever be the position of the axis when the pressure is made round the ball of the eye and within the edge of the orbit, the spectra usually appear round the margin of the field of view. I noticed a curious circumstance in making these examinations, which is, that though motion of the blunt point on the eyeball produces very free motions of the spectrum, yet, when pressure is made on any point, as the inner canthus, for instance, so as to produce a spectrum, and the point is then kept perfectly motionless, and the axis of the eye is directed inwards or outwards, which one would suppose amounted to the same thing as movement of the point itself, in this case the spectrum remains almost perfectly at rest. If the axis of the eye is directed up or down, in the same circumstances, there *is* a movement of the spectrum, which, however, is not at all proportional to the movement of the axis. Though these facts may seem incapable of explanation, and even inconsistent with each other, they evidently give no support to the notion that the retina has the property of representing objects in lines perpendicular to its surface.

Assuming this law of visible direction as true, Sir D. Brewster endeavours to account for the stability of objects which occupy the field of vision during the motions of the eye, by supposing that the centre of visible direction, or the point through which all the lines of visible direction pass, is coincident with the centre of motion of the eye. He says (Treatise on Optics, p. 224), "when we move the eyeball, by means of its own muscles, through its whole range of 110°, every point of an object within the area of the visible field, either of distinct or indistinct vision, remains absolutely fixed; and this arises

from the immobility of the centre of visible direction, and consequently of the lines of visible direction joining that centre and every point in the visible field." That objects within the visible field remain fixed during motions of the eye, is a fact; but that the above is the cause of their stability, is by no means true. He says further, "Had the centre of visible direction been out of the centre of the eyeball, this perfect stability of vision could not have occurred." It certainly could not, if the centre of motion coincided with the centre of the eye, and that the retina had the property stated; but the stability of objects, as I shall presently shew, is perfectly compatible with other properties of the retina: in fact, the supposition of the existence of the property stated, is the only thing that could make a difficulty about it. He says further (same page), "If we press the eye with the finger, we alter the spherical form of the surface of the retina; we consequently alter the direction of lines perpendicular to it, and also the centre where these lines meet, so that the directions of visible objects should be changed by pressure, as we find them to be." Now this is a complete misapprehension of a fact; the directions of visible objects are changed, in this instance, not from any change in the form of the eye, but because it is either directly pushed or drawn out of its direction towards the object, by actions induced in some of its muscles by the pressure; for precisely the same change of situation will be produced, even more perfectly, by laying hold of the under or upper eyelid, and drawing the eye in different directions by means of its attachment to the conjunctiva; a proceeding which cannot give rise to any alteration in its sphericity.

The following observations will lead, as it appears to me, if not to clearer and more simple, at least to truer notions on the subject of vision, than those usually received:—In the first place, as the refraction of the humours of the eye is unchangeable, it follows that rays standing at the same angle from the axis will always be refracted to the same part of the retina; they will not one time come to a certain point, and another time be bent more deeply into the eye, but will always strike the membrane at the same distance from the extremity of the visual axis. Considering, therefore, the nervous matter of the retina as made up of numerous zones distributed in parallel bands around the point where the visual axis strikes the back of the eye, we see that rays entering the eye at an angle of 45° for instance, with the axis, will fall on a zone of nervous matter situated somewhere about 63° from this point. Rays from every object standing at that angle from the axis all around, must fall upon some part of this zone; no rays from objects standing at other angles can ever touch it; and we find that this zone has the property, when rays fall on it, no matter with what obliquity, of representing the object from which they come as standing 45° from the axis. It must be remembered that I am now speaking of facts: that the retina has this property cannot be disputed, however it may be supposed to have come by it; whether it was an original endowment of the membrane, or an acquired property, is another question, one which seems to me to have lost its interest, and not to be of much importance; yet I would remark, that the circumstance of our being at length obliged, as I feel we are, to resolve all our knowledge of vision, not into a general law, but into a simple statement of the fact, that all parts of the retina have the property, no matter with what obliquity the rays fall on them, of representing the objects from which they come in their true directions, is a strong argument for the latter supposition; for this is precisely what we should expect to occur as the result of experience. Taking each of the other zones of the retina in the same manner, we see that our perception of the angular distance of every object from the axis is predetermined by the zone of nervous matter on which rays from it fall, and its actual position by the part of the zone to which they are brought Without speculating on the cause of these properties at all, it is evident that under this arrangement there can be no instability of objects in the field of view during motion, wherever we suppose the centre of motion to be situated, and whatever point the centre of visible direction, if any such there be, may occupy. In fact, as I have said before, the conferring on the retina a property of representing all objects in the direction of lines perpendicular to the surface on which the rays impinge, is the only thing that could endanger

their stability during motions of the eye, since, in this case, the coincidence of the centre of visible direction with the centre of motion and centre of curvature, would be absolutely essential to its maintenance, which conditions would be by no means necessary in any other case. A little consideration will show this to be a fact.

There is something so attractive in the simplicity with which a general law groups and classifies various kinds of facts, that we are apt to be dazzled and caught by it, and to overlook difficulties which, if traced to their consequences, would certainly compel us to its rejection. I cannot otherwise account for the circumstance that a passage in the fifth edition of the late Dr. Turner's Chemistry, which I fell upon while writing this paper, should be so entirely disregarded by Sir David Brewster, Mr. Mayo, and many other physiologists, who, notwithstanding its clearness, still adhere to the explanation I have been endeavouring to refute, and which seems so wholly untenable. In that passage Dr. Turner expresses himself not satisfied with Sir D. Brewster's statements as to the law of visible direction, and traces its operation to consequences very analogous to those which I have exhibited above. I cannot help expressing some surprise that consequences so obvious as these should have escaped so many; the drawing even of a single diagram of the eye, if the circumstances are at all attended to, would inevitably lead to them, for it will be found in every case except where the object is in the axis of vision, that the law usually adopted would represent it out of its true position, and the line of visible direction will not even, as Sir D. Brewster says, be parallel to the line of its true posititon; even when situated in the axis, it will, if it is of any size, be magnified and distorted. Mr. Mayo, indeed, seems to have met with some difficulty of this kind in drawing the diagrams for the fourth edition of his Physiology (1837); for finding, on trial, that lines drawn from the point of the retina on which the rays fell, towards the extremities of the object, were not perpendicular to the retina, he very strangely, instead of questioning the truth of the law, seems to be obliged to modify his expressions regarding these "lines of visible direction," and says, in proceeding from a consideration of the simple lens and luminous point, to a diagram of the eye—"Let me substitute for this diagram one of the eye in vision, in which the dotted lines, A B, C D, are meant to be vertical to the points A and C, and are to be understood to be so." It seems curious that so accurate an observer as Mr. Mayo usually is, should not have followed this difficulty into all its consequences. Had he done so he must necessarily have arrived at the same conviction that Dr. Turner expressed so clearly, nearly four years since, and that I have been endeavouring to establish in this paper.

From the slight difference in refractive power that exists between the aqueous, crystalline, and vitreous humours, the influence of the two last on a ray of light is very trifling, so that, as I have mentioned above, by far the greater part of the whole refraction takes place at the cornea. Though this is pretty generally known, I am afraid the small amount of the refraction is scarcely sufficiently considered. Indeed, when once this first refraction has been effected, the course of the ray afterwards is so nearly that of a straight line, that a superficial observation would lead one to think that no allowance had been made for the remaining refractions. A most important practical inference arises from this; that is, the immense importance of preserving the original shape of the cornea in operations for the extraction of cataract. Those who are familiar with the grinding of lenses and specula, and who know what exceedingly minute errors of figure are capable not only of giving rise to indistinctness, but of destroying all distinctness in the image, will perceive the importance of this remark. The loss of the lens must not be considered the greatest injury the eye has sustained in this case; indeed, I strongly suspect that the uncorrected spherical aberration of the cornea is the principal source of the indistinctness that remains even with the use of glasses, after those operations for cataract, even of the most successful kind, in which the cornea has been untouched. The aberration of lenses being imperceptible in small refractions, and the refraction of the lens being exceedingly slight, in consequence of the slight difference in refractive power between it and the aqueous humour, it can scarcely be supposed, if the index of refraction given be at all

near the truth, that its peculiar structure of diminishing density was introduced for the purpose of correcting its own aberration, which could never be worth taking into account.

I have never seen, nor even heard, of an operation for cataract, in which perfect distinctness was produced; yet this ought sometimes to take place in operations for soft cataract, if the cornea contained a means of correcting its own aberration, and the only injury the eye had sustained was its being made somewhat presbyopic from the loss of the lens; for a glass lens would, in such a case, make up for the deficiency of refraction, without introducing any perceptible aberration. It is therefore of great importance not to introduce any incorrigible, or unsymmetrical aberration, by any carelessness about the shape of the cornea in the management after the operation. The great indistinctness which such unsymmetrical refraction may occasion in an eye otherwise good, is fully shewn by Professor Airy's observations on the defective figure of the cornea of his own eye, the discovery of the nature of which, and the success of the remedy applied, form one of the most interesting instances on record of the application of optical knowledge to the removal of imperfect vision. I do not make these remarks from any apprehension that the replacing of the flap after extraction, or the accurate healing of the wound, could ever be considered matters of light moment, but simply from this circumstance, that when an end is sought to be attained, it is always of consequence to hold clearly in view the most important principle upon which its attainment is desirable.

While engaged in writing the above, Professor Rainy, of Glasgow, directed my attention to a paper on the same subject, which appeared in the ninth number of Poggendorf's *Annalen*, by Professor Mile. I understand that he also denies the existence of the law I have been discussing; but I have not yet been able to procure an account of his facts or reasonings on it, which I am very anxious to do.

I have seen several articles in the medical periodicals, many of them containing very ingenious speculations, on single and double vision, and other subjects relating to optical physiology, by Mr. T. Williams, Mr. Grove Berry, Dr. Graves, of Dublin, and others. All these gentlemen, as far as I can perceive, found their reasonings on these subjects on the supposition of the truth of Sir D. Brewster's law of visible direction; most of their opinions, therefore, do not admit of discussion while this remains unsettled. If they doubt the assertions I have made above, with regard to the refraction of the rays, all I have to request is, that they will take the trouble to draw a large diagram of the eye, using the refractions and curvatures given above, and inform me what part of the retina rays at the angles above mentioned really do fall on. This is the great and fundamental point, and we cannot proceed a single step until it has been fully determined. I do not contend that my determinations of these points are perfectly accurate, but I have no hesitation in saying, that they do not contain any error which, if removed, would make Sir David Brewster's law at all possible. Dr. Graves, in one of the numbers of the Dublin Journal of Science, in speaking of this law, expresses some surprise that the Rev. H. Lloyd, and Sir J. Herschel, should, in their works, have taken so little notice of it. I confess I was myself surprised at their being so little influenced by views which were stated so universally, and seemed to be supported by various facts; but though I still think many of their views regarding vision incorrect, a particular examination of the subject has made me feel the necessity of every law being based upon principles that cannot be shaken, before one can insist on its universal reception,

Before proceeding to another part of the subject of vision, I must mention a curious fact regarding the intolerance of light, which I discovered some time ago. If we look at a bright sunny road in the height of summer, or at one of those white fleecy clouds called cumuli, the light is so intense, that, besides the pupils being contracted to the utmost, we are obliged to cover a considerable portion of them by half closing the lids. In these circumstances the sensation of intolerance is felt in the eye, and may be thought to have its seat in the retina. If, however, we close one eye entirely, we shall find that the other may be then freely opened without uneasiness, which shews that the real seat of the sensation must be some part of the sensorium itself, and not the retina, which is actually then receiving more light than before.

We have here, therefore, a highly intellectual sense—intellectual as regards its anatomical connexion with portions of the brain devoted to the process of thought, and intellectual as regards the mental processes which many of its perceptions imply; exhibiting, at least as far as concerns its common sensibility to light, the same law which has been found to prevail in other parts of the nervous system—namely, that when a certain state is induced at the centre of the nervous mass, the resulting sensation is referred to its extremity. This curious fact may, perhaps, be of some importance in the management of those annoying and intractable forms of ophthalmia, in which intolerance of light is so prominent a symptom.

The following experiments were undertaken for the purpose of determining the situation and size of the punctum cæcum of the retina. The greater number of them were performed in the following manner:—The back of the head being placed in contact with one wall of the apartment, the distance was measured, as near as possible, from the centre of the eye to the opposite wall. A candle was so placed as to make its image appear in the centre of a convex mirror hung there, which gave the flame of the candle a small and star-like appearance, better adapted to the experiment. The right eye being then fixed first on the image, was directed to the left of it, and at the last point, where I was certain I could see it, a wafer was placed on the wall. Moving the eye still to the left, a wafer was placed again on the wall, at the first point, where I was certain I could *not* see it; going on still to the left, a wafer was placed at the last point, where I was certain I could *not* see it, and again at the first, where I was certain I *could*. Drawing a line now, from half the distance between the inner wafers to half the distance between the outer, it is evident that this line might be taken to represent the angular breadth of the insensible spot; and, accordingly, when the right eye was directed to the middle point of this line, the image of the candle was perfectly invisible, from its then falling on the centre of the blind spot. Moving the eye upwards and downwards from the middle of this line, the vertical diameter of the spot was obtained in the same manner. The length of these diameters being measured, as well as the distance from the centre of the mirror to the point where they crossed, the lengths thus obtained were divided by the distance of the centre of the eye, which gave the tangents of the angle subtended by the blind spot, and of its angular distance from the visual axis. As it it is not necessary to go into these calculations, I have just subjoined the results. That I might not be misled in repetitions of the experiment, by my having taken the measures of those that I first performed, the experiments were varied in different ways as to distance and light; and with the same view, the calculations were not entered upon for any until the whole were completed.

FIRST SET (RIGHT EYE.)				SECOND SET (LEFT EYE.)			
No. of Experiments.	Distance from Visual Axis.	Diameter.	Centre of Spot above Visual Axis.	No. of Experiments.	Distance from Visual Axis.	Diameter.	Centre of Spot above Visual Axis.
1	15° 4′	7° 11′	1° 10′	1	15° 24′	7° 31′	1° 47′
2	15° 44′	7° 31′	(1° 18′)	2	15° 28′	7° 31′	(1° 18′)
3	15° 6′	6° 59′	1° 29′	3	15° 50′	6° 19′	1° 37′
4	16° 6′	6° 12′	(1° 5′)	4	15° 49′	6° 19′	1° 38′
5	15° 11′	3° 15′		5	16° 18′	6° 0′	(1° 5′)
				6	15° 30′	2° 45′	
Means	15° 26′		1° 15′	Means	15° 43′		1° 29′

Taking the means, we have, therefore, for the distance of the centre of the spot from the visual axis in the right eye, 15° 26′, and in the left 15° 43′.

Taking the mean of these means, we have 15° 34′ as the most probable value from these experiments. But it would not be fair to take a mean of the diameters of the blind spot in the same manner, for the differences which the tables shew seem to depend upon circumstances which I will now explain, and which lead to a conclusion of some importance. Some of the experiments were performed in the way I have just described; some by placing a circular paper, seven or eight inches in diameter, on a light-coloured wall, and standing just so near it that the whole would be completely but barely hidden, when the axis of the eye was turned in a proper direction; others, again, were performed by shading the flame of a candle with a cylinder of dark paper, in which a small hole was cut for its light to appear; the experiment, in other respects, being proceeded with as at first described. Lastly, they were done in Dr. Young's manner, with two unshaded candles. Taking, therefore, for each eye a mean of those experiments in which all the circumstances were alike, and distinguishing the others, we have the following interesting results:—

RIGHT EYE.		LEFT EYE.	
Nature of Experiment.	Diameter of Spot.	Nature of Experiment.	Diameter of Spot.
With paper on light coloured wall	7° 31′	With paper on light coloured wall	7° 31′
With image in mirror	7° 5′	With image in mirror	6° 19′
With luminous point through the cylinder	6° 12′	With luminous point through the cylinder	6° 0′
With unshaded candles	3° 15′	With unshaded candles	2° 45′

Here we have first a white paper on a light-coloured wall, in which the light is feeble and the contrast slight: we have, therefore, a large diameter. Next we see the image in a convex mirror, in which more than half the light is dispersed and lost: there is still, therefore, a tolerably large diameter. Next we have the direct light of the candle seen through a small aperture, by which the intensity of the light is twice as great as before, and we have a diminished diameter; and lastly, we have unshaded candles (Dr. Young's method), in which, besides having light of the same intensity, we have a considerably increased quantity of it; and in this case the diameter is less than in any other. Hence we see that the diameter of the spot is diminished as the strength of the light increases; and this circumstance seems to indicate at once the cause of the blindness which appears to owe its origin not as Mayo and others suppose, to the presence of the artery in the centre of this spot, but to the thickness of the nervous matter at this part; the optic nerve not having yet spread out into those thin filaments which are exhibited in the structure of the other parts of the retina. This conclusion best explains the facts; for we see, that at some distance from the centre of the optic nerve, its sensibility seems dull to moderate lights, and it is only capable of being roused by very strong lights at the centre itself. Indeed, I found the centre perfectly insensible to the image in the mirror, when it was brought directly opposite it. The optic nerves, in this respect, resemble other nerves in the body which are not fit for their functions until they have been distributed in thin and fine filaments. Moreover, this conclusion, if true, seems to be important in another point of view; for if the thickness of the nervous matter here is the cause of the blindness, it may reasonably be asked whether the thinning down of the nervous matter which takes place, according to some anatomists, around the so-called foramen of Soemmering, may not be the cause of the great acuteness of vision that exists at the end of the optic axis?—supposing it should be found, by experiments of the above kind and by measurements of the eye, that these two parts coincide. I found the presence of the artery at the centre of the optic nerve quite percepti-

ble by a reddish glare, which shewed itself about the centre of the invisible part of the field; but this appearance only took place in the experiment with unshaded candles.

In performing the above experiments, I found a tendency in the eye, in moving outwards, to move also a little upwards; from the circumstance that the image, or candle, was more perfectly hidden then, than when the axis of the eye was directed to a point in the same horizontal line with it. This seemed to indicate that the punctum cæcum was situated a little higher than the extremity of the visual axis. It is evident, however, that no complete proof of this could be obtained, except from experiments performed with both eyes at the same time, since there is otherwise nothing to assure us that the head is not placed obliquely during the experiment; which would not much matter in the case of both eyes, as one would be depressed as much as the other was elevated, and we could take a mean. The results of the experiments performed with both eyes at the same time, are given in the third column, and included in brackets. They represent the centre of the optic nerve as elevated above the plane passing through the visual axes of both eyes; in one experiment 1° 18′, and in another, 1° 5′. I say the plane containing the axes of both eyes, because it is evident that many planes might contain the axis of one eye. Taking the mean of these values, we have 1° 11′ as the probable elevation, from experiments on both eyes at the same time. I have not, however, discarded the others, as they are uniform in indicating an elevation, though it may not be, and is not probably correct in its amount.

In most of the experiments with the mirror, the vertical was somewhat longer than the horizontal diameter. I had reason to think, however, that this arose from the flame of the candle being longer in the vertical than the horizontal direction; and when I made use of the cylinder of paper, with a round hole in the side, to expose a part only of the flame, this difference at once disappeared.

Experiments such as the above, if repeated carefully, would give a means of determining the part of the retina on which the visual axis rests, or whether it coincides with the symmetrical axis, or what is the amount of its deviation from it if it does not. M. Le Cat considered the diameter of the insensible spot to be about one-third or one-fourth of a line. Daniel Bernouilli found it to be about one seventh part of the diameter of the eye, and Dr. Young made it about 5°; while my experiments, performed in the same manner with candles, represent it as about 3°. Such differences are of less importance when considered with reference to the varying circumstances mentioned above, on which they depend. I dare say a small point of light, of exceeding intensity, would assign a very small diameter to the insensible portion of the retina, if it was capable of discovering it at all.

AN ACCOUNT OF SOME CASES OF SLOWNESS OF THE PULSE*.

By Herbert Mayo, F.R.S.
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In the hope that the narration will not be unacceptable to the scientific body to which I have the honour to present it, I have drawn up the following account of several cases of unusual slowness of the pulse.

I have not included among them instances of failure of the pulse *occurring towards the approach of death*, when, under complicated disease, or the operation of some strongly prostrative force, the heart's action flags, the organs of animal life continuing in more or less undisturbed possession of their functions.

It is thus that in Asiatic cholera consciousness persists some hours after the arterial pulse has been extinguished: and that in many other cases of mortal disease, the powers of the mind remain for days and weeks, the heart beating at half, or a third only, of its normal frequency. The most remarkable instance of *this* kind on record, is given in the 17th vol. of Duncan's Med. Comm.—"A person at the age of 54 was one day suddenly taken with a transient fit, in which he lost his senses; this was followed by three other similar fits, after which he lived between two and three days." The following is the

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account of his condition the day before his decease :—" He had been very faint almost the whole night, and had been attacked with frequent fits, attended with convulsions, and everything he attempted to take seemed to have the effect of inducing a fit. He now felt, at their commencement, a violent pain which darted through his head, but when free from the fit, he was perfectly recollected and distinct. When I numbered his pulse," continues Dr. Spers, " I found that it beat only 10 strokes in the minute, although it still continued equally strong and regular as before. I ordered him to take a glass-full of whisky, after which he remained for an hour pretty quiet and easy, and his pulse rose again to 24 strokes in a minute. But at 3 in the afternoon, I found that his pulse was only 9 in the minute, and it was neither so strong nor so regular as before. He was now in great distress from constant sickness and faintness, but perfectly sensible and collected. At 7 in the evening, I found his pulse still 9 in the minute, but much weaker. He continued sensible, but was unable to speak. He was not, however, affected with any more returns of convulsions, but he was observed not to move his right hand or leg afterwards. He expired at 9 the following morning."

" Upon examining the head, two ounces of a watery fluid were found in the ventricles of the brain, and a gelatinous appearance was observable on some parts of the pia mater."

Passing by the consideration of instances of the foregoing class, I propose in this communication to exemplify two kinds of slowness of the pulse, *which are compatible either with ordinary length of days, or, at all events, with the continuance of life for an indefinite period.* The instances which I shall narrate, with two exceptions, either have been witnessed by myself, or have been given to me by medical practitioners who witnessed them. They are of two kinds. In one, the rate of the pulse is known to have been always the same, or was not known at any former period of life to have been different. In the second, the pulse has been originally of the average frequency, and has dropped to a slower rate through the influence of causes that, for the most part, were determinable.

I. The first case which I shall narrate exhibits a remarkable slowness of the pulse, which, it may be presumed, has existed (with allowance for years) from birth, as a constitutional peculiarity. Mr. Lennard, of Craven Street, who had attended the patient, introduced me to him, and gave me the opportunity of verifying the following particulars, which the subject of them gave me permission to communicate :—

Mr. T. H. is 35 years of age, 5 feet 6 inches in height, thin, and of a slender frame, his complexion delicate, with a colour. About 14 years ago he became aware that his pulse was slower than that of other persons : he was in perfect health. Four years ago Mr. Lennard first counted his pulse; he found it to be about 40. On the 16th of March, in the present year, on counting it twice, after he had walked to Mr. Lennard's to meet me, I found the pulse 38 and a half. It is regular, full, and strong. The action of the heart, as heard by the stethoscope, is perfectly healthy. Mr. Lennard attended this gentleman on one occasion for headache, with great depression of spirits ; the pulse during this indisposition rose to 52. Mr. H. is now in perfect health ; has a good appetite and good digestion; sleeps soundly, and equally well on either side, but by preference on his back ; he sleeps from six to eight hours ; he takes considerable exercise, and supposes himself to be as capable of bodily exertion as other persons of the same slight frame as his own. His father and mother died of consumption, the former at the age of 40, the latter about the age of 35.

II. The next case which I shall narrate was communicated to me by Mr. Hewlett, of Harrow. It is that of a clergyman, who died at the age of 32, after a few days' illness, of obstruction of the bowels caused by narrowing of the colon. " Till this fatal illness, the Rev. Mr. B. had enjoyed excellent health ; he was exceedingly active in mind and body; few people excelled him in conversational talents; and he would run, and frequently did, and skip about like a schoolboy. I have known him," continues Mr. Hewlett, " I have known him, in the playfulness of his temper, declare he could *jump* further than I could, nor did our hills embarrass his breathing in the slightest degree." He frequently, however, like most other excitable persons, had fits of languor and depression. His pulse was regular at 36; his only bodily

ailment was a disposition to torpor of the bowels and dyspepsia. Aloetic aperients, with bark and ammonia, resorted to on such occasions, would greatly benefit him, and his pulse would sometimes rise to 42, or 44, when he was pursuing such a course of medicine. He had experienced *one* attack of fever, when the pulse at the onset was so high and forcible as to lead Mr. Hewlett to bleed him.

When the body was examined, commencing ossification of the aortic valves was found.

III. The particulars of the third case which I shall mention, were given to me by Mr. Arnott, in the words in which he had received them from the medical attendant of the party.

"About 10 years ago, he, being then 67, first remarked the slowness of his pulse, since which time it has never been above 36 strokes in a minute. In general it beats 28 or 29: when I last saw him," says Mr. Arnott's correspondent, "a few days ago, it beat 29. When it beats 36 he feels very unwell, having thirst, and other symptoms of fever. He has remarked his pulse as low as 25. He has had no complaint since it became so low, except that about six years ago he had jaundice. I did not see him at that time; he consulted Dr. Abercrombie, who was much struck with the slowness of pulse, but he does not remember whether during the course of the disease his pulse varied much. It never intermits, or becomes irregular. His intellect is clear; and he enjoys very good health for his age, As a landed proprietor he is accustomed to superintend his farming operations, and rides occasionally eight miles to market."

The next case was communicated to me by Mr. Jones, of the Strand, in the following statement:—

IV. "Mr. —— has been known to my father for forty years; but until five years ago he had such excellent health, as to require no medical attendance. At that time I was sent for by him, and found him suffering with oppression of the chest, swelled legs, and a sensation of fluttering in the head. He told me that he had for many years believed that he was of too full a habit, and had therefore restricted his diet to a very small allowance, never touching either beer, wine, or spirits. His age was 70. His pulse very regular at 38 to 40, never intermittent. His symptoms were relieved by antispasmodics, opiates, and aperients; and afterwards being put upon a generous diet, in about four months he got perfectly well. As his strength returned, his pulse got down to 28, and was very regular. He remained well for two years, when the sudden death of one of his servants so affected him, that he suddenly lost the recollection of the names of persons, places, and things (for instance, he would call a house an apple); still he retained his reason perfectly, and well knew what others were talking of, and would discover in a moment if they spoke incorrectly, and would recollect the names of things when set right by a friend, though in two minutes he would forget them again. In this latter respect he has gradually got better, and can now converse freely upon any subject, though he finds it very difficult to write correctly. But his former symptoms have never returned. A few weeks since he had a severe cold, disordered bowels, cough, and slight fever; and on one day his pulse was as high as 55; but as he recovered, it gradually subsided to the original standard of 28, where it now remains."

In the cases which have been narrated, the rate of the heart's action appears either to have been originally, or at some unobserved period to have become, permanently different from the average rate. In these cases there is a strict analogy to those in which the circulation of the blood is normal. Acceleration of the pulse in both is a sign of disordered health; although, in the cases which have been recently considered, the acceleration has been an approach to the average frequency of health. A like phenomenon is observed in cases in which the action of the heart is naturally irregular. In these, of which I have witnessed one instance, the heart's action only becomes regular when some febrile disturbance is present.

In the second class of cases which I propose to exemplify, the acceleration of the pulse is, on the contrary, always attended with improvement of the health and bodily feelings. It differs again, as it has been premised, essentially from the preceding kind of acceleration, in that it may be traced to the operation of more or less obvious causes, which have interfered to depress the circulation. The surprising part of such cases is, the

degree and permanency of the depression of the circulation, without material impairment of the general well-being of the patient; yet even this is rendered less surprising through the consideration of the classes of cases before adverted to.

The cases which fall under the present head result from either of several causes:—the heart beats slowly either in consequence of disease or lesion of the brain—or of general depression, exhaustion, and reduction of strength—or from sympathy with the digestive organs, or from ossification of the coronary vessels, and atrophy, with or without passive dilatation.

I shall not offer examples of the first kind, of lowered frequency of the pulse attributable to cerebral lesion or disease. In its two varieties, when the effects of the lesion wear off, and the pulse resumes its former rate, or when those effects are aggravated, and death ensues, this pathological phenomenon is necessarily familiar to the present audience; to whom, in the latter alternative, it was strikingly presented by Sir Henry Halford in his account of the last illness of the late Earl of Liverpool. But I shall proceed to narrate without any further comment, (*as the physiological and practical deductions from them are obvious*) instances, which, while they exemplify the force of the influences to which I have attributed them, at the same time present other features that may not be uninteresting.

V. The first case, which I shall bring forward, is that of a young lady, mentioned by Mr. John Scott, who, when under the care of Mr. Pennington, for neuralgia of the face and throat, was recommended by Mr. Pennington to abstain entirely from taking food by the mouth. Nutritive enemata were administered twice or thrice a day, composed of beef-tea, with an egg dissolved in it, for which veal, broth, and milk, were afterwards occasionally substituted. At this time the secretions were much disordered; there had been no appearance of the catamenia for nine months; the pulse was frequent, quick, and irregular. After pursuing the plan just named for four days, very considerable excitement was produced, attended with some degree of fever, quick pulse, and flushed cheeks, &c.; but it shortly subsided, the pulse becoming less frequent, and the fever disappearing. At the end of a fortnight, bilious secretions were observed, and in a month the catamenia re-appeared; the pulse was between 70 and 80, and not deficient in strength. *After the lapse of five weeks and three days, without any nourishment being introduced by the mouth, the pulse suddenly sank to* 35 *in a minute*, and it was then deemed advisable to give by the mouth a dessert-spoonful of beef-tea twice a day; and this being continued for four days without inducing any return of the spasms, a small piece of fish was then allowed, and then some chicken; and, proceeding thus cautiously, in the course of a month she was able to eat and drink anything without the slightest inconvenience.

VI. The next case which I have to mention, exemplifying the same principle, is that of a surgeon with whom I have the pleasure of being acquainted; but the narration of whose case I communicate in his own words, as sent by him to Dr. Hope, and by Dr. Hope conveyed to me.

"My dear sir,—In reply to your note relative to the pulse affair, it occurred many years ago, while I was a pupil of Mr. Abernethy.

"I had sat up six successive nights with a lady, whose life was very valuable, and who required constant watching, without sleep in the day. The seventh night I had the leisure, but not the power, to sleep, or rather not the inclination. This was followed by an attack of fever, which lasted some ten days, and for which I was treated by Dr. A. doubtless *secundum artem*. Mr. Abernethy saw me daily. At my urgent request, he ordered me—Calomel gr. iv. jalapæ gr. v. 3tiis horis, to purge me, and remove, as I fancied, some obstacle to my more rapid recovery. The physic did its work, and almost liquified my solids. My pulse fell, as I have told you. The doctors were, as I believe, really alarmed, but I was not. My pulse remained as low as *about* 30, I think for a day or two; it then rose to 40, and remained there for many days. Notwithstanding nourishing food and constipation, judiciously ordered by Dr. Ashburner, many years elapsed before it rose to 70."

VII. The next two cases involve more complicated elements. A gentleman, aged 52, of a full habit, easily excited, accustomed to live freely, and to take much exercise, having had one

or two transient giddinesses or sensations as of *losing* himself, after a day's shooting on the first of October, 1835, on going down into his cellar had a stronger and more threatening return of the sensations; but he recovered himself without falling. Dr. Ferguson, whom he consulted, prescribed reduction of diet and aperient medicine. The same sensations, however, returned. His diet was then still further lowered; and in the course of a year he lost, by this means and by purging, four stone in weight. He now had a long interval of improved health. But towards the close of the next summer, the lowering plan being continued, the occasional faintnesses reappeared. His pulse had become weak; but it was between 70 and 80 in frequency. He was now seen by Dr. Watson and myself in consultation with Dr. Ferguson, and it was agreed that a more liberal diet, with two glasses of wine at dinner, should be allowed him. He improved considerably upon this change being adopted; when, being in the country, he had an attack of sore throat, with great debility, for which the surgeon who attended him found it necessary to increase his allowance of wine. He returned to town, took less wine again, and was again engaged in very harassing business; at this time his pulse was first observed to sink. On the 1st of December, 1836, his pulse was 39; he was again seen in consultation by Dr. Watson and myself, with Dr. Ferguson. On listening to his heart, nothing now was heard but a blowing sound at the time of the systole of the ventricle; no second sound was perceptible. But he had no symptom of deranged circulation, except some shortness of breathing after walking up hill, or going hastily up stairs. It was agreed to increase the quantity of nutriment and stimulus which had been allowed him. He still, however, continued involved in very anxious mercantile business; his pulse gradually dropped to 34 and 30. On the 23rd of January, having taken a saline aperient draught, prescribed by a lawyer, he fell, when at the water-closet, in an epileptic fit. I saw him in this state with Dr. Ferguson; his pulse was weak, and had fallen to 27: as soon as he could swallow, hot brandy and water was given to him; his pulse rose two or three beats, and he gradually rallied. Towards evening he had completely recovered his faculties. He was considered now i very imminent danger, coupling togethe the presumed valvular disease of the heart the epileptic seizure, and the slownes of the pulse. But from that time, (wit some considerable interruptions, when h has temporarily fallen back), he ma be said to have exhibited a slow bu progressive amendment in health, not withstanding much occupation in busi ness, which circumstances have rendere unavoidable. The plan of regime since followed has been the use of mo derate but nutritious diet, with a pin of wine daily. He has had no retur of epilepsy; but when he has bee threatened, which has once or twic happened, with giddiness, a dose o brandy and hot water has restored hi circulation and strength. During th cold spring of 1837, his pulse, while hi general self-feeling was improving, fel in frequency, to 28, 26, 24: on one day on which he had imprudently expose himself to a chill, I found that his pulse had fallen to 21; but he was not aware from any inward sensation, that it had sunk so low, and he walked and talked as usual, and as if in perfect health. His pulse for the last three months has been from 29 to 32. The only additional circumstances to be mentioned are, that the blowing sound at the ventricular systole remains as before, *neither increased nor diminished*, but the second sound is now, and early indeed became, distinct and clear; and that there was a short interval before the revival of the second sound, when two imperfect actions of the ventricle could be heard between the stronger ones, which alone were attended with a pulse at the wrist.

The next case which I propose to mention, is that of a medical gentleman, who has now attained the age of 62, and, in a long career of successful practice, has become known to many of those who are now present.

VIII. Up to the age of 59 he had enjoyed, *with two interruptions*, good health. He had great activity of mind and body, well fitted for the latter by a light and spare frame; his height is five feet nine; and his complexion is still ruddy and healthy. The only ailments he had been subject to were pains in the bowels, which he attributed to flatulence; his pulse was regular, and 72. His two illnesses had been, one, severe headaches, which supervened upon domestic distress: the second was

the influenza, in the spring of 1834; for which, as it was attended with fever and much pain in the head, he insisted on being bled; he was bled at once to 25 ounces, and he afterwards lost 25 to 30 ounces more. This injudicious self-management was followed by temporary extreme weakness of body and of mind, and such impairment of vision, that he was nearly blind. In a few weeks, however, he appeared to have completely recovered, and continued quite well for more than a year. One night in September 1835, he went to bed in perfect health, and slept well; when, having risen, on walking to his dressing-table, he had a sensation in the chest as if something turned over. On feeling his pulse, he found it beat 30 or 32 only. He took stimulants and other remedies with no effect; when, on the third evening, he became sick, vomited, and threw a considerable quantity off his stomach, when his uneasy sensations *at once went off*, and his pulse reverted to healthy frequency. He remained after this perfectly well till April 1836, when, *again, on rising in the morning*, uneasiness of the chest, and a sudden slowness of the pulse, supervened. This remained between three and four weeks in the same state, and was totally unrelieved and unaffected by every plan that he tried. On a sudden, in the afternoon, he felt that he was well, and the pulse beat 68. He continued well till the April of 1837, when he was again attacked; but this time in the evening, and after fatigue. Since then his pulse has never returned to its proper frequency. When I felt it a few days ago, it was 33. It has been as low as 22.

The most remarkable feature in this case is one already mentioned—*the suddenness of the invasion, and of the disparition of the symptoms on two occasions.* But there are others of great singularity. Sometimes the heart has neither been heard nor felt; and on one occasion its beating was discernible, and alone discernible, on the right side. Again, the uneasiness of the chest is a sense of general constriction across the front of the chest; but it is unattended with any sensations in the scapulæ, or in the left arm, while pain is felt in the insertion of the right deltoid, or rather in the course of the long tendon of the biceps; and that pain may be brought on by either raising or carrying backwards the right arm. The lungs are perfectly healthy. On two occasions the ankles have become swollen; they are not so at present.

It has been mentioned that, in this case, stimulants have always failed in restoring the action of the heart. One rule alone seems capable of contributing to this patient's comfort. This is, to observe the greatest caution and gentleness in motion; stooping, raising the right arm, any hurried exertion, suddenness in rising or lying down, produce pain and constriction of the chest, and pain of the right shoulder. Avoiding these, this gentleman appears in perfect health; his complexion is ruddy and healthy; his appetite and digestion are good; and to the ear his heart acts healthily with no murmur or peculiarity, except that perhaps it beats over a somewhat larger surface than is natural.

It may not be uninteresting to mention, that in the two last cases narrated, the breathing is gentle, and of the common frequency. It is the same in the case shewn to me by Mr. Lennard. So the ratio which has been held to exist between the frequency of breathing and of the ventricular systole, is contingent on other circumstances, in one extreme. So, likewise, in the other. On the 21st of March I amputated the leg of a patient above the knee for albuminous sarcoma of the fibula. The leg was becoming swollen, in consequence of the tumor having been probed two days before, and was intensely painful. The pulse was 140, but the breathing was gentle and not hurried. The pulse sank to 120 by the night, and has gradually dropped to 96. (I may mention that the patient has done well.)

I will conclude with the notice of a case still more extraordinary than those which have gone before, in reference to the immediate object of the present paper. The outline of this case, indeed, is very brief, and leaves some points untouched, on which it would have been desirable to have had fuller information; but of its correctness no doubt will be entertained, when I mention that, although given from recollection only, it was communicated to me by Sir Astley Cooper.

About twenty years ago, a person towards 40 years of age was an out-patient of Guy's Hospital, under the care of Dr. Cholmondely and Mr.

Stocker; there was some embarrassment in his breathing, and he was supposed to have water on the pericardium, for which he was treated. His pulse was usually 29 in a minute; but on one day that it was felt, in his visit to the hospital, it was found to be below 14 in a minute: it beat 27 times only in two minutes. In three months this patient entirely recovered; the oppression at the chest disappeared, and the pulse returned to the natural standard.

CLINICAL REPORTS.

By John Burne, M.D.

Physician to the Westminster Hospital.

Of the Spotted Fever.

During the past winter, and particularly since Christmas, a great many cases of the spotted fever have been admitted into the Westminster Hospital, and some have arisen in the hospital itself. In all of them the character of the fever has been adynamic*, viz. low or typhoid in a greater or less degree, presenting the usual signs of prostration of strength, delirium, deafness, pungently hot skin, dry brown tongue, sordes about the mouth, flatulent belly, and so on. The spotted skin has been very remarkable, altogether unlike the petechiæ which are constantly seen in the ordinary adynamic fever of this country: the spotted eruption in the present epidemic has appeared and run its course with a degree of uniformity and regularity which never before fell under my observation.

The eruption varies from distinct smooth red spots, a line or a line and a half in diameter, to a generally suffused and purplish brown mottling of the skin spread all over the body, the spots even when most vivid having a purplish tinge. They differ from petechiæ in being much larger, redder, more vivid; in the colour shading off at the circumference, the line of which is therefore not marked and abrupt, as in petechiæ. The suffusion and mottling of the skin resembles very much the brown colour of measles, or of scarlet fever, when these diseases assume an adynamic character; and the resemblance to the latter is more striking from the additional accompaniment of sore throat, which I have witnessed on some occasions.

The course of the disease is very similar to the regular exanthemata, there being a distinct precursory febrile stage, and an eruptive stage, of pretty uniform duration, concluding sometimes with desquamation. The attack commences suddenly with severe headache, great prostration of strength, chills and shiverings, succeeded by pungent heat of the skin, delirium, restless nights, and all the other signs of fever; and about the fourth or fifth day the spotted or mottled eruption appears, and having continued five, six, or seven days, fades and passes away, the skin desquamating afterwards in some instances.

The most important point connected with this fever is the fact that *a latent and dangerous pneumonia* exists in almost every patient, and is so obscured by the prostrate, stupified, and delirious state, as easily to escape detection; when a speedily fatal termination is the pretty sure result. My attention was roused to this circumstance by the first case that came under treatment; and in all that have succeeded I have detected more or less pathologieal congestion of the lungs, the danger being in an exact ratio to the degree and extent of this inflammation.

In the treatment I have relied on mercury, which, administered in a decided manner, has been attended with the happiest results. Blood-letting was out of the question, owing to the adynamie character of the fever, and the prostrate condition of the patients, most of whom had been subject to all the privations and distresses of squalid poverty. In one case only did I venture to draw blood to the amount of eight ounces; and I am satisfied that a few ounces more would have turned the balance against the patient, notwithstanding he was athletic in frame, in a less prostrate condition than many of the others, and labouring under a violent double pneumonia. The objections to blood-letting applied equally to the use of antimony, which, moreover, it was not necessary to have recourse to, the mercury proving so signally efficacious. The preparation employed was calomel; the dose five grains every three, four, or six hours; and when, from the prostrate

* See my Treatise on the Adynamic Fever. The title adynamic fever is intended to include the putrid or malignant fever of Sydenham; the synochus, typhus mitior and gravior, of Cullen; the epidemic of the Irish writers; the contagious of Bateman; the typhus of Armstrong, &c. &c.—Page 8.

nd stupified state of the sufferer, it was oubtful whether the calomel was swal-owed, recourse was had to the unguen-um hydrargyri, two scruples of which vere rubbed in every three or four hours. However low the patients, wine was prejudicial until the inflammation had been subdued, when it became requisite, nd was of much service.

Case III.—*Adynamic Spotted Fever, with Purulent Pneumonia.*

One of the nurses of the hospital was he first patient who came under my care, affected with the spotted fever. She had already been ill several days, nd was now lying upon her back in a very stupefied somnolent condition, with he eyes closed. She had become deaf, nd it was not without difficulty she was roused either to answer questions or to take her medicines.

The face and the whole of the body were deeply suffused and mottled, the suffusion being of a dusky red, and the mottling of a purplish brown red. She had a sore throat, a red tongue with a brown surface, black sordes on the teeth and lips, and a pungently hot and dry skin; the pulse was frequent, fluent, and compressible, and she was constantly in a state of low muttering delirium. There was no embarrassment in the respiration to attract attention, nor did she cough in my presence. The aspect of the case impressed me with a notion of scarlet fever of a very adynamic character; and a treatment was prescribed accordingly. Next day wine was allowed to the extent of four ounces, which she drank in part only; the remainder she refused, because it "made her hot;" it was therefore discontinued. At the succeeding visit I found her coughing; and observing the respiration to be rather frequent and difficult, I examined the chest without delay. The anterior parts of the thorax, as she lay, resounded well on percussion, and the respiration was vesicular; but having raised her up in bed, I discovered the crepitating wheeze humid and soft, distinct, nevertheless, in the supra and infra spinatus, and lateral regions of the left thorax, which declared the existence of a pneumonia in the corresponding portion of the upper lobe of the left lung. Calomel was ordered every four hours, in moderate doses, without avail. She grew rapidly worse, and died in the course of two days.

The *sectio cadaveris* confirmed the diagnosis. The whole of the superior and posterior part of the upper lobe of the left lung was dense, almost solid, from purulent infiltration, and, as it were, swollen, the anterior parts of the lobe having collapsed by expiration. The pleuritic surface of this diseased lung was of a pale greyish and yellowish colour, with a tinge of red here and there interspersed with black lines, a combination of colour resembling very much some kinds of granite polished, as has been observed by Laennec; and incisions being made, there exuded from the cut surfaces an opaque, yellowish, greasy, and decidedly purulent matter, tinged red more or less by the blood of the divided vessels. The knife was smeared with the same purulent matter; and the lung being pressed, a quantity of pus oozed from innumerable points of the cut surfaces. The whole of the diseased portion presented the same anatomical characters, and was free from the slightest trace either of engouement or hepatization. The rest of the lungs sound, as well as the heart and abdominal viscera.

Case IV.—*Adynamic Spotted Fever, with Pneumonia.*

Thomas Mahony, aged 31, admitted into the Westminster Hospital on the 21st February, 1838. Having got wet on the 9th instant, he was seized with shiverings, followed in a day or two by cough and shortness of breath; he had been under medical care, and had had eight leeches to the chest.

He was a tall, large-boned, thin Irishman. He had a dusky mottled suffusion, bordering on purple, of the face and whole surface of the body, suffusion of the eyes, a dry brown tongue, sordes about the mouth, a pungently hot skin, and flatulent abdomen. He lay on his back in a heavy, stupefied, somnolent state, free, however, from delirium and from head-ache. He had a rough hoarse voice, and a severe jarring and lacerating cough, with rather frequent and somewhat difficult respiration. In this instance the pulmonary affection was so prominent as at once to attract attention—an exception to the generality of cases.

The resonance in the superior region of both sides of the thorax was good; the respiration loud, coarse, broncho-vesicular, with here and there a dry

sonorous wheeze. In the inferior regions there was muco-crepitating wheeze, with dull resonance, and entire absence of vesicular murmur; the respiration 38, thoracic and abdominal; the expiration rather prolonged; the pulse 128, short, smart, transient stroke, with a sense of falling back. *Diagnosis.* — *Adynamic inflammation of inferior lobes of both lungs; congestion of the large bronchi, with inspissated mucus.* This patient was in a highly dangerous state: the first question was that of blood-letting, which I determined on to the extent of eight ounces, because, although he was very prostrate, he was free from delirium, and the thoracic disease was very urgent.

Sanguis è brachio ad. ℥viij. detrahatur. Hydrargyri Chloridi gr. v. quartâ quâque horâ. Enema commune statim.

Feb. 22nd.—To-day his general aspect is improved; there is less stupor, the eyes are less suffused, the skin rather less dusky, the cough not so hard; the respiration still 38, but the inspirations are deeper, and the expiration freer; the pulse 120, compressible, and soft; the sonorous wheeze less dry and less loud, and the respiration superiorly is more vesicular than bronchial: the crepitating wheeze not altered; the skin is no longer pungent, but soft, and of good temperature; the bowels have been freely open; the dejections, which yesterday were dark and offensive, are to-day of good character; the urine is scanty, brown, and rather acid; and the dryness and sordes about the tongue and mouth are diminished, and moisture beginning to appear; the blood was neither buffed nor cupped.

23rd.—The cough troubles him less; the respirations are reduced to 33, and there is decided improvement in the pulmonary symptoms; but the pulse is accelerated to 126, very compressible; his face is growing sharp, of a leaden pale colour, and anxious; the tongue is less coated, but dry and brown; he is very low and exhausted, and asks anxiously for support; the bowels not open.

Haustus Rhei statim. Hydrargyri Chloridum minuatur ad gr. ii. tertia quâque horâ. Julepum Ammoniæ quartâ quaquâ horâ. To be allowed beef-tea, one pint of milk, and four ounces of gin.

25th.—The nourishment has rallied him very much; he enjoyed the beef-tea and milk, and the gin has not heated him; the expression of his eye and face is quick and good, the leaden hue nearly gone; the crepitating wheeze inferiorly is diminishing, and bronchial respiration taking its place; the sonorous wheeze superiorly is scarcely heard; and the respiration, though frequent, is free. The tongue is growing moist, and the brown coating and sordes disappearing. The pulse is reduced to 100, of fair strength, and the skin is cool.

To continue the same nourishment, and the Ammonia and gin. To take the Calomel only twice a-day.

From this period he improved steadily, though slowly, the broncho-vesicular respirations gradually taking the place of the crepitating wheeze; the adynamic signs passed away, leaving him emaciated and weak to the greatest degree.

On the 2nd March, the cough still troubling him, I examined the chest. The inferior lobes of the lungs, the seat of the pneumonia, had nearly recovered the vesicular murmur; but in the anterior superior region of the right thorax there was bronchial respiration and bronchophony, announcing the probable existence of tubercles.

The calomel, which had slightly affected the mouth, was omitted.

Morphiæ Acetatis, gr. ⅓ omni nocte. Decoctum Guaiaci compositum cum Ammoniæ Sesquicarbonate ter quotidie.

Under this treatment Mahony's health improved steadily, and on the 12th he was reported convalescent, the hoarseness having nearly passed away, the cough subsided, and the crepitating wheeze having being replaced by the natural vesicular murmur; the state of the summit of the right lung unchanged.

A week subsequent to this, Mahony was seized with hæmoptysis suddenly, from which, however, he rallied, became again convalescent, and was on the point of leaving the hospital, when on the 11th April the hæmorrhage returned to a large amount; in three days after which he sunk and died.

The friends, all Irish, would not permit an examination.

Case V.—*Adynamic Spotted Fever, with Pneumonia.*

Edmund Whitworth, age 32, ad-

mitted into the hospital the 27th of February, 1838; a strong man of middle stature, who had enjoyed perfect health until the present illness. Ten or twelve days before admission, he was attacked on his way to work with shiverings, followed by fever, pain in the chest, and cough, for which he had been under medical care.

When admitted he was excessively prostrate, lay on his back stupefied, and perpetually delirious: his face and eyes were deeply suffused, and his body and extremities covered all over with dusky-red spots; the tongue and mouth were dry, and loaded with black sordes; the skin pungently hot; the urine brown and scanty; the belly flatulent; the pulse 116, small and weak. He was deaf too, so that it was with vast difficulty he could be roused to take his medicines, or to answer questions; which last, indeed, he could not articulate.

His general aspect was that of a patient labouring under a severe degree of adynamic fever; and so little trouble was there in the respiration, and no cough in my presence, that any suspicion of organic inflammation would not have crossed my mind, had I not been warned, by the case of the nurse first related, to examine the condition of the lungs in all future instances of this spotted fever. As Whitworth lay on his back nothing could be discovered, the anterior parts of the chest presenting no anormal phenomena; but no sooner was he raised up in bed, and the ear applied to the posterior parts of the thorax, than a soft, humid, crepitating wheeze was heard over nearly one-half of the inferior and posterior regions, which yielded no resonance on percussion: superiorly the respiration was vesicular and rather puerile, with an entire absence of mucous or sonorous wheeze. The respirations, when counted, were found to be 36.

No time was lost in placing him on the mercurial treatmeat.

> Hydrargyri Chloridi, gr.v. sexta quaquâ horâ. Mistura Ammoniæ Acetatis, ter quotidie. Enema statim et quotidie.

The result of this treatment was an immediate diminution in the pulmonary symptoms, and an improvement in his general state. In four days he was reported to be losing the stupefaction and somnolency and delirium; to have the tongue growing moist, the flatulent belly subsided, the pulse reduced to 98, small and weak; the respiration freer; the crepitating wheeze mixed with bronchial respiration; the face less deeply suffused; the eyes less dull, and the spots on the body fading away. He was very low, and desired food.

> To have beef-tea, arrow-root, and four ounces of wine. The calomel to be taken every eight hours instead of every six.

After this, nothing untoward occurred. He was profusely salivated about the fifth day, and his improvement progressed in the most regular and satisfactory manner; the inferior parts of the lungs recovering, first the broncho-vesicular murmur, then the natural vesicular, in a less space of time than I could have believed possible. He took the *compound decoction of guaiacum* on the ninth day from the commencement of the mercurial treatment, and his perfect convalescence was quickly established.

Other cases, varying from this last in the more or less stupefied and delirious state, and in the distinctness and vividness of the spots, but having a pneumonia equally violent and dangerous, and equally masked and obscured by the general state of low fever, it would be unncessary to relate: suffice it to say, they also yielded to the same mercurial treatment.

Differing from the cases which have been related, are some which have arisen in the hospital itself, in patients who were under treatment for various complaints. These patients were in the same wards with those labouring under the spotted fever, and one after the other fell ill with this fever, the obvious effect of contagion. In one ward every patient not affected with the fever on admission, caught it; and the continuance of this infection has rendered it necessary to clear the ward, for the purpose of purification. No less than six or seven patients in a ward holding ten beds, have been successively attacked in the course of the last few weeks. These cases occurring under my own immediate observation, have afforded a good opportunity of noticing the manner of attack and course of the fever, as described at the outset of this paper. They have differed from the patients brought to the hospital already affected with the fever, chiefly in the less de-

grec of adynamy and of pulmonary disease, and in the greater distinctness and redder character of the spots. They have yielded to a milder mercurial treatment, and have run a course of twelve or fourteen days. One, a man affected with renal anasarca, died quickly from an intense double pneumonia; which occurrence and fatal issue may be referred in part to the renal disease, it being no uncommon circumstance for patients labouring under the granular disease of the kidney, to be seized and carried off suddenly and unexpectedly, by an internal organic inflammation.

[To be continued.]

CARCINOMA UTERI.

To the Editor of the Medical Gazette.

Sir,

Having seen the report of a discussion at the Medico-Chirurgical Society, on the subject of Carcinoma Uteri, in which the local remedies I am about to name were not alluded to by either of the speakers, I am induced to trouble you with a few remarks on the same disease, should you deem them worthy a place in the pages of your valuable journal.

I am, sir,
Your obedient servant,
George Jewel.

Golden Square,
April 23, 1838.

The term *cancer* has been usually employed to designate a disease of a malignant character which destroys the tissue or organ in which it is produced, the destructive process usually extending to the neighbouring parts, and eventually terminating in death; but the word "cancer" is seldom used among modern pathologists. It is rather more usual to consider the different forms of cancer according to the pathological character which the disease assumes. Thus, in speaking of uterine diseases, Mme. Boivin and Mons. Dugés, have divided cancerous affections into four forms, and have distributed them into four chapters, under the titles of the Tuberous Cancer, the Ulcerous, the Fungous, and the Hematode; an arrangement which is supposed to accord with the consistence and colour of the disorganized tissue. A little consideration will enable us to perceive how far the terms given to these varieties are sufficient to explain their true pathological distinctions. It is scarcely necessary to observe, that carcinoma has been the term recently employed to signify cancer, the word scirrhus being applicable to the indolent enlargement of the part diseased, and by most writers and pathologists considered as the first stage of cancer.

Now the term tuberous has been used by Mme. Boivin to designate every partial or general tumefaction of the uterus arising from some morbid change of texture incapable of resolution, and inevitably extending and becoming ulcerated, unless the divided tissues be removed by art. It has been said by Professor Andral, that hypertrophy of the cellular tissue is the first step to scirrhus. If this pathological view be correct, it is satisfactory to know that not unfrequently we find the hard and swollen condition of the cervix uteri, truly characteristic of hypertrophy, removed, and the patient restored to health. The second division of Mme. Boivin is the one distinguished by the term ulcerous cancer, a form of the disease frequently presented to the observation of the practitioner, the ulcerative process having commenced; whilst the local and general symptoms have become more unequivocally developed. Under the head of Fungous Cancer, all cancerous excrescences, of whatever form, origin, or extent, are included in the same nosological arrangement; but Mme. Boivin confines this chapter to prominent, elongated fungous growths, which spring from an ulcerated surface, not yet attended by deep erosion. The last form of uterine cancer named is the hematode, the fungus hematodes of Hey, the hematoda of Hooper, the sanguineous cancer of Duparcque.

It cannot be denied that these varieties of carcinoma possess much pathological interest; and although the prognosis must be for the most part unfavourable, yet it is certain, that in the very early period of the diseased action, a curative plan of treatment will not unfrequently prove successful. I believe that disease at first of a very simple kind; as, for instance, common superficial ulceration, will, for want of due cleanliness, or rather a removal of the morbid secretion from the divided tissue, often terminate in malignant

alteration of structure, which no therapeutical agent can remove. There are two points usually considered important: first, the severity of the local symptoms; and secondly, the colour and consistence of the vaginal discharges as indicative of the true nature of the morbid change. Neither the one nor the other can be wholly relied upon. There are, in fact, but two modes of ascertaining, with any degree of accuracy, the pathological character of uterine disease, namely, by the touch and by the speculum. Practitioners of some years standing will recollect that at no very distant period the majority of uterine diseases obtained but little medical attention; for when attended with leucorrhœal discharges, they were commonly treated by local astringents only; and upon the discovery of any morbid alteration of structure, it was too frequently pronounced to be "cancerous," and consequently incurable. The attention which has been bestowed by the profession on the pathology of the utero-vaginal system, both on the continent and in this country, within the last few years, together with the additional means now had recourse to in its investigation, has brought to our knowledge many interesting and important facts which must necessarily tend to diminish the mortality which unfortunately prevails from this class of diseases at the present moment. The increasing knowledge of the times, too, may be mentioned, as leading females to be aware of the necessity of an early application for medical advice. Few females now object to the usual examination by the touch, and the objections to an exploration by the speculum are easily surmounted. It may be said that the speculum or vaginal dilator has accomplished that for the elucidation of uterine disease, which the stethoscope has for diseases of the chest; with this difference, that in one case the viscera being concealed, their various morbid actions are recognized by the peculiarity of sounds only; whilst in the other, the tissue or part affected is brought into view, and the specific character of the disease is readily detected with the eye. The practitioner accustomed to this mode of exploration can with comparative ease and confidence discriminate between simple and carcinomatous or malignant ulceration, between inflammation and hypertrophy of the cervix uteri, between fœtid leucorrhœa and organic disease. He can discover whether there be excrescences about the margin of the os uteri, or granular inflammation so well described by Mme. Boivin. The importance of this addition to the means of diagnosis is so obvious as scarcely to require any further remark.

The period of life most obnoxious to the disease, the pathological condition of the cervix uteri, the train of symptoms to which it gives origin, and lastly, the mode of treatment which offers a probability of the removal of the local disease, or which will alleviate the sufferings of the patient, must all be considered. It is a well-established fact, that a majority of uterine diseases become developed at that period of life when the natural functions of the uterus become irregular, prior to their cessation. I believe the disease in question seldom commences subsequent to the final cessation of menstruation. According to a table to be found in the work of Mme. Boivin, of 409 cases of cancer of the uterus, the greatest number occurred between the ages of 40 and 45. It is also well known that the disease more frequently attacks females who have been married without having a family, and particularly those of great nervous susceptibility and of inactive habits. A woman whose uterus has been weakened by frequent abortions, who has been subject to great irregularities in the menstrual periods, will be more liable to carcinoma uteri than others. It is scarcely necessary to dwell on the local symptoms. They are so analogous to those which arise from local inflammatory action, not perhaps of a specific kind, or severe leucorrhœa, that it is only by examination with the finger, or by the speculum, that the disease can be fully and decidedly ascertained. It is true that occasionally in the incipient stage of the disease the diagnosis is somewhat difficult; not so in confirmed carcinoma, when the local and constitutional symptoms, the general appearance of the patient, the alteration in the uterine structure so obvious to the touch and to the sight, all declare with certainty the true nature of this frightful and destructive malady. To shew the insidious nature of the disease, it is only necessary to allude to its coexistence with pregnancy. I am acquainted with the case of a woman who had been labouring under unequivocal

indications of carcinoma for many years, during which she conceived twice; on one occasion only going to her full period. One question naturally presents itself, namely, is there a cure for scirrhus? In the year 1830 I wrote a small treatise on Leucorrhœa, and in that work I expressed a belief that by active and judicious measures the disease might be cured; that I had seen a great number of cases which had been pronounced to be carcinoma, and by practitioners whose opinions were held in high estimation, terminate well, even by the employment of common remedies, and the patients perfectly restored to health. I also expressed my belief that common inflammation, attacking the cervix uteri, might terminate in malignant disorganization. This subject has been remarked on by Mme. Boivin, who does not doubt that, in the greater number of cases, cancer is preceded by common chronic inflammation, which, of course, admits of a complete cure.

It is not my intention to enter upon the treatment of cancer of the uterus, unless in the way of general remark. It is almost unnecessary to say, that if the patient judiciously applies for medical advice in the early stage of the disease, the strictest antiphlogistic plan of treatment should be adopted. The local abstraction of blood by cupping, or the application of leeches over the inferior part of the sacrum, is decidedly preferable to general bleeding, and should be had recourse to without loss of time. Should there be much tumefaction and tenderness in the cervix uteri, leeches should be applied by means of the speculum to the affected part. Purgatives regularly administered, varying in strength according to the condition of the system and of the digestive apparatus, should never be neglected; whilst perfect rest of the body in all uterine affections is indispensable, as well as abstinence from sexual intercourse. The hip-bath, twice a week, is an excellent antiphlogistic remedy. As to medicines which are said to act specifically, such as mercury, iron, arsenic, and iodine, I confess that I have no faith in them. I feel quite confident that, by a judicious perseverance in the means suggested, very many cases of inflammation in the cervix uteri, whether specific or not, would be cured.

It unfortunately happens that, as soon as an ulcer is detected, the case is too frequently considered hopeless; and it cannot be doubted that the ulcerative process, when once established, soon degenerates into a malignant and corroding disease, which eventually destroys life. The object of the practitioner is to arrest the progress of the ulceration as quickly as possible, which, if superficial, and not of a malignant character, can be accomplished without much difficulty; and if it be malignant much good may then be done. Various remedies, principally astringents, have been employed; but there are two only in which any reliance can be placed, namely, the *nitrate of silver*, and the *nitrate of mercury*. These remedies I have used extensively, without witnessing in a single case any bad effects from their application; but, on the contrary, the happiest results. In some instances the ulcerated surface has healed permanently, whilst in others a check at least has been given to the destructive process, and the patient's life has been prolonged.

One very striking case came under my observation twelve months since. I was requested to visit a lady residing about twenty miles from town, who was supposed to be labouring under cancer of the uterus. I found her greatly emaciated, and she had been confined to her bed for a period of four months. There was a copious vaginal discharge, yellowish and offensive; and every now and then profuse hæmorrhage. Upon examination per vaginam, I found an ulcer on the cervix uteri, of a dark and livid appearance, extending over the cervix as far as could be seen through the speculum. The nitrate of silver had not been employed, and I therefore ordered it first in solution, to be continued during a fortnight. At the expiration of that time it was used in the form of a paste, and applied to the part affected by means of a camel's hair brush every other day, the injection being used three times a day in the intervals. The result was most satisfactory. Little or no pain was experienced by the application of the remedy; the morbid discharge lessened materially, and became less offensive; the menstrual periods returned with regularity; and the general appearance and condition of the patient, under the treatment, considerably improved. At the present time the nitrate of mercury

is daily applied to the cervix through the speculum. It is scarcely necessary to add, that mild aperients and enemas were ordered alternately; hyoscyamus, or acet. morphiæ in camphor mixture, at night; and a light tonic during the day. It is true the disease is not eradicated, the ulceration having extended too far into the parenchyma of the uterus; but the devastating process has been kept in check in an extraordinary manner. Twelve months ago this patient was not expected by her two medical attendants to survive a month; she still lives, nor does the local disease make any progress.

The attention of the profession in this country is now drawn to these valuable agents; and it is quite certain that they may be applied as readily, and with equal success, to the cervix uteri, as to any external part of the body.

MEDICAL GAZETTE.

Saturday, May 5, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

FRENCH METHOD OF DEALING WITH QUACKERY.

In our late articles upon quackery, we slightly touched upon some of the difficulties which environ every attempt to suppress this kind of fraud. If the question is merely made one of degrees and diplomas, and men are to be punished for their deficiency in the proper parchments, public feeling will not be on the side of the law, and, in this country at any rate, the exterminatory measures would be almost null. If, on the other hand, each case is to be tried, as the lawyers say, on its merits, why, then, unfortunately,

Iliacos intra muros peccatur ex extra:

and the possessor of the most exquisite diploma, with the finest signatures, might find himself an inmate of a prison, duly convicted of being an arrant quack! Perhaps even some of those who have cried out most lustily for vigorous justice, might, like the frogs in the fable, sigh for the good old times of King Log, when on the very point of being gobbled up by successor Stork.

A case, decided about a month ago, by the Royal Court at Orleans, is really enough to make any one quake who has the smallest tinge of *charlatanerie* in his composition; we should not recommend any one to boast of curing cancer without the knife, or having an infallible liniment for sea-sickness, within twenty leagues of this redoubtable tribunal; nay, even the self-laudatory phrases of ordinary authors—"an unusual success in treating this obstinate disease enables me to say," or, "an experience of more than thirty years in various climates has confirmed my original theory"—might be unsafe if uttered near this ancient city.

It seems, that about the month of February, 1837, three doctors and a druggist left Paris, on a medical tour, with the intention of holding gratuitous consultations, and selling medicines of their own composition. Like true medical philanthropists, wishing to deduct as many units as possible from the vast sum of human misery, they took care to announce their arrival in a given town some time before the event, so that the sick and lame might be got ready for the approaching cure. This was done (with the candour of genius, despising all mock modesty) by means of bills stuck upon the walls, stating the hour of their coming, the lodgings they intended to occupy, and the various societies which had the honour of enrolling them among their members. This maximization-of-happiness plan succeeded; and wherever they came they found a crowd of patients. The gouty, the paralytic, the rickety, the leprous, the blind, the deaf and

dumb, nay, even the maimed, were brought from hamlets and villages, and eagerly awaited their cure at the hands of MM. Sabattier, Guérin, and Neirac. Nevertheless,

"Envy will merit as its shade pursue;"

and the course of true physic was not fated to run smooth. The medical jury of Beaugency having diligently perused their precursory bills, laid an information against the doctors whose merits were therein set forth; and on their arrival at Orleans their titles were strictly examined into, and their physic was confiscated.

The medical jury, however, acknowledged that the strangers, being doctors, had a right to practise physic, though not to sell drugs, especially in towns containing druggists. The jury declared, moreover, that the medicines which had been seized did possess the virtues attributed to them in the prospectus, and valued the remedies sold at 2 francs, at 1 fr., and 1 fr. 50 c. on the average.

So far so good; but this was merely the beginning of the judicial storm. The doctors had hitherto been merely punished for selling secret remedies, according to the laws on that point; but that Rhadamanthean tribunal, the *Tribunal de première instance* at Orleans, contrived, by stretching the law, to convict the unfortunate wanderers of swindling (*escroquerie*). Considering, says the Court, that the doctors posted bills at Beaugency, Meung, and Orleans, bearing in large type the words HIPPOCRATIC SOCIETY, *gratuitous medical and surgical consultations*, and that they distributed in the three towns a pamphlet entitled "Improvement of Practical Medicine, Medical Discovery, Sabattier's Mexican Sugar*;" that the accused went to Beaugency, Meung, and Orleans, calling themselves physicians of the Hippocratic Society of Paris, and sold several of the advertised remedies at exorbitant prices, prescribing them without alteration according to lithographed formulæ, whatever was the age, sex, or disease of the patients;

Considering, continues the Court, that the accused, by advertising gratuitous advice, of which the only object was the sale of their physic, by giving the name of a medical discovery to a mere mixture of jalap and sugar, and by guaranteeing a speedy and complete recovery to the most incurable patients, did, in fact, assume the title of physicians of a non-existent society, put off a mixture of two very common substances as a discovery, and excite the hopes of cure in the minds of persons whom they had attracted by fraudulent methods, and thus cheated them out of a part of their substance;

Considering all this, and article 405 of the penal code, the Court condemned Sabattier to two years' imprisonment, and a fine of 2000 francs; Guérin to fifteen months' imprisonment, and a fine of 1000 francs; and Neirac to a year's imprisonment, and a fine of 600 francs.

M. Neirac appealed from this sentence to the *Cour Royale*. It was alleged in his favour by M. Bonjour that the court below had gone too far, by applying the penalties of article 405 to the law concerning secret remedies; that where the infraction of a statute carries its penalty with it, the executive power is not allowed to borrow a punishment from a different enactment, under pretence that the proper penalty is too light; that no one had complained of any damage suffered, nor of the dearness of the drugs, nor of any bad effects from their use; but, on the contrary, that several patients had experienced the promised cure; and that the virtues of the medicines could not be

* This denomination of *sucre Mexico-Sabattier*, applied to a mixture of jalap and sugar, is not wanting in ingenuity.

called chimerical, when they were supported both by the medical jury and by the fortunate results which they produced; that there was no crime in the display of titles and honours in the placards, as they were not intended to hurt any one, but rather to lend more brilliancy and authority to truth; and finally, as concerned the sale of secret remedies, it was requisite that a useful remedy should be capable of being employed with safety by its inventor, before its admission into Pharmacopœiæ, and during the performance of its long quarantine at the gates of the official Codex; particularly when it was submitted, as the doctors had done, to all the means of publicity recognized by reason and good sense, and to every kind of scientific analysis.

The end of the case was, that the *Cour Royale* confirmed the judgment of the court below, but reduced the imprisonment to three months, and the fine to 300 francs.

Every body knows that we were once subject in England to accusations of what was called constructive treason. John Green, nettled at the rejection of his suit at court, might cry out, about the middle of his second bottle, "Kings are but men;" Tom Smith, who had been out of work for nine weeks, sighed, and said he hoped better times were coming. These criminal speeches evidently shewed a wish, if not an intention, to levy war against the king, in order to make better times, or to set a monarch on the throne who should be somewhat superior to a man; and, accordingly, Green and Smith were executed for these their constructive wickednesses.

The Courts at Orleans appear to us, with a similar ingenuity, to have established a constructive swindling, which will leave few persons in safety, whether belonging to our profession, or simple laymen.

Thus, for example, physician A asserts verbally, and in writing, that he never lost a case of cholera out of 74 which he treated; surgeon B brings out a second edition of his work on Hydrocele, with nothing new but the title-page; apothecary C exhibits in his window a puffing handbill, in which he lauds his own merits in terms that would be extravagant if applied to Sydenham or Boerhaave; practitioner D sends to Erlangen for a diploma, and exhibits it as a proof of his acquirements, though it is a proof merely of his expenditure of 20*l.* to gratify his craving for the title of doctor; grocer E obstreperously asserts that he sells the best black tea imported into this country for five shillings a pound, though the registers of the East India Company shew that the wholesale price of such tea is seven shillings and fourpence a pound, duty not included.

Whenever these attempts on the common purse are successful, as they too frequently are, one *might* say of the criminals, in the words of the Orleans judges, "*qu'ils ont par ces moyens escroqué partie de la fortune d'autrui;*" yet it would be an unheard-of rigour actually to punish the parties as swindlers.

If, however, Astræa has returned to the earth, and fixed her residence near the Loire, such punishments may become common in that quarter; but if affairs are to be conducted with ordinary lenity, the three physicians-errant have been rather hardly used. When George the Third was implored to pardon a celebrated forger, he replied, "If I do so, then Perrier was murdered;" and if the Courts of Orleans hereafter shew any compassion to human frailty, every one will be ready to exclaim that the vendors of Sabattier's Mexican sugar were martyrs. At any rate this instance may serve to shew the more zealous persecutors in our profession, how difficult is the system which they advocate,

and that even the punishment of quackery is a pursuit in which the roses are not free from thorns:

> ——— "Medio de fonte leporum
> Surgit amari aliquid, quod in ipsis floribus angat!"

THE "MYSTERY" OF OUR OPPOSITION TO DR. M. HALL EXPOSED.

IT is a thousand pities that the quotation "Quem deus vult perdere, &c." is so hackneyed: we are really ashamed to use it, yet we know of nothing so appropriate to the present condition of Dr. M. Hall. Mr. George has shown the identity of his views with those of Prochaska, by placing passages from both in parallel columns; and this Dr. M. Hall answers by informing us that Mr. George — "is young." Mr. Newport has had his most private affairs dragged before the vulgar gaze by Dr. M. Hall, who, being charged with the indelicacy of the proceeding, replies, that in all he has acted "upon a purely public principle;" and having first told us (but not correctly, as has since been proved) that he had fed, housed, and clothed Mr. Newport, now gravely assures us that he "never had any feeling of friendship towards him." These several monstrous absurdities and contradictions, which are contained in his last published lecture, we shall leave to the parties referred to, if they think it worth while to notice them*. But there is one insinuation pointed at ourselves, which we think it right to refute; and the rather as it constitutes the third time that the learned *President of Sydenham College!* has made this journal the subject of remark in his lectures. Dr. M. Hall implies that our opposition to him results from the circumstance of his having published his "important views" in another periodical;—"wherefore," asks he, "all this opposition? One fact will explain the mystery. I have gladly availed myself, for the dissemination of these important views, of the widely-diffused circulation of the Lancet." Now the fact is, that all Dr. M. Hall's views, whether "important" or not, had already been published in this journal*. So long ago as 1834, we got thoroughly sick of Dr. M. Hall and his endless repetitions, and made this so manifest, that he wrote to the Lancet to complain of us,—a proceeding which afforded such a prospect of relief that we immediately inserted the following notice, in the hope of clenching the business and getting rid of him altogether:—"We perceive that Dr. M. Hall has opened a correspondence with Mr. Wakley, in an attack upon this journal. Of all the correspondents we ever had, we will do the worthy Doctor the justice to say, that he has been the most troublesome. We sincerely congratulate our contemporary on his acquisition†." So much for our courting Dr. M. Hall! Now for the other side.

So highly did he prize the advantage of having his papers published in the MEDICAL GAZETTE, that, notwithstanding the above contemptuous notice, he actually made overtures, some time afterwards, to have his lectures on the Nervous System, then in course of delivery at Aldersgate Street, inserted in this journal. Rendered cautious by past experience, we stipulated for the insertion of one, and only one lecture, if he chose to bring his "important views" within that limit. The Doctor "gladly availed himself" of the offer, and a paper, running nearly ten pages of our small type, and containing every thing which has even a pretension to novelty in his views, will be found at page 632 of vol. xvii. 1836.

These simple, and fortunately undeniable facts, will show whether it was we who were anxious to publish Dr. M. Hall's lectures, or he who would have "gladly availed" himself of our circulation, if he could. As to the rest,—and we mean it not insultingly, but as a simple statement, which his attacks have drawn from us,—we have too much respect for the readers of the GAZETTE, and for the interests of the property, to have allowed his lectures to appear in our pages. Against this short-sightedness of ours, which he will of course regard as originating in a most erroneous estimate of his importance, his *amour propre* will easily afford an antidote; for be it known that he has already likened himself to three of the greatest men our

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* Since the above was written, we have received a letter from Mr. George, which will be found at page 252. No one can peruse it without seeing at a glance that Dr. Hall is no match for him.—ED. GAZ.

* See vols. x., xi., xiii., and xvii.

† MED. GAZ. vol. xiv. p. 32.

profession ever produced. "*Harvey* (says he) *and Haller, and even Jenner, experienced similar treatment.*" To the consolation which this reflection cannot fail to afford, we leave him.

DECEASE OF MR. THOMAS BLIZZARD.

Mr. Thomas Blizzard died a few days ago, at Brighton, whither he had repaired on account of his health, which had been for some time in a declining state. It is now many years since Mr. Blizzard retired from practice, having enjoyed the advantage, so rare in our profession, of acquiring fortune at an early period of life. Notwithstanding, however, the time which has thus elapsed since he was engaged in the busy scene, his name continued familiar to the present race of practitioners, and never was mentioned without respect, Mr. Blizzard having obtained for himself, and we believe most deservedly, the general esteem of his brethren, as an accomplished surgeon and high-minded gentleman.

NATIONAL COLLEGES OF SURGEONS.

Deputations from the Colleges of Surgeons in Edinburgh and Dublin have lately visited London, and held conferences with some members of the Council of the London College, with a view of equalizing the education and examinations of candidates, and rendering the diploma of any of these several bodies equally available in all parts of the empire. We have reason to believe that the terms of such an arrangement as cannot fail to be of mutual advantage, have been agreed upon.

WIDOWS AND ORPHANS OF MEDICAL MEN.

In the month of April, 1788, the following benevolent members of the medical profession projected the formation of a society, which should have for its object the relief of the widows and orphans of the more indigent members: these gentlemen were, Dr. John Squire, Dr. Andrew Douglas, Dr. Thomas Denman, Dr. John Sims, Dr. Richard Dennison, Mr. William Chamberlaine, surgeon, and Mr. Thomas Rendall, apothecary.

By their united exertions, many physicians, surgeons, and apothecaries, were invited and prevailed upon to become members; and on the 29th of October, 1788, a general meeting took place, at which it was unanimously resolved,

"That the members present at this meeting do declare that they form themselves into a society, under the title of 'The Society for Relief of Widows and Orphans of Medical Men in London, and its Vicinity.'"

Aided by the subscriptions and benefactions of medical men principally, though other benevolent persons have occasionally given their generous support, this society has now for fifty years continued its endeavours to give the promised relief. It is, indeed, melancholy to reflect, that the profession of physic should, in so many instances, fail to secure to its members the means of making competent provision for their widows and children; but the Society has the gratifying assurance, that, through their instrumentality, the sorrows of the widow and the orphan have been mitigated, and comfort and happiness administered to a large number of destitute women and children; no less a sum than 24,000*l.* having been distributed, since the formation of the Society, among those who require and are eligible to receive its assistance.

The annual dinner of the Society was held on Saturday, April 21st, at the Freemasons' Tavern, Sir Henry Halford, President, in the Chair, supported by about forty members and friends. A statement was made to the company by Dr. Merriman, one of the Treasurers, from which it appeared that the present amount of members is about 340; that at present, relief is afforded to forty widows, several of whom have children, and to five orphans, having neither father nor mother; and that the sum of 1400*l.* is thus annually applied in half-yearly payments.

The annual contribution is 2*l.* 2*s.*, payable by half-yearly instalments, for twenty years, after which no further demand is made; but twenty guineas paid at once is considered to be a life subscription. It is obvious that this very small annual premium could not effect the object in view, were it not that a very great majority of the subscribers consists of physicians, surgeons, and apothecaries, whose circumstances in life render it scarcely possible that

their widows and children can become claimants. The Society, indeed, from its commencement, has been fostered and supported by all the most distinguished practitioners who have lived during its continuance; and many of the most eminent physicians, surgeons, and apothecaries, of the present day, have enrolled their names in the list of subscribers.

The Society receives into its community all physicians who are fellows or licentiates of the Royal College of Physicians of London; all members of the Royal College of Surgeons; all members of the Worshipful Society of Apothecaries, or other apothecaries duly licensed by that Society, under the act passed in the 55th year of King George the Third, "for better regulating the practice of apothecaries;" provided that such physicians, surgeons, or apothecaries, be living in London, or its vicinity, at the time of their admission; and this vicinity is defined to be within seven miles of London, in the counties adjoining Middlesex, and extending to the whole county of Middlesex.

The persons eligible to receive assistance are—

1st, The widows of members, not possessing more than 50*l.* per annum, clear income.

2dly, Orphans of members, having mothers alive, whose provision does not amount to more than 12*l.* per annum, clear income.

3dly, Orphans of members, having no mother, whose provision does not amount to more than 25*l.* per annum, clear income.

Under certain conditions the Court of Directors has power to give assistance to indigent members, or to the mothers or sisters of members; but the funds of the Society require that this power should be exercised with very great discretion.

Considering all the good which this Society is capable of effecting, it seems extraordinary that it should be so little known, and its members so few: that among the great number of practitioners in London and its neighbourhood, only 340 should belong to this Society, is really astonishing, especially as hardly a week passes without an application to the medical public from some indigent widow, or destitute orphan, of medical men, for pecuniary assistance and relief. Surely there must be much of criminal negligence on the part of the husband who does not avail himself of the opportunity, by paying one guinea half-yearly, to secure to the most beloved objects of his care the advantages which this Society offers.

FURTHER OBSERVATIONS ON MESMERISM.

By Herbert Mayo, F.R.S.

To the Editor of the Medical Gazette.

Sir,

As you have been good enough to insert my account of some of the mesmeric phenomena displayed by patients of Dr. Elliotson's in the two last numbers of the Gazette, I venture to solicit space for a few concluding remarks upon this subject.

The ordinary stages through which any discovery in physiology, involving views that go greatly beyond those commonly entertained, has to pass, are, first, to be positively denied as false and contrary to experience, absurd, and unworthy the attention of sensible men. Secondly, to be shewn to have been known to, and admitted before, to a degree depriving the observer, who just now was ridiculed for having believed it, of any credit for having seen with more justness and originality than others, the disputed novelty. Thirdly, to be denounced as a perilous innovation, endangering religion and the moral bonds of society. Fourthly, and finally, to be received by every body as a matter of common knowledge, the only wonder about it being that it was ever doubted, and the supposed anomalies and contradictions in it to Nature's laws being seen to be and represented by the writers of the day as the most striking (because the newest) illustrations of the harmony of the physical world.

Mesmerism is at present in the first of the stages which I have supposed it will pass through. As soon as the industry and ingenuity of different observers shall have got together facts enough to make it a science, it will be seen to enter upon the second. But my object in writing a third letter on this subject is not the hopeless one of preventing either of the usual stages, or of hastening their term, but to point out

to such as shall have opportunities of following up these inquiries, the objects to which their attention may be advantageously directed.

I. The experiments which I have described have established the existence of an influence originating in muscular motion, that excites, in a certain number of those subjected to it, a new state of the nervous system. The step which should obviously follow this first step, is the *rigorous* observation of the same phenomena, and the *exact measurement* of the elements, which are involved in and modify their character. Nor is there reason to doubt that, in the progress of this analysis, new phenomena, of an equally surprising character with those already verified, will make their appearance.

II. It has been mentioned that the state of mesmeric sleepwalking resembles the fit in double consciousness and ordinary sleepwalking. With the former it may be said to be already known to be identical. Two of the children exhibited by Dr. Elliotson spontaneously fall into the fit, which is otherwise produced in them by mesmerism. The characters of the fit are the same, whether it is produced by mesmerism or occurs spontaneously. But this singular circumstance is to be remembered. When the fit of sleepwalking is brought on by mesmerism, it occurs *secondarily*. The *first* effect of mesmerism is to produce *coma*, out of which the patient is awakened by the manipulation described into somnambulism. The coma, on the other hand, never makes its appearance except as the result of mesmerism.

Now, in the two sisters exhibited by Dr. Elliotson, there is a wide difference as to the character of the somnambulism. In one, vision is limited to a narrow circle, and her bodily movements (unless excited by mesmerism) are so slow and measured, as to leave a doubt whether she retains muscular sensation. In the other, sight has its common range, and her movements are so sure and rapid, that it is evident that although cutaneous sensation is gone, and common feeling extinguished, yet that the sense of muscular effort remains.

Another interesting field of inquiry is thus opened—*What are the differences which are determinable in different cases of double consciousness?* This, of course, suggests another question—*What are the determinable differences in the ordinary cases of sleepwalking, which are so parallel to the fit in double consciousness?*

Another inquiry is suggested by the state of mesmeric coma—*What differences may be discovered in trances, in hysteric seizures, and in epileptic attacks, and what resemblances, through the features now disclosed, bringing their varieties nearer to, or carrying them further away from each other, and from mesmeric coma?*

III. It is evident that the solution of the preceding questions is by no means a speculative inquiry. The more completely the phenomena adverted to are understood, the greater will be the chance of our finding the means of controlling them. We possess, besides, in the mesmeric process, a new test for the discrimination of this class of pathological phenomena; and at the same time a means which it is highly probable will be capable of importantly modifying, and perhaps suppressing, some of them.

The entire subject of *fits*, in pathological science, thus promises to receive elucidation from inquiries growing out of mesmerism.

But there is another subject of still greater interest, on which the present means are calculated to throw an additional light. The subject to which I advert is the nature and treatment of insanity It will not be doubted by the many who have witnessed the phenomena which I have described, that in many cases of early insanity the nervous system may possibly be so strongly wrought upon by the mesmeric process, as to be, through its influence, strained from its wrong direction, and bent anew towards health. The numerous public lunatic asylums throughout the country furnish ample opportunities for testing the soundness of this anticipation. There is every reason to expect that distinctions that now are not thought of, will be elicited by this means, in cases of insanity, the treatment of which, as of other diseases (supposing the mesmeric agency to be no further available in it), must of course improve in proportion as their nature is better understood.

I cannot help venturing in conclusion to hope that in declaring my conviction, founded on the observations which Dr. Elliotson has most liberally allowed me to make, *that there is something in it*,

I may contribute to rescue this singular inquiry from the general contempt into which the credulity, ignorance, folly, and roguery, of some of its advocates have forced it, and to place it within the domain of common sense and legitimate science.

I am, sir,
Your obedient servant,
HERBERT MAYO.

19, George Street, Hanover Square,
April 29, 1838.

MR. GEORGE AND DR. M. HALL.

To the Editor of the Medical Gazette.

SIR,

I HAVE just read, in the last number of the Lancet, a lecture of Dr. Marshall Hall's, containing a notice of (rather than a reply to) a paper of mine lately published in your journal; and although there is in this notice little worthy of attention, yet one or two parts I am loth to let pass without a remark.

In the first place, Dr. Hall's assignment of any particular motive to me is perfectly gratuitous. I know of nothing in my paper which should lead him to suppose me a party to a "conspiracy," or to class me with "assassin writers." When I do conspire, sir, it will be against some nobler object than a querulous egotist; and when I assassinate, it will be some one who may give me reason to fear, or be worthy of hatred. I may go still farther: there is in what I have written no statement of any wish or intention to deprive Dr. Hall of his originality; no passage which can be said to assert that the discoveries claimed were not made. I say this now, not to deprecate the anger of this gentleman, but because I have done nothing more than compare old opinions with new; and the charge of plagiarism therefrom resulting is one which Dr. Hall has brought against himself in my name: let him, therefore, look to the grounds on which it stands, nor be surprised if the deduction which he himself has made from the comparison be entertained by others. My supposed opinion, as long as it is unstated, none have a right to quarrel with; and it is probable that I may have as great a regard for what I have heard called the high originality of Dr. Hall's views (which, I take it, means originality upon stilts), as for the low originality of his abuse and invective.

Secondly, It is true, sir, that I am a young man, and moreover I am right glad of it; but I am old enough to have learnt that when, in scientific controversy, one person charges another with youth, it is a sure sign that the accuser is getting very old, and that he derives more support and authority from the number of years during which he has lived, than from the life itself, or what has been done in it. Beside this, I hold it better to be a young man and suffer correction, than an old man and want it; and whilst I thank Dr. Hall for allowing that though pert I may be ingenuous, I am inclined to think that if any judgment of his youth may be founded upon his late career, it would differ from that which he has so kindly formed of me in this particular,—that though probably pert even to arrogance he certainly never could have been ingenuous.

I am sorry to have taken up so much time in replying to Dr. Hall's personalities which I thought would at least have been spared to me, as in what I wrote I was carefully guarded in my expressions lest I should give offence: this same offence however, Dr. Hall would seem to regard as a sort of common property, since he is equally ready to take it when unoffered and to offer it unrequired.

The accuracy of the opinions advanced and the comparisons instituted in my communication, I think no way impugned by what Dr. Hall has said. This, however, is a matter of judgment; he entertains one idea, and I another: most willingly do I leave it for the readers of your journal to decide between us.

But it seems, in the third place, sir, that my inaccuracies have been so great as to pass beyond the "bounds of truth." Perhaps I ought to regret that my mind is not sufficiently callous to allow a charge of falsehood made in a public journal to remain unanswered, *as some others have done;* but I cannot be easy without inquiring into the grounds of such a charge. These, as given by Dr. Hall, are three. In the first place, he says, "the assertion, 'exclusively of the cerebrum and cerebellum,' is incorrect, for the crura of both, and part of the thalami, are included." Now this I take to be a mere quibble, for the passage stands thus:—"The spinal marrow and nerves, *with the parts from which the latter take their rise*, exclusively of the cerebrum and cerebellum." It might, perhaps, have better suited Dr. Hall's misinterpreting faculties to have said the "rest of the cerebrum and cerebellum;" but others who have read it do not think the expression incorrect enough to justify a charge of untruth.

Secondly, "The phrase, 'vis nervosa of Haller,' is incorrect, for it is not the vis nervosa of Haller, as I have understood and used it, which is meant." To this I

ay, look at the words of Prochaska—'This (element), since it is so obscure and unknown, just as is the attractive power of bodies, appears to me to have been rightly named the *vis nervosa**". What was I to make of this sentence, but that which I did make of it? Or to what vis nervosa does Prochaska allude, if not to that of Haller? Oh, but it is not the vis nervosa of Haller, as Dr. Hall has understood and used it, forsooth! I was speaking of it as Haller's, and not as Hall's; therefore what right has the latter gentleman to assert that my expression is incorrect, and to found thereon a charge of untruth? With as good grace might he accuse me of falsehood, if, in speaking of Savory's steam-engine, my description did not answer to the machine as improved and used by Watt; although I do not mean by this to suppose any resemblance between the improvements of Dr. Hall on the vis nervosa of Haller, and those of James Watt on the mechanics of his predecessors.

Thirdly, "The phrase, 'the class of spasmodic diseases,' is incorrect, for such a collective phrase has not been used by any one, I believe, but myself." What a round assertion, and how modest! Surely either Dr. Hall never heard of any one but himself, or I have been listening to *his* lectures when I thought myself attending those of others; for the phrase is familiar to me from my earliest professional recollections. The term "class of spasmodic diseases" never used by any one but himself! Here is originality with a vengeance! Do you, Mr. Editor, assume to yourself some such unthought-of term as "venous congestion," or "circulation of the blood;" let there be a scramble for such undeniably novel expressions, or let original phrases, like original pictures, be valued all the more that they are by the old masters. Is the charge of untruth verified? I trust not.

It has amused me not a little, in all the controversies which I have noticed, to see how individuals compare themselves to some one of those sufferers for truth, Newton, Harvey, Galileo, and the rest; though often with one single point of resemblance, that they have been both abused. Such persons seem to forget that—even as many bad men may prosecute a good one, so many good ones may persecute a bad one. Have none else ever been ridiculed and attacked but these illustrious founders of science, or discoverers of truth? And yet the comparers entirely overlook—or studiously avoid the slightest allusion to—the numerous arrogant pretenders who have been so treated. St. John Long, with his "infallible," and Johanna Southcote, with her spurious Shiloh, were both "persecuted;" but it is not of these that Dr. Hall thinks, when he compares himself to "Harvey and Haller, and even Jenner," and "the unfortunate and amiable Lavoisier;" the comparison here is strikingly obvious, but it is only by contrast.

Dr. Hall will, however, "willingly grant all that the paragraph (quoted from Prochaska) contains, without feeling that he has sustained the loss of one jot or one tittle of what he has claimed for himself." Now what strikes me as being a very singular part of Dr. Hall's lecture, is, that after this denial of any thing existing in Prochaska which he has claimed for himself, he should be so angry and hurt that some calumnious writer had insinuated that he (Dr. H.) had read Prochaska before making known—and perhaps before knowing himself—his own discoveries. Surely, sir, if there be nothing like those discoveries in this author, Dr. Hall need not so strongly deny having seen the work. Suppose the calumnious writer were to say, that the author of the "Memoirs on the Nervous System" had read Paracelsus or Hippocrates, would the Doctor have deemed it worth while to write to a librarian, and demand an account of how often, and when, he had taken these works from the library? And why not, sir? for if there be nothing in Prochaska which Dr. Hall has claimed, there would obviously be quite as much in the others, and therefore equal calumny in stating that Dr. Hall had read them. There is not "one jot or one tittle" of what he has claimed, in Prochaska! Then why is it of consequence to affirm (which he calls proving) that Prochaska was never seen by him till three years after publication of his opinions? Dr. Hall knows, and others may imagine. According to his own shewing, however, the author of the "Memoirs on the Nervous System" had possession of Prochaska's works more than a year before the publication of those memoirs; how, then, was there not some mention made of their contents in the history of "Opinions of former Physiologists?" I can only suppose that, as the works contained not "one jot nor tittle" of what has been claimed by Dr. Hall, he deemed any notice of them irrelevant.

Allow me, sir, in conclusion, to apologize—not to Dr. Hall, but to the readers of your journal—for any expression in this letter which may be thought too harsh or too personal. I am unused to controversy, and therefore may thus have erred; but when I find that a calm comparison of passages from different works (apart from

* Op. Min., part II. p. 65.

a single statement impugning the originality of the latest author), cannot be made without being declared the result of "malignity" and "conspiracy;" its writer charged with falsehood, and classed with "assassins;" and the whole proceedings averred to be "disgraceful," I repent not of having used the strongest of those expressions, and can only regret the necessity for their employment, and the correctness of their application.—I am, sir,

Your obedient servant,

JOHN DURANCE' GEORGE.

University College,
May 1, 1838.

MEDICAL AND SURGICAL AGENCY OF THE AIR PUMP.

To the Editor of the Medical Gazette.

SIR

DURING many years, by means of various publications, I have endeavoured to direct professional attention to the medical and surgical advantages of augmenting atmospheric pressure for one class of diseases, and of diminishing that pressure for complaints of an opposite nature *.

Having explained and demonstrated the various adaptations of these two great physical powers to several meetings of the British Association, considerable inquiry has been excited, and a desire expressed by several influential practitioners in London, that some of their patients might avail themselves of one or other of these new resources of the healing art.

I find from Mr. Clarke, surgeon, 14, St. James's-place, Hampstead-road, who conducts the operative application of these great principles, that much misunderstanding prevails respecting the influence of artificial compression or condensation of atmospheric air on one hand, and its diminution or withdrawal on the other.

Anxious to remove these misconceptions, I have been induced to remain in town until the necessary discrimination of suitable cases shall be more generally understood, and to give a reasonable explanation to the profession respecting two great physical agents capable of producing local or general revulsion or determination of the circulating fluids, to or from any part of the human body, to any extent, and for any length of time required. I have had occasion to shew its power of reducing hernial protrusions to several surgeons. Cases of its efficacy were witnessed at my house, by Messrs. Carmichael, Ellis, Lynch, Duggan, and others.

That the distinction between the influence of *increased* and *diminished* pressure may be better observed, I beg to subjoin the outlines of two cases from many others.

Effect of encreasing Atmospheric Pressure.

B. R., aged 23, delicate and scrofulous; contracted swellings of both ankles, which were stiff, puffy, and painful; veins blue and turgid; inability to take exercise, with the use of leeches and mercurials, increased his condition of weakness. It appeared that the veins had lost their tone, thus detaining the fluids in the tissues and synovial membranes of the joints.

As bandages do not suit irregular surfaces, I directed an increased pressure of 2 lbs. per inch of air to be applied in the manner formerly described. In three weeks the diminished diameter of the vessels enabled them to recover their natural tone; the blood was sent upwards by the deeper-seated branches; and the swelling disappeared. The youth is long enjoying that exercise so conducive to the preservation of his health and life.

Effects of diminishing Atmospheric Pressure.

A lady, aged 25, began to complain of cold and numbness in the wrists and ankles, attended with weakness of these joints. Motion became so much impeded, that she was unable to walk or use her hands.

Part of the atmospheric pressure was withdrawn from the joints, and the blood consequently elicited into the vessels, which became red, warm, and full, and the sensibility was speedily restored. It was evident, from the improved feeling of the general nervous system, that the abstraction of so much blood from the head and spine to the extremities, was particularly beneficial in removing the probable causes of the complaint.

In treating irritable ulcers and inflamed surfaces, in reducing tumors, in abating untoward sensibility and neuralgic pains, the influence of atmospheric pressure is so effectual, easily regulated, and maintained, that it becomes a public duty to extend its sphere of usefulness.

On the contrary, where too much blood is determined to the head, chest, or abdomen, it is of immense importance to draw any required quantity of it to the limbs, until a natural balance of the circulation shall be restored. We can at any time produce fainting, and maintain relax-

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* Treatise on the Influence of Heat, &c. by the same Author. Longman and Co.
See also London Medical and Surgical Journal, 1832. Lancet, March 28, 1835.

ation as long as we desire, without reducing our patient by bleeding.

I am, sir,
Your obedient servant,
JAMES MURRAY.

Uxbridge House, May 2, 1838.

GLASGOW EYE IMFIRMARY.

Capsular Cataract extracted through the Sclerotica.

4TH OCTOBER, 1833.—Elizabeth Robertson, aged 17 years, ten years ago received a cut through left eye-brow, which has been followed by cataract in that eye. The capsule is evidently opaque, and much thickened. There is a minute chink towards lower edge of pupil, which is clear, and through which she discerns a pen and other objects, when they are held somewhat to her left side, and her back is turned to the light. The eye is affected slightly with strabismus divergens.

14th Jan., 1834.—No material change since last report.

Adhib. Belladonna.

15th.—A curved needle was introduced through the sclerotica about one line behind the margin of the iris, and the opaque capsule detached. An incision was then made with the extraction-knife, backwards from the puncture, in length about two lines and a half. The capsule was then removed by a pair of forceps; not, however, till after several ineffectual attempts, during which a considerable portion of vitreous humour escaped.

16th.—Has continued quite free of pain. Last night, as the pulse was firm, sixteen ounces of blood were taken from the arm. One ounce of sulphate of magnesia was given this morning. Pulse still firm.

V.S. a ℥x. ad xij. Rep. Sulph. Magnes. c. m.

22d.—Yesterday the eye was opened for the first time, and to-day it has been examined more particularly. The cornea is clear, the wound in the sclerotica is healed. There is still a very slight ecchymosis at the outer margin of the cornea. The pupil is still dilated, but less so than yesterday. Its figure is circular, and it is affected by light, though feebly. About three-fourths of the disc of the pupil, towards the outside, is occupied by a whitish and somewhat flocculent-looking substance. A small space towards the inner angle is clear, and through this she can discern large objects with tolerable ease.

29th.—The belladonna having been washed off about four days ago, the pupil contracted very nearly to its natural size. Yesterday a portion of the opaque matter was observed projecting through the pupil near its outer margin, and causing some irregularity in its shape. The belladonna was re-applied. To-day the opaque matter has divided into two fragments, one of which has fallen into the anterior chamber, and lies below the pupil; the other is behind, but depressed considerably below the axis of the eye, so that more than three-fourths of the area of the pupil is clear. Since the 26th, has been taking one Plummer's pill morning and evening.

22d Feb.—White matter has entirely disappeared. Pupil circular, and readily affected by light. Vision very dim, but most distinct when the image of the object falls to the inside of the axis of vision.

Capsular Cataract ixtracted through the Cornea.

9th Oct. 1835.—David Wylie, aged 62 years. Thirty years ago, while in the West Indies, his right eye appears to have become cataractous. The greater part of the pupil is occupied by thickened opaque capsule; but towards the nasal edge of the pupil there is a clear spot, through which he enjoys some share of vision. About two years ago he found vision of left eye declining, and is now able with considerable difficulty, and only by holding the object within three or four inches of this eye, to read the large letters on the Infirmary card. The humours of this eye are glaucomatous, and the lens appears partially opaque. Both pupils lively.

Belladonna ad palpebras dextras.

21st.—A curved needle being introduced through temporal side of right sclerotica, the opaque capsule was separated from its connexions, and the needle withdrawn. A quarter section of the cornea was now made parallel to its temporal edge, a hook introduced, and the capsule extracted. A small portion of hard opaque lens sank behind the iris, the vitreous humour being evidently dissolved, and a considerable quantity of it draining away by the puncture in the sclerotica.

Haust. Anodyn. c. Tinct. Opii, gtt. xl. vespere.

23d.—No redness. Wound in cornea seems united. Pupil dilated, being still under the action of belladonna. Bowels not opened since the operation.

Cap. Sulph. Magnes. ℥j.

30th.—In the site of the wound of the cornea there appears to-day a small vesicular protrusion, and the pupil is more irregular, being drawn angularly in that direction.

5th Nov.—Cap. Sulph. Quinæ, gr. j. ter indies.

9th.—Aqueous humour continued to flow through wound of cornea till within these few days, but has now entirely ceased. Pupil clear, and vision good.

TESTIMONIALS TO SURGICAL TEACHERS.

To the Editor of the Medical Gazette.

Sir,

Will you permit me to correct an observation in your leading article of last week, relative to the examination which future anatomical and surgical teachers are expected to undergo before the Council of the College of Surgeons.

During the past summer I voluntarily passed that examination, although I had taught anatomy publicly previously to the enactment of the law. My colleague, Mr. Dalrymple, also has his name before the College as a voluntary candidate.

With regard to the "promise of special testimonials," I can only say that I was not aware of any such promise; nor was I prompted by such a motive in requesting an examination. I need not add, that I have received none.

I trust, sir, that yourself and readers will take this communication as I intend it,—merely as the record of a professional fact.—I have the honour to be, sir,

Your obedient servant,
W. J. Erasmus Wilson.

Sydenham College,
May 2, 1838.

ERRATA IN MR. TAYLOR'S PAPER.

To the Editor of the Medical Gazette.

Sir,

In the paper which you did me the favour of inserting in last week's Medical Gazette, there are one or two errors of the press, which, as they render the text unintelligible, I should be obliged by your noticing in the errata.

In the table of alternating calculi, the last calculus mentioned stands simply as "oxalate of lime;" whereas it should be, "oxalate of lime, uric acid, oxalate of lime, 1." At page 192, col. 1, line 23, *for* "muriate in lime," *read* "of lime;" and in the same page and column, 17 lines from the bottom, "phosphate of ammonia and magnesia" should have been in Italics, and have headed the next sentence.

Regretting that I have given so much trouble, which has probably arisen from my own inaccuracies, I remain, sir,

Your obliged servant,
Thos. Taylor.

New Bridge Street,
May 2, 1838.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, April 26, 1838.

Thomas Griffiths Williams, Cwmllich, Breconshire. — Alfred Crabb. — John Douglas Strang, Bridport, Dorset. — Benjamin Smith Chimmo, Sheerness, Kent. — Robert Welham, Clackton, Essex.—Edward Hare, Stanhoe, Norfolk.

WEEKLY ACCOUNT of BURIALS.

From Bills of Mortality, *May* 1, 1838.

Age and Debility	22	Heart, diseased	1
Apoplexy	4	Hernia	2
Asthma	7	Hooping Cough	9
Consumption	30	Inflammation	6
Constipation of the Bowels	2	Brain	1
Convulsions	21	Lungs and Pleura	4
Croup	2	Insanity	1
Dentition or Teething	4	Liver, diseased	2
Dropsy	4	Measles	1
Dropsy in the Brain	8	Mortification	2
Dropsy in the Chest	1	Small-pox	9
Epilepsy	1	Thrush	1
Fever	13	Tumor	1
Fever, Scarlet	3	Unknown Causes	85
Fever, Typhus	2	Casualties	7
Hæmorrhage	1		

Increase of Burials, as compared with the preceding week . . . 56

METEOROLOGICAL JOURNAL.

April.	Thermometer.		Barometer	
Thursday . 26	from 37	to 50	29·78	to 29·90
Friday. . . 27	31	50	29·87	29·84
Saturday . 28	27	53	29·78	29 68
Sunday . . 29	26	48	29·64	29·67
Monday . . 30	29	54	29·55	29·50
May.				
Tuesday . 1	40	54	29 58	29·59
Wednesday 2	44	67	29.74	29·73

Winds N.W. and S.W.

Except the 28th and afternoon of the 30th ult., and 2nd instant, generally cloudy; rain on the 30th and following day; distant thunder in the S. snd S.W. during the evening of the 2d, accompanied with vivid lightning, which was almost incessant from every quarter except the north.

A remarkably bright parhelion formed to the eastward of the sun, about six o'clock on the evening of the 2d.

Rain fallen, ·1125 of an inch.

NOTICES.

Dr. Alcock's "final letter" shall appear in the *extra limites* of next week.

Papers are preparing for publication from the following correspondents:—Dr. Fergusson, Dr. Wilson, Dr. Griffin, Dr. Burne, Mr. J. Jeffreys, A Retired Physician, Mr. Calder, Tritus, Observer, K., parts 1 and 2, Dr. Cowan, Mr. Gosse, Mr. Reid, and Mr. North.

Wilson & Son, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 12, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton-Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XIX.

Diseases of the Parenchyma of the Lungs:— What is the Parenchyma?—Pneumonia, its Seat and Causes — General Symptoms— Anatomical History: Engorgement; Hepatization; Suppuration; Gangrene; State of adjoining Textures—Pathological History: Functional Symptoms; Physical Signs: Cause of crepitant Rhonchus; Signs of Hepatization, Suppuration, and Gangrene— Varieties: Typhoid Pneumonia; Pneumonia with Bronchitis; Pleuro-pneumonia.

Having, as far as our time would permit, considered the diseases of the membranes and tissues investing the lungs, both outside and inside, we have now to think of what lies between these membranes, the pulmonary parenchyma. But what is this parenchyma, if you take from it the membrane that lines the air-tubes and cells, and the pleura which covers their exterior? See how delicate is the structure of this piece of healthy lung! how thin are the films that separate these minute membranous bubbles — this tissue of froth! How can we distinguish the parenchyma from the investing membranes? We can scarcely do so by anatomy. But physiology and pathology will supply us with a distinction that enables us to make good the division, long acknowledged in practice, of diseases of the lungs, into those of the investing membranes and those of the parenchyma. Let us recollect the great purpose of the function of the lungs, and we shall bring to mind the air and the blood that exert across the membranes a mutual influence on each other. We have spoken of the vessels which contain the air, but not of those which contain the blood: these it is that lie between the aerial and pleural tissues of the lung, and with the connecting cellular tissue, exceedingly delicate between the cells, but thicker around the tubes and between the lobules, these constitute the proper parenchyma of the lung.

In considering the diseases of the parenchyma, we, as usual, first take up inflammation, that formidable disease *pneumonia*, or *peripneumony*; and there is the greater reason why inflammation, an affection which chiefly concerns the blood-vessels, should be a prominent object of study in the parenchyma of the lungs, because this parenchyma is so mainly constituted by blood-vessels. It is well worthy of remark, that the inflammations which we have already considered, bronchitis and pleuritis, affect vessels which freely communicate with those of the parenchyma; but although they do occasionally extend to the latter vessels, and become thus converted into peripneumonic inflammations, yet they are generally quite distinct from this in their present course and ulterior effects, and require a distinction in treatment, which should ever be borne in mind. We find an explanation of this difference in the peculiar character and importance of the pulmonary blood-vessels; in their great number and capacity; in the large proportion which they must bear to the other solids of the lung; and in their great

liability to congestive distension. This extensive and important plexus of vessels, through which the whole blood of the body passes, is, as we have before seen, peculiarly liable to distension from any cause which may disorder the function of the heart or lungs. Thus all those causes which tend to induce asphyxia, produce also that congestion of the pulmonary vessels which, added to irritative reaction, may constitute inflammation; and thus we see that various causes which affect the balance of the circulation, particularly by deranging the passage of the blood through the lungs, such as asphyxiating poisons, congestive fevers, violent exertion, diseases of the heart, bronchitis, asthma, tubercles in the lungs, long-continued exposure to cold, and the moribund state, may become causes of pneumonic inflammation.

It is scarcely necessary to detain you with a notice of the general symptoms of pneumonia. You will find them amply described by authors; and I shall be the less particular in stating my own views on the whole subject of this disease, because what I have to say more than is to be read in books, may, for the most part, be found in the article "Pneumonia," in the *Cyclopædia of Practical Medicine*. With respect to the general symptoms, I dare say that your own experience has suggested to you how insufficient they sometimes are to characterize the disease. The pain in the chest generally more dull and deep-seated than in pleurisy, the cough with more expectoration, the pulse quick, but often soft; the quickened, and sometimes very oppressed breathing, made worse by lying on the affected side, often truly indicate the complaint; but the pathologist knows that these symptoms are more accidental than essential, and that they may all exist without pneumonia. Examinations after death have often startled medical men by discovering proofs of a pneumonia, the existence of which had scarcely been suspected during life; and did time permit, I could cite many instructive anecdotes to this effect. But now let us resort to the anatomy of the disease, to instruct us as to the physical changes which pneumonia produces; and we shall then be enabled to understand the physical signs that they may cause produce during life.

The first condition produced in the lungs by inflammation, is that of *sanguineous congestion* or *engorgement*. The vessels are then so much distended that the whole tissue appears red, of different shades. In some cases a frothy serum exudes when the lung is cut: this is probably the effect of the coagulation of the blood after death, for it is not observed in those cases in which the blood remains in a fluid state. It is a common notion that the blood of an inflamed lung is effused into the air-cells; but several reasons incline me to believe it to be contained still in distended vessels and in the tissue, although it may afterwards be extravasated. Andral examined an inflamed lung after drying and slicing it; and the only difference which he could perceive in the structure, was, that the coats of the cells were somewhat thicker and redder than natural; but there was not that obliteration of the cells that might have been anticipated if they had been filled with blood. In typhoid pneumonia, and that from asphyxiating gases of the sedative kind (such as those of sewers), the inflammation does not seem to go beyond this stage; or if it do, it passes at once into a half gangrenous, half purulent destruction of the tissue, there being, apparently, under these circumstances, some change in the vital properties of the tissues, or in the condition of the blood, which incapacitates it from supplying lymph, the deposition of which constitutes the next stage, that of hepatization.

The second stage of inflammation, *hepatization*, when complete, brings the lung into a state of solidity like that of liver. But you are not to suppose that the transition from the first to this stage is sudden; it is gradual, and is the result of that same overflow of the nutritive function which we found to cause the effusion of lymph on the inflamed pleura. The tissue of the lung, besides being rendered more solid, is also generally more fragile than usual, so that on being pressed it breaks down under the finger. This softening is, I think, chiefly the consequence of the interstitial deposit of soft fresh lymph, which diminishes the molecular cohesion of the tissues; and the more acute and recent the inflammation is, the greater in general will be the softening. The colour of a hepatized lung will vary much, according to the quantity of the blood left in it: if this be much, it will be red; if little, pinkish brown or reddish grey, if mixed with the black pulmonary matter. The deposition of lymph seems to supersede the red particles, or it may possibly be formed at their expense. A hepatized lung, on being cut or torn, often presents a granulated aspect; numerous little points, of the size of pin-heads, being of a lighter colour than the rest. These granules Andral first represented to be the single air-vesicles, or terminations of the bronchi; and he conceived that they were distended with the same viscid mucus, secreted by their mucous linings, which is seen in the sputa. In this opinion he was joined by Laennec. Soon after this view

was published, I made minute examinations of the granulations of hepatized lungs, and was soon convinced that they contained no appreciable quantity of the viscid mucus of the expectoration; and I was led to regard them as vesicles with their coats distended by an interstitial deposit of lymph, and perhaps containing the same matter in their interior. In his more recent work on Pathological Anatomy, Andral has expressed a similar opinion. But hepatized lungs do not always present this granulated appearance: sometimes there is a uniform condensation of a deeper red than usual; and this condition Andral refers to a more complete obliteration of the cells, a further degree of solid effusion. But this would not account for the redder colour; and I would rather view this kind of non-granular hepatization as the result of inflammation confined more to the plexus of vessels and interstitial tissue, and affecting less the membranes forming the cells: hence the consolidation partakes more of the character of the vessels and of the blood which they convey, and less of the lighter coloured deposit which the membranes of the cells secrete.

The third stage to which inflammation brings the lung is that of *suppuration*, or *yellow hepatization*. This consists in the conversion of the semi-solid particles of lymph or blood, which constitute the solid of red hepatization into an opaque, light yellowish, friable matter, and finally into a fluid pus. This suppuration is generally diffused in the form of purulent infiltration; and it is very rare to find it assume the character of a distinct abscess. You see a sufficient reason for this in the very porous structure of the lung, which renders the circumscription of the matter, by the effusion of lymph, such as takes place in abscesses in general, an unlikely result; and the life of the patient, as well as the vitality of such a delicate and porous structure as that of the lung, is generally destroyed before the process of suppuration can be completed. Hence, even where the suppuration has advanced most, there is generally much of the tissue of the lung remaining; and a gangrenous condition is often added to the suppuration, giving the matter a very offensive odour. Nevertheless, circumscribed abscesses are now and then met with in the lung; and this is generally when the inflammation is limited, or more intense at one part, so as to lead to the early formation of pus, whilst the adjoining parts are still capable of throwing out a circumscribing lymph. In this way I have met with abscesses terminating pneumonia affecting separate lobules; thus, too, abscesses are formed around foreign bodies within the lung, such as a musket-ball; and around calcareous and scrofulous tubercles, which may act as foreign bodies. In all these cases the inflammation of the most irritated parts reaches the stage of suppuration long before that of those around them, and the latter thus forms a separating wall of effused lymph, which may afterwards constitute a kind of cyst. The purulent deposits which are sometimes met with in the lungs of patients who have had extensive suppurations in distant parts, generally present this circumscribed character.

Gangrene, unconnected with suppuration, is a very rare sequel of inflammation. It seems, however, to arise pretty generally from the influence of those noxious gases which directly destroy the vitality of the tissue of the lung. The lungs of persons who have died some days after being nearly asphyxiated in sewers, have been found reduced in parts to a dark brown, greenish, or livid softening; having a very fœtid odour, and being probably the result of the poisonous influence of the gas on a congested lung.

The state of the tissues adjoining the vascular plexus, the proper seat of pneumonia, is worthy of notice. The interlobular cellular texture sometimes partakes of the general redness, and sometimes it is singularly free from it, or has it so much less, that a section of the lung is quite marbled by its lines: so also in the hepatized stage, the interlobular septa retain their cohesion, and, in more chronic cases, sometimes become thicker and harder than usual. The mucous membrane of the large and middle-sized bronchi is almost always more or less inflamed, and presents the red striated aspect that is also seen in the more intense forms of bronchitis. That of the smaller bronchi is often of a deeper red than in bronchitis; but, from its bluish tint, this would appear to arise rather from the blood under it than in it. The bronchi in the inflamed part generally, but not always, partake of the softening in the parenchyma; and, in the stage of hepatization, I have sometimes seen the finer ones plugged with lymph—conditions which I have before noticed to you as likely to lead to the permanent obliteration of some tubes, and the dilatation of others. More commonly, however, the tubes contain more or less of the slimy rusty mucus which is seen in the expectoration. The pleura is generally, but by no means constantly, inflamed; at least I have seen it free from redness, lymph, and liquid effusion, even when covering a hepatized lung. The cases of pleuro-pneumonia in which the inflammation of the pleura is so considerable as to modify the course of the pulmonary in-

flammation, will require our attention afterwards.

Now let us see what effects these various lesions will have on the vital and physical properties of the lungs, and in developing symptoms and signs of their presence. The disorder of the vital functions need scarcely occupy our attention, for they are amply described in works; and although often most important in directing our treatment, it frequently fails in distinguishing pneumonia from other affections of the organs of respiration. The febrile disturbance is commonly of a very pronounced kind, as it might be expected to be from the extent of the vascular plexus involved in diseased action. But it is very uncertain whether this fever shall be of a sthenic or an adynamic type, for we meet with pneumonia of both characters; and we find it also variously complicated with febrile irritation, particularly involving the hepatic and gastric functions. We meet with it also associated with the various eruptive fevers, and arising out of the congestions left by remittent and continued fevers of the typhoid type. You must see that our time will not permit us to enter into the details of these varieties, which are more proper subjects for a course of clinical medicine; and I shall only say a few words on the general symptoms most common to pneumonia in general. The pulse is generally much quickened, and in the early stage it presents often the strength and hardness common in severe inflammation. But the disease in its severe form seldom extends far, or continues long, without a change in the pulse, which, retaining and increasing its quickness, becomes small and thready, or liquid yet jarring, as if a little blood were thrown with much force into a large and loose vessel. Such, in fact, is the case, for the obstructed state of the lungs prevents the left side of the heart from receiving its due supply of blood, whilst the right side is over distended, and irritated into increased frequency and abruptness of contraction, which are communicated to the left. Whether these pulses shall reach the radial artery as small thready strokes, or as looser liquid ones, will depend on the condition of the surface and the tonic contractility of the artery; circumstances that I formerly adverted to in speaking of the analysis of the general symptoms of diseases of the chest. Hence you can understand that the pulse may be very deceptive with regard to the amount of the inflammatory disease; hence, too, it is not at all unusual to find it increase in fulness and force after blood-letting. The difficulty of breathing is frequently by no means proportioned to the extent of the disease; although, when present, it is less equivocal than the pulse, and may be a better guide to distinguish the variations of the complaint. Cough is a very uncertain symptom, being often very slight in the worst cases, and, as may be supposed, depends rather on the attendant bronchitis than on the parenchymatous inflammation itself. But the cough, however slight, is generally accompanied by the expectoration of a matter, which is in great degree characteristic of pneumonia, and this circumstance makes it a symptom of much importance. The sputum of pneumonia is that of the most intense form of bronchitis, viscid, glutinous, capable of being drawn into threads, and having at first a certain degree of transparency, but afterwards often becoming opaque; but what distinguishes it from that of bronchitis is its reddish or rusty colour, which sometimes passes into an orange, a yellow, and even a greenish tint. These colours depend on the intimate admixture of different small proportions of the colouring matter of the blood, and we cannot doubt that it proceeds from the mass of blood accumulated in the pulmonary plexus, and tinging the bronchial mucus. This rusty expectoration was first described by Andral, and it is considered by him and others to be quite pathognomic of peripneumony. To this, however, I cannot assent; the rusty tinge may be communicated to the bronchial mucus by congestions that are not inflammatory, and I have seen an expectoration of the same character in cases of bronchitis supervening on organic diseases of the heart causing great pulmonary congestion, and also on pulmonary apoplexy: either of these complications may be attended by the expectoration of viscid semitransparent mucus, variously tinged with the colouring matter of the blood. Still, when occurring with other symptoms of inflammation of the lung, these sputa may be considered as pretty surely conclusive of its existence. The intensity of the inflammation is sometimes represented by the degree of viscidity of these sputa; but this is not always the case; nor can you expect it to be so, when you reflect that the sputa are the product, not of the pneumonia, but of the bronchitis attending on the pneumonia, which are by no means in constant proportion to each other. So also it sometimes happens that the expectoration is altogether wanting; and in the case of young children it is seldom brought within our view.

If we study the physical signs of pneumonia, we shall find in them much surer means of distinguishing the disease, and they will furnish us with a good pathological history of the conditions of the affected organ. On listening to the chest of a person with incipient inflammation of

the lungs, you will hear, generally in the inferior and posterior region of one side of the chest, a fine crackling sound accompanying the respiratory murmur. In its slightest degrees it is scarcely more than an unusual loudness and roughness in the vesicular murmur, as if the air met with slight short resistances in its passage, which destroy the smoothness of the sound; but in its more pronounced degree there is a distinct crepitation, like that heard when kitchen salt is thrown on a hot iron, or like that caused by rubbing between the finger and thumb a lock of hair near one's ear. It is first heard at the commencement of inspiration and the end of expiration; but it soon accompanies the whole respiratory act, and in advanced degrees of the first stage it is heard only at the end of inspiration and the beginning of expiration.

Now what is the cause of this fine crepitation, or *crepitant rhonchus*, as it is also termed? M. Andral considered it to be produced by the passage of air in minute bubbles through serum effused in the minute air-tubes and vesicles, and that the fine and even character of the crepitation depended on the smallness of the bubbles in the extreme tubes. Thus he supposed that this crepitation differed from the mucous rhonchus only in the size of the tubes in which it is produced, and the consequent size of the bubbles in them. This opinion has been adopted by most writers; and you will find the various liquid rhonchi of catarrh, and the crepitation of pneumonia, all reduced to the two heads—large and small crepitation. From a consideration of the pathology of pneumonia, and the course followed by its physical signs, I was long ago led to consider the crepitation which attends its early stage distinct in nature from the other rhonchi. We have before had occasion to notice that the structure and motions of the lungs tend to bring all liquids secreted in the minute tubes into those of larger size, whence they are ultimately collected in the trachea, and expelled by expectoration. Now were the crepitation of pneumonia dependent on serum in the smallest tubes and cells, we ought to have proofs of the presence of this serum in the other tubes by a bubbling rhonchus, if not in the expectoration as well. Now I have had frequent occasion to observe that the first stage of pneumonia is in many cases remarkably free from bubbling sounds in the large tubes; and you know that the expectoration is of a viscid mucous, instead of a serous character. In fact, there is good reason to believe that the serum which exudes from an engorged lung after death, and which Andral assumed to be the cause of the crepitation, is chiefly the result of a cadaveric change, the coagulation of the blood in the distended vessels, which does not take place during life. There are other reasons, which we have not time to state, which induce me to take another view of the cause of the crepitant rhonchus.

What is the condition of the extreme air-tubes and cells in the first stage of peripneumony? Are they not narrowed and partially obstructed by the enlarged vessels which are distributed between and around them? And as the smallest tubes are narrower than the cells in which they terminate, may not the obstruction become such in them, that the air can force itself through the viscid mucus which lines them only in successive minute bubbles, the crackling of which constitutes the crepitation in question? This appears to me to be the true mechanism of the crepitant rhonchus; and in this view you can understand how, at first, the crepitation must be but slight, and confined to the most collapsed state of the tissue; that as the narrowing increases, it extends to the whole respiratory movements; that subsequently the obstruction is such that it permits the crepitating passage of the air only when the lungs are most expanded, as at the end of inspiration and the beginning of expiration; and finally, that the obstruction becomes complete, and the crepitation ceases, except perhaps still, on a forced inspiration.

You can see, too, that the natural respiratory murmur will be rendered rough, and perhaps sharper, before the crepitation begins*; that it will be diminished as the crepitation comes on and extends to more of the minute tubes; and that it will cease when the abnormal sound occupies them all. If the disease be extensive, and the function of the lung much infringed on, the energy and frequency of the respiratory movements will be increased, and consequently the respiratory murmur on the sound side will be louder than usual, having the character of *puerile* respiration.

The increased matter of the congested lung will have further effects on its properties with regard to sound. It will deaden the sound on percussion, so that the affected side will give a sound rather duller than the opposite side; and different degrees of force in percussion will not ma-

* Dr Stokes considers that there is a stage of pneumonia before the crepitation begins, and that it is denoted by a puerile, or unusually loud respiratory murmur in the part. This is an important observation; but I doubt the propriety of calling that another *stage*, which probably is only a smaller degree of the first stage, and which afterwards causes crepitation — the partial narrowing of the tubes.

terially affect this variation. But the first stage of inflammation, without liquid effusion, is insufficient to make the sound on percussion quite dull; for even in its most advanced degree there is still enough of air in the lung to give an elastic resistance to the walls of the chest, and to leave their vibrations pretty free. The motion of the affected side will be diminished in proportion as the air fails to get admittance into the inflamed lung; and instead of being fixed in a state of distension as in pleurisy, or in a state of contraction as in spasmodic asthma, the side holds an intermediate size, measuring less than the sound side on full inspiration, and more than it on the completion of expiration. The increased density of the congested lung also makes it conduct sound better than in the light spongy condition of health; so that in extensive inflammation, even during the first stage, and whilst the crepitation yet continues, you may hear in some measure the bronchial respiration and vocal resonance, that are fully developed only in the stage of hepatization. Let us pass on to the signs of this stage.

The deposition of lymph which constitutes hepatization of the lung completes the obstruction of the minute tubes and cells: hence all crepitation ceases, and the only sounds that reach the ear are those of the air and voice in the larger tubes. The respiration is no longer heard with its prolonged murmur; but in the neighbourhood of the bronchial tubes only there is a *short whiffing*, confined to parts only of the respiratory act, and often ending abruptly with a *click*. This bronchial whiffing is not to be heard in every case, but only when the hepatization involves considerable bronchial tubes, and is most commonly found in the middle portions of the chest. Here, too, may be both heard and felt the various degrees of morbid bronchophony, or vocal resonance. When the consolidation of the lung is very complete, and involves especially the central parts of the lung, the voice may be heard to resound over a space of considerable extent in the mammary, scapular, or axillary region, and so loudly, that it appears to issue from the part like the voice in pectoriloquy, with which it is sometimes confounded. You may generally distinguish the bronchophony of a consolidated lung, by observing that it extends over a considerable space, and that it is greatly diminished by using the stethoscope with the stopper in, which is not the case with the pectoriloquy of a cavity. The vocal resonance of the tubes is also transmitted by the condensed lung to the parietes as a vibration, or fremitus, which may be distinctly felt by the hand placed on the affected side, and which is much stronger than that on the healthy side. This affords an easy mode of distinguishing between a hepatized lung and a pleuritic effusion; for the latter generally abolishes completely the vocal vibration. The dulness on percussion is now pretty complete, but it is seldom so uniform and general in the lower and middle portions of the chest as the dulness from liquid effusion. You will see as a reason for this, on examining a hepatized lung, that there are generally some lobules or parts retaining enough air to prevent them from sinking in water; and this is also sufficient to prevent the sound on percussion from being perfectly dull. And when the consolidation is perfect, it will transmit the stroke of percussion to deeper seated parts, which will yield their resonance. Thus on the left side a hepatized lung will sometimes give you the tympanitic resonance of the stomach; and near the sternum, or in the mammary regions, you may occasionally have the amphoric resonance, or *bottle-note* of the large air-tubes. It is not difficult to distinguish these sounds from that of healthy percussion, and when once you understand their cause, their presence and properties will serve rather to instruct than to confuse you. In the hepatized stage, the lung being inexpansible, you have the corresponding walls of the chest nearly motionless; and they are so in a state neither of distension nor of contraction, without fulness of the intercostal spaces, or displacement of the viscera, and thus you have further distinctions between this case and that of pleuritic effusion. When the left lung is solidified, it transmits the sounds and impulse of the heart to an unusually wide extent of surface, instead of diminishing and displacing them, as pleuritic effusion does. When the hepatization is complete, and involves the root of the lungs, I have known it to cause a bellows murmur at each pulse, over a considerable extent of the affected side; and I think that a similar phenomenon has been described by Dr. Graves, of Dublin. This is probably owing to a partial pressure produced by the solid deposition, on the pulmonary artery, or some of its chief branches, and then transmitted with the current throughout the consolidated lung.

In the third or suppurative stage there is no change in the condition of the lung which can modify the physical signs until the effused matter begins to liquefy; and then there is a mucous or bubbling rhonchus, from the secretion into the air-tubes. There may be a simultaneous change in the nature of the expectorated matter, if there be strength enough left in the body to keep up this excretion. Andral

describes it to be sometimes a thin slightly glutinous liquid like treacle and water, or the juice of stewed prunes: this is a sero-mucous fluid, coloured with the hæmatin in an altered state. I have seen it more after pulmonary hæmorrhage than in pneumonia. I have seen pus expectorated in the third stage of pneumonia without abscess; but I believe more commonly there is no expectoration at all, or such only as comes from the upper tubes, and therefore gives no evidence of the state of the lung. In fact, we are rather to infer the supervention of the third stage from the duration of the disease, and the general symptoms, than from any physical signs. The inflammatory symptoms and fever give way to great prostration, rigors, cold sweats, a weak thready pulse, whilst the breathing is as short as ever, and the countenance exhibits the pallid, waxy, anxious, drawn, tremulous features of ebbing vitality.

The formation of abscess is less unfavourable, because it implies less extent of suppuration, and a power in the structure to circumscribe it. There are several cases of recorded recovery from pneumonia after the signs of abscess had manifested themselves. These signs are, those of a cavity, first containing liquid and air, and the seat of a coarse bubbling or gurgling sound on coughing; and after the expectoration of pus, cavernous or hollow respiration, with pectoriloquy, or loud resonance of the voice, in some part of the chest corresponding with the affected spot. These abscesses are not uncommonly of a gangrenous character; and then there is added to the signs a putrid fœtor in the matter expectorated, as well as in the breath of the patient.

Now without entering into further details, (for which I must refer you to my treatise in the Cyclopædia), you may perceive that the physical signs of pneumonia will indicate the situation and extent of the inflammation; and, as far as relates to the diseased organ, they may guide us in the prognosis and treatment. Thus a crepitant rhonchus heard in a whole lung, or in a considerable part of both lungs, implies extensive disease. If heard at the root of the lung, or at its apex, that is, at the scapulæ, or under the clavicles, it indicates a severer form of disease than if heard only at the lower parts of the lung in the back. The extension of this crepitation, or its presence, in new parts, is a proof of the increase of the inflammation. Its cessation, and the substitution of bronchial respiration, are proofs of its advancement to the second stage. On the other hand, the return of the crepitation where it had been replaced by bronchial respiration, and perfect dulness on percussion, announces a progress towards cure, by the absorption of the obstructing lymph, and by the air again gaining a straitened admission into the air-cells. As this process proceeds, the act of respiration, accompanied by crepitation, becomes longer in duration, until it equals the duration of the respiratory movement on the healthy side; but the sound is still for a time somewhat whiffing where it has been bronchial; and a crepitation of a looser, less even character, also remains after apparent cure; this is the *subcrepitant* rhonchus, and probably depends on the presence, in the smallest bronchi, of a little thin serous mucus, such as that which is seen in the expectoration, and the secretion of which seems, as in bronchitis, to assist in removing the depositions left by the inflammation. If the inflammation have proceeded to the stage of hepatization, and particularly if it have verged on that of suppuration, in which the albuminous deposit, becoming opaque and lower in vitality, is less susceptible of absorption, the restoration of the texture of the lung to its natural light condition requires a considerable period of time, even after the apparent cure of the disease; and during this period there will remain more or less of the physical signs just mentioned, as well as some dulness on percussion, and perhaps, also, not a full power to expand that portion of the chest. We suppose here the cure to become perfect in time, but there are other cases, in which inflammation of long duration produces permanent changes in the lung, by the entire obliteration of some portions of the tissue, and the dilatation of others: these changes happen most frequently when the inflammation is modified by a contemporaneous effusion in the pleura. This we shall notice hereafter.

We have not time to examine all the varieties and complications which inflammation of the lungs presents in its signs and course; but there are two or three which I cannot pass over.

The form of pneumonia that is called typhoid, whether it be secondary to continued fever, or whether it be primary and assume the typhoid type, from a constitution lowered by excesses, or by the depressing influence of foul air or of an unhealthy season, differs remarkably in many of its phenomena from the common pneumonia. The local symptoms are by no means prominent, and although there may be pain, cough, and very imperfect breathing, the obtuse state of the mental faculties prevents attention from being drawn to them. But the general functions are greatly disordered; the pulse is quick, small, and weak; the skin is harsh, dry,

and partially hot, or covered with a clammy sweat, and sometimes spotted with petechiæ; the tongue is furred, brown, and dry; the alvine excretion dark coloured and otherwise disordered; and the urine is scanty, turbid, and ammoniacal. The lungs in such cases are sometimes found after death so engorged, particularly their posterior portions, that they sink in water; the texture is very soft and fragile, and when broken exudes a dark grumous blood: there is only an imperfect approach to hepatization, but the texture in some parts occasionally shews a softening of a lighter colour, which seems to be an imperfect suppuration. In these cases we find dulness on percussion, and absence of the vesicular respiration in the posterior parts of the chest, but no crepitation; or if it be present, it is of very short duration. In the anterior parts of the chest the breathing may be distinct enough, and accompanied by sibilant and sonorous rhonchi. Now I cannot help viewing these cases as of a congestive rather than of an inflammatory character. By some unknown cause, whether in the condition of the blood, or in the affected capillaries, or in both, the blood stagnates and accumulates in particular viscera; generally to some degree under the influence of gravitation; and the functions of the organs are proportionately impeded or disturbed. There is at the same time more or less irritation, which may give this congestion somewhat of an inflammatory character; but its products are imperfect and irregular; and neither in the effusion of lymph, or in the formation of pus, is there manifest a true inflammatory orgasm. We can see why, in the lung, this condition should be unaccompanied by the usual signs of the gradual formation of a crepitating obstruction, because the engorgement is at once produced, and renders a great part of the tissue impervious to air. But this degree of congestion, if it occupy the middle parts of the lung, may give bronchophony and bronchial respiration.

The complication of pneumonia with bronchitis is very common; in fact, as I have before said, there is bronchial inflammation in nearly every case of pneumonia. But it sometimes happens that pneumonia becomes added to extensive bronchitis; and this, especially when attended with copious secretion, obscures the physical signs of the pulmonary inflammation. On applying your ear to the chest you hear sonorous, sibilant, and mucous rhonchi, so loud and general, that you can scarcely distinguish whether crepitation be present or not. In such a case listen particularly to the end of each inspiration, at the inferior and posterior regions of the chest, about the margins of the lobes, and if crepitation is to be heard at all, it will generally be then and there. Look out also for the rusty tinge in the sputa, and as the disease proceeds, for greater dulness on percussion; and you will generally succeed in discovering by these means when the inflammation has extended to the pulmonary plexus of vessels.

The effects of a concomitant pleurisy on the pathology and signs of pneumonia, are more remarkable. Whenever the inflammation extends from the lung to the pleura, it may be supposed to increase the serous secretion, and perhaps lead to the effusion of lymph; but when the pulmonary inflammation has existed first, and become extensive, these pleuritic products are commonly to small amount. When the inflammation has simultaneously attacked both the parenchyma and the investing membrane, and nearly to an equal degree, constituting the disease called pleuro-pneumonia, the effusion of the pleura by its pressure modifies the effect of the inflammation in the lung. The lung is found after death consolidated, but tougher and redder than in the state of ordinary hepatization, and totally destitute of the granular aspect. It very much resembles the substance of muscle; hence Laennec termed this condition *carnification*. It seems to exhibit the more essential part of inflammation of the lung; the sequel or effect, effusion into the coats of the air-cells, which constitutes granulation, having been prevented by the pressure of the external liquid effusion. This combination is therefore, too, slower in progress than simple pneumonia; the degree of the inflammation, as well as the quantity of its product, is restrained by the external pressure, and it scarcely, if ever, proceeds beyond the second stage. But this slower rate of process tends to make its results more permanent. If false membranes are thrown out on the pleura, they become more firmly organized, and bind down the lung in its more compressed state; and the lymph effused in the tissue itself, scanty though it be in comparison with that of a hepatized lung, may become the means of adhesion of the sides of the compressed cells and finer tubes, and of consequent obliteration of more or less of the proper tissue of the lung. When the liquid effusion is removed by absorption, and when the chest becomes again capable of expansion, what will supply the place of the obliterated cells? There will be the atmospheric pressure at each inspiration brought to act on the obstructed tubes; the air can no longer penetrate to the cells; so it must dilate the tubes,

and make their increased size compensate for their defective terminations. Pleuro-pneumonia is, then, as I have before had occasion to remark, a cause of dilatation of the bronchi; and, in stating this, I have better ground than theory, from having watched two remarkable cases soon after their inflammatory origin in acute pleuro-pneumonia, through a chronic course, in which signs of permanent consolidation of the lung, dilatation of the bronchi, and contraction of the chest, were the physical conditions which accompanied dyspnœa, weakness, and a dropsical and cachectic state, which ultimately terminated in death, when I found on dissection the conditions which I have described.

On referring to the records of other cases of dilatation of the bronchi, I have met with several in which the symptoms are described to have originated in an inflammatory attack, like pleuro-pneumonia; and I incline much to the opinion that all the cases in which the dilatation affects the bronchi of one side only, and in which there is also general consolidation of the lung, with some contraction of the chest, owe their existence to this cause.

The signs of pleuro-pneumonia are a combination of those of pneumonia and of liquid effusion in the pleura. At first there is crepitation; but this, as the lung is pushed aside by the liquid, becomes indistinct, whilst the dulness on percussion is much more marked than in pneumonia, at least in the lower parts of the affected side. In the central region of the chest, bronchial respiration and bronchophony are soon produced by the condensed lung being pushed against the walls; and if a thin layer of liquid intervene, the bronchophony acquires a loud buzzing accompaniment, like the voice of *Punch*; in fact, the voice seems double, which probably depends on a part of its vibrations being modified into a buzzing or bleating, by passing through the thin layer of liquid, whilst other vibrations pass unchanged. The vocal resonance is generally louder in pleuro-pneumonia than in either pleurisy or simple pneumonia; and I suppose this arises from the chief tubes being pressed so closely against the walls of the chest, with complete condensation of the vesicular structure. The same circumstance will sometimes give the amphoric or tracheal sound on percussion in the mammary region, which forms a singular contrast to the dulness of other parts: so also I have found the respiration quite tracheal in this spot, and the resonance of the voice quite as loud as that of caverns.

RECOLLECTIONS OF CHOLERA:

ITS NATURE AND TREATMENT.

By W. Griffin, M.D.

No. XII.

Saline Treatment.

The sulphates and muriates of soda, it has been already shewn, were not only employed for the cure of cholera in India and Eastern Europe, but acquired some degree of reputation in these countries before the disease reached the British Isles. The physicians in the Isle of France, Drs. Ysenbeck and Brailow of St. Petersburgh, Mr. Searle, and others, were warm advocates of the practice; and, it is stated, had very considerable success with it. The two German physicians at St. Petersburgh are reported to have lost only three out of 30 cases, which, if they all came under treatment before the pulse was lost at the wrist, was about equal in success to the calomel treatment; but if half were in collapse on admission, far exceeded it. The number was perhaps too small, even if the condition of the patients was more particularly stated, to draw very decided inferences from. A nearly similar treatment, except that alkaline salts were chiefly used in preference to the neutral, has been strenuously recommended in London by Dr. Stevens, and through his confident advocacy of the remedy has been more or less tried in almost every part of the three kingdoms. Before entering more particularly into the merits of a practice which gave rise to much angry discussion at the time, I may just observe, that the very striking testimonies in its favour, from several practitioners, are few indeed when compared with the numerous trials it received, the results of which, if successful, would certainly have been made known. It was employed in every hospital in Limerick after the appearance of Dr. Stevens's papers on the subject, but whether from the extreme severity of the cases in which it was used, or its absolute inefficacy, it proved, in a great majority of them, a failure. Dr. Mackintosh, of Edinburgh, employed it in the Drummond-Street Hospital, and did not find it serviceable in a single case; while it proved injurious in many, by increasing the vomiting and purging. Dr. Tweedie, and other prac-

titioners in London, found it to do harm by increasing the insensible purging, and it was tried by the Liverpool and Dublin physicians without success. It appears to me impossible that a remedy capable of producing such magical effects in the hands of Dr. Stevens, should fail so universally when tested by medical gentlemen exceedingly anxious for some means of staying the dreadful mortality which was taking place in their several districts, and equally uninterested in either the fame or the disappointment it was likely to bring to a physician with whom they had neither communication nor acquaintance. I shall, however, inquire into the merits of the practice on Dr. Stevens's own shewing, and endeavour to arrive at some conclusion as to its probable curative influence, which, in the absence of any accurate statement of the cases when they fell under treatment, must prove, as in other instances, exceedingly difficult.

The cases offered in evidence of the success of the saline treatment were those occurring in the Cold Bath Fields Prison, in which, at the time it commenced, there was a population of about 1300 souls. It is reported that from the 8th of April, when the saline method was commenced, to the cessation of the first epidemic, in the close of April, 100 individuals were, in a greater or lesser degree, under the influence of the disease. Of these—

50 had bowel complaint, with some degree of irritation of stomach—fluids ejected deficient in bile.

31 with the above symptoms—cramps to a greater or lesser degree.

19 are described as malignant, having been seized with the disease or fallen into collapse in the cold wards of the prison during the night.

Out of the first 50 cases none died; out of the 31 cases two died; out of the 19 malignant, one died—in all, three deaths, or 97 recoveries, in 100 cases.

The first idea that must occur to every experienced observer on looking over this statement is, the obvious effort, the anxiety to accumulate cases with the view of establishing a favourable result. With what other object can we suppose the first 50 cases of simple premonitory diarrhœa are classed with cholera cases? Every practitioner knows that when cholera is epidemic in any locality, nearly half the population, and sometimes three-fourths, are usually affected with diarrhœa, often to a very severe degree; but neither in hospitals nor private practice are they, or ought they to be, classed with cholera cases. If we had not excluded such cases, to the utmost of our ability, from the Limerick Hospitals, we might have exaggerated our amount of recoveries ten-fold. With what view, again, is it stated that this diarrhœa resisted the ordinary remedies; such as calomel, opium, chalk, astringents, &c. when every practitioner of experience is aware that the premonitory diarrhœa of cholera almost invariably yields to one or more of these medicines, and that such cases run into the confirmed disease only through neglect or mismanagement? In endeavouring to form a comparative estimate of the value of the saline treatment, it is obvious that these 50 cases cannot be fairly taken into account at all.

The 31 cases which come next had, it appears, cramps to a greater or lesser degree, in addition to the symptoms of which the 50 complained. Now, although cramps are suspicious symptoms when connected with diarrhœa, they are by no means diagnostic of epidemic cholera, and the only symptoms which are admitted on all hands to be so, the total absence of bile in the discharges and of the secretion of urine, were not present. It is merely reported that there was a deficiency of bile in the ejected fluids. We may, therefore, reasonably doubt whether any of even these 31 were cases of genuine cholera; yet, out of the number, it appears two died. Under the calomel treatment in a similar number, in whom the secretions of bile and urine were suppressed, a proportion of from one and a half to four would have been lost; but, in 31 cases in which these secretions were only deficient, it can hardly be said that one would have been lost, unless at times or in places where the epidemic happened to be of an unusually rapid type.

The third section of 19 cases, which are denominated malignant by Dr. Stevens, were all, it will be readily admitted, instances of true epidemic cholera, and of these only one died, or nearly six in the hundred, which is a little less than the average lost under the calomel treatment, when the subjects were brought into the hospital before

the cessation of the pulse at the wrist. As we cannot ascertain from the word *malignant* how many of these cases were in absolute collapse before any treatment was commenced, this comparison must however be considered very uncertain. A nearer approach to correctness may be made by taking all the supposed cholera cases together, which are comprised in the second and third sections, and amount to 50, of whom three died, or six in the hundred. These cannot be imagined to have had among them, in an institution in which they were daily watched, a greater or indeed so great a proportion of cases running into collapse (before treatment was commenced) as are brought into public hospitals, and may, therefore, fairly be compared with the cases reported in Limerick. Out of 119 cases admitted into St. Michael's Hospital, in the month of July, in whom the pulse at the wrist was perceptible, only five died, or four in the 100; out of 128 in the Nunnery Hospital only seven, or about six in the 100; and, out of 419 at St. John's Hospital, only 29, or about seven in the 100. So that in the small number of cases, at Cold Bath Fields Prison, as reported by Dr. Stevens himself, the success of the treatment did not exceed the average success of the treatment by calomel, tested in several hundred cases in various hospitals, and under different physicians. In a second incursion of the epidemic, which Dr. Stevens describes as more violent than the first, there were 105 cases and 15 deaths: if they were all absolute cholera cases, with perfect suppression of bile and urine, there was, even here, as great a mortality as appears in the most unfavourable reports of any of the Limerick hospitals, excluding the pulselesss or cadaverized cases.

The strongest reports put forward by any member of the profession, in support of the saline treatment, are those published in the MEDICAL GAZETTE*, by Mr. Bossey, surgeon to the conviet hospital ship at Woolwich, where were confined, at the time, about 800 convicts. These reports having an appearanee of aecurate classification, are more likely to create a favourable impression of the treatment than others of a more general nature; but, if examined, they will be found to afford quite as little material for arriving at a correct conclusion as any of Dr. Stevens's. Mr. Bossey had, at first, 65 cases, of which nine proved fatal; afterwards 61, of which 11 died; in all, 126 cases, 20 deaths—rather more than the proportion lost on the average of hundreds of cases in the most unfavourable reports of any of the Limerick hospitals, excluding the cases admitted in pulseless collapse, but less by half than the mortality in those hospitals, if all the admissions are taken together; for in that case 40 would have been probably the proportion of deaths in 126 cases, instead of 20. It is, of course, essential to any correct inference from Mr. Bossey's cases, that we should be made acquainted with the number of decided cholera cases among the 126, as well as the number in collapse before any treatment commenced. His report of them runs thus:—

Premonitory cases 23	
In moderate collapse	24
In severe collapse	55
With insensible purging .	27
Fatal cases (of course cases of collapse)	20
In all.....	126 cases.

Here again are 23 premonitory cases, or, in other words, cases of simple diarrhœa, classed with regular cholera cases, and credit taken for them as recoveries in the total amount, just as was done by Dr. Stevens. Throwing these altogether out of consideration, the real cholera cases amount to 126; and how many of these does the reader imagine are reported in collapse? 126!—the whole number! They are divided into moderate and severe collapse—but all were in collapse! In no hospital in the known world, unwilling as people are to go into hospital early, has it happened that every case was admitted in absolutc collapse. More than half are seldom in that state, and sometimes not more than a third. Yet, in a convict ship, where the inmates were medically inspected three times daily, and any man who had three evaeuations while at labour was immediately placed under observation, and the dejections examined; all the true cholera cases are reported in severe or moderate collapse, and of these 20 die*! The more one

* Vol. x. p. 729; Sept. 8, 1832.

* If the word collapse is not meant to express cases pulseless, or nearly so, it is only another illustration of the idleness of all reports in which equivocal terms are made use of.

studies this report, the more beset with difficulties does it seem. If there was such a system of inspection kept up, how did all the cases run into collapse in defiance of the saline treatment? If the saline treatment utterly failed in that stage of the malady, when so many other remedies are effective, how came it to effect such a miraculous number of cures in that stage when all other remedies are known to be useless? and, again, if it was so magical in its influence over the period of collapse, how has it happened to fail so egregiously in the hands of so many other medical men, who certainly felt considerable interest in the attainment of some successful method of treatment?

The only additional report in favour of the saline treatment which claims mention, is an anonymous one from Warrington, which states that out of 108 cases, 78 were treated in various ways, and all, without exception, died! while of the remaining 30, which were placed under the saline treatment, only two were lost. Knowing nothing of the state of these patients when they fell under treatment, there can be no reasoning about the value of the remedies employed, but it must necessarily appear to any casual observer that the universal fatality in the first instance is, if possible, more extraordinary than the almost universal recovery of the latter.

If one were merely to consider some of the foregoing observations, the want of more general testimony among the profession at large, in favour of a remedy very generally tried—the extravagant pretensions to success—the eking out of statements by the addition of diarrhœal cases—and above all, the disingenuous and unqualified abuse of other remedies by the advocates of the saline treatment—he would perhaps be disposed to refuse it any degree of confidence whatever; and Sir David Barry's statements would certainly tend much to strengthen any incredulity arising on the subject. It is, however, the fate of many valuable remedies to lose character even for the virtues which they truly possess, by the extravagant praises of those who advocate their use, and it is very probable the saline treatment has suffered in this way to the advocacy of Dr. Stevens. We have sufficient evidence that saline medicines, in one shape or another, have been employed for a considerable period, and in different countries, for the cure of cholera, and though never attaining the reputation which calomel seems to have done, at least among Indian practitioners, still holding a place among remedies said to be very successful. Like emetic tartar, and the acetate of lead, it requires to have its capabilities more accurately tested, both previous to and after collapse, before we could consent to substitute it for any medicine whose curative influence is more fully ascertained. In the stage of collapse I believe it to be wholly uninfluential, and that it has been apparently more successful than other remedies at such period of the disease only because it is more inert. It, I believe, approaches nearest, in harmlessness as in success, to the cold water treatment of Dr. Shute, and this has been one of the great secrets of its reputation. After medical men had failed in numerous cases to effect a single recovery by the most strenuous efforts of medical skill, they naturally were struck with amazement on witnessing, under the saline or cold water plan, a patient's slowly reviving after long collapse and struggling on steadily to convalescence; but this amazement subsided as their experience extended, finding those recoveries were, after all, very limited in amount; and were more attributable to the undisturbed efforts of nature, than to the influence of remedies.

Injections.

Injections have been so very generally, I might almost say universally, used in cholera, that they cannot be passed over without more particular mention. They have been employed in all stages of the complaint, and although they have never attained the reputation of a specific in the disease, scarcely any practitioner has disputed their pretensions as auxiliaries. The variety of medicines which have been applied in this way to the lining membrane of the lower intestines, has nearly equalled that exhibited by the stomach. Opium, camphor, quinine, æther, turpentine, oil, rhatany, the metallic astringents, &c. They may be all said to resolve themselves into emollients, anodynes, stimulants, perhaps contrastimulants, and astringents, under which heads they may be conveniently noticed.

As emollients or fomentations, warm

water, bran water, starch, bread-crumb water, beef tea, &c. have been used very generally in Paris and other cities on the continent, as well as in these countries. But their effects, even when they appeared to soothe and quiet the intestinal irritation, were, I believe, very temporary, nor do I think much importance can in any way attach to them. As instances, however, of what perseverance may do, even with a very inefficient remedy, three cases may be quoted (two in bad collapse) which did well under the management of Mr. Devonald, by the administration of spiced beef-tea by injection, unassisted by any other remedy, except saline draughts and cold water, which were administered by the mouth, until the stomach became quiet, after which the drink also was beef-tea. His plan was to give an injection after every motion, on which system no less than 160 were given to one patient, and 100 to another! Whether the cold water or beef-tea treatment had most influence in these recoveries, it is somewhat difficult to determine.

Stimulant injections, for the purpose of reviving the sinking powers of life, when a patient is running into or already in collapse, were naturally suggested by the advantage derived from them in states of similar failure and insensibility of the nervous and circulating powers, after poisoning by opium. In this way turpentine, æther, camphor, wine and brandy, have been tried in greater or lesser doses; but as in the case of mere emollients, they often fail in producing any effect at all, and even where they seem to succeed the improvement is very temporary. If this disadvantage is attempted to be obviated, either by increasing the strength of the injection or repeating it more frequently, there is, on the other hand, considerable danger, in the stage of collapse, that the feeble degree of vital power which remains, if continually spurred up and excited, may only wear out the more rapidly. The cases to which these stimulant injections appear most applicable are those in which the patient seems to be running fast into collapse, and we wish to prevent this result even by a temporary excitement, to gain time for those more influential remedies which we are at the same moment administering by the stomach. When a patient has once fallen into perfect collapse, it becomes a question whether, even if we could, by the force of stimulant injections, arouse him out of it, there would be any advantage in doing so? Dr. Shute asserts that the more gradual a patient's revival from collapse is, the more probable is his prospect of recovery; and I think it not unreasonable to suppose that, as in mechanics, what we gain in time by such treatment is lost in power.

Astringent injections seem to me to be of far more importance than any other in the cure of cholera. I do not believe that they exercise any directly counteracting influence on the disease, but by checking the serous discharges they certainly tend to delay the approach of collapse. As we possess very effective remedies for controlling the primary stage of cholera, to gain time in that stage is, in fact, to attain a cure. The discharges in the complaint do not, it is true, constitute all, nor perhaps the chief danger, as cases may run on to collapse and death, even after the purging has ceased; but instances of the kind, previous to collapse, are very rare, and when they do occur are indicative of a virulence in the type of the malady, which would be likely to resist the best devised treatment. It may indeed be considered a general law in cholera, that the progress to collapse always bears some relation to the quantity or number of the evacuations as well as to the constitutional strength of the patient, or the virulence of type in the epidemic, and that without a continuing diarrhœa few or no patients become pulseless. In their effects on the system, as well as in their serous nature, the discharges in cholera certainly bear a remarkable likeness to pure hæmorrhage, they are followed by the same pallor and sinking of the countenance—the same pulselessness—vomiting, restlessness, and anxiety, and very often by the same suddenness of death. We have therefore some analogy for connecting such results with the diarrhœa, and for suppressing the latter in every possible way by topical astringents, as I look upon these injections to be, while we are endeavouring to counteract the exciting cause by remedies directed to the general system. I have not myself been in the habit of directing injections in the early stage of cholera, because I have found that calomel, in large doses, or even opium, very generally suppresses the bowel complaint at the same

time that it alleviates the other symptoms; but whenever it seemed disposed to resist the treatment, or any sinking or depression was observed in the countenance, I always found it necessary to administer the subacetate of lead, or laudanum mixed with starch, by the rectum. The former was preferred alone when the patient was not suffering with distressing cramps; but, whenever there was much pain, or the injections were too readily returned, or the patient appeared to be on the verge of collapse, and it was of importance to prevent even one other evacuation, a teaspoonful of laudanum was added to the mixture. If the narcotic effect of laudanum was observable after the remedy, the event was likely to be less favourable, nor have I seen advantage from its exhibition in such cases even where the stupor was accompanied by suppression of the vomiting and diarrhœa. Laudanum should be used by injection, purely as an astringent, or to allay violent spasmodic pain. Latterly, in making use of the subacetate of lead injection, I was in the habit of repeating it after every evacuation, however frequent or numerous; and, I believe, with very great advantage to the general treatment. In those cases in which the irritability of the bowels was so great that neither by the smallness of the injection, nor by the addition of opium to it, could I get it retained for more than a few moments, I had recourse to pressure at the anus, by means of a folded towel, with success. Dr. Clanny, of Sunderland, has been very much ridiculed for proposing a wooden plug for this purpose—certainly with some justice, if he meant it as a cure for cholera, but any absurd pretensions connected with it should by no means make us insensible to the value of the principle in cases to which it is applicable.

The injection commonly employed in the Limerick hospitals, and with very obvious benefit, was composed of half a drachm of the subacetate of lead dissolved in starch. In other parts of the country the same astringent was administered in half-scruple doses only, combined with equal proportions of the sulphates of copper and alum. Some physicians administered astringent gums and decoctions, while many depended altogether on opium.

Quinine, tobacco, and other powerful medicines, have been also tried in enemata, with no clearly defined view that I can understand. As, however, they do not appear to have acquired any reputation, it is unnecessary to notice them.

[To be continued.]

CONTRIBUTIONS

TO THE

DIAGNOSIS OF THORACIC DISEASE.

By Charles Cowan, M.D.

Physician to the Reading Dispensary.

(*For the Medical Gazette.*)

No. VI.

Intra-thoracic Carcinomatous Tumor, diagnosed during life—Post-mortem appearances. Case of Empyema—Remarkable Adhesions — External Thoracic Abscess — Difficulties of Diagnosis.

The chapter on Cancer of the Lung, in Dr. Stokes' admirable treatise on "Diseases of the Chest," is by far the most complete we possess on the subject, though, as the author very candidly admits, it still leaves much to be desired. It contains an analysis of two cases observed by Dr. Stokes himself, of three recorded by Bayle under the term of cancerous phthisis, of two by Andral, one by Dr. Carswell, and one by Dr. Houston, in none of which was the affection recognised or even suspected during life. Laennec merely gives a pathological account of carcinoma, detailing no examples illustrative of the diagnosis, contenting himself with stating, "that the stethoscope *ought* to point out the existence of the medullary tumor of the lung, when this is of considerable extent." Dr. Stokes states, "that as yet almost nothing has been done in establishing the diagnosis of this disease;" and a careful perusal of the cases he has related will point out the difficulty which the subject presents, and the occasional impossibility of arriving at any satisfactory conclusion during life.

To the same author we are also indebted for a most valuable description of the physical indications of intra-thoracic tumors, of whatever class they may be; and to the labours of this distinguished observer we must attribute

our having formed a correct diagnosis in the instance we are going to narrate.

So long as the lung itself is the only seat of cancerous disease, and no increase of bulk has resulted, the physical signs are merely those of solidity more or less extensive, and the real nature of the case can only be suspected after repeated examinations, and careful comparison of the local signs with the history and progress of the general symptoms, assisted occasionally by the presence of external disease. The latter, however, is frequently absent; and every observer who is conversant with the varying nature of the former, will feel that in the great majority of cases, under the circumstances we are alluding to, the difficulties of forming a correct diagnosis must be very great. Should, however, the encephaloid or the scirrhous matter be first external to, and afterwards displacing the lung, or should the size of the tumor originally commencing in the lung materially exceed the natural boundaries of that organ, our judgment would then be aided by various combinations of the symptoms of compression, displacement, or obliteration, as indicated by the state of the trachea, lungs, œsophagus, nerves, arteries, and veins; and if these different effects were in all cases carefully searched for and attended to, we believe that the diagnosis of the tumors we are now considering, aneurismal as well as others, to be in the majority of cases practicable.

The following observation presents an interesting example of the form of tumor we have last described, and we believe it is the first instance recorded of the successful application of physical diagnosis. We were consulted, in June 1837, by Mr. H., æt. 45, of rather spare habit, red hair, and freckled complexion; of average height, and a well developed thorax. His general aspect indicated great anxiety and distress, and the exertion of leaving the carriage and entering the house produced such urgent dyspnœa, that he was unable to speak, and could only recover himself by leaning forward for a considerable time, and grasping the back of a chair. His head and face presented great vascular fulness and venous engorgement, and his respiration was painfully prolonged and stridulous. On attentively listening, it was evident that the latter effect originated below the larynx, coming as if from beneath the sternum. His voice was husky, like a hoarse whisper; but at moments, if he particularly exerted himself, became almost natural.

He stated that eight months previously, while enjoying his usual health, he was attacked with cough and mucous expectoration, accompanied from the first with a wheezing laryngeal sound. His voice at that period was not affected, nor was he sensible of any tightness of breathing. Three months subsequently he suffered from general febrile symptoms, and pain in the left side. For this he was twice largely bled and leeched. During the continuance of active treatment the tracheal wheezing very much diminished, but returned with his convalescence. He rapidly recovered flesh and strength, and soon after could walk from the city to the west end of London without fatigue. About one month since he was attacked, rather suddenly, with violent dyspnœa, presenting all the characters of an asthmatic paroxysm. This has frequently recurred, with variable intensity and duration, and is succeeded by an interval of comparative ease. During the attack he always leans forward, and for some time past has not been able to lie on the right side without increasing the cough and dyspnœa, and experiencing a sensation of something falling over from the left to the right side of the chest. He had never suffered from dysphagia. The expectoration consists of opaque yellow mucus. At several periods he has had hæmoptysis, but it was never copious; the cough is not very frequent; no hectic symptoms have been observed; and no œdema of the upper or lower extremites. Smoking increases the expectoration, and relieves the dyspnœa, and his paroxysms have always been much mitigated by the usual antispasmodic medicines. The digestion and other functions do not materially deviate from a state of health.

Physical signs.—Thorax is deep and well formed. Breathing principally abdominal; and though the respiratory effort is very great, and accompanied with a prolonged wheezing sibilus, yet the sub-clavicular regions (especially the left) expand very little, nor do the cervical muscles act forcibly. Both jugular veins are permanently distended, and very tense to the finger, but without pulsation. There is general congestion

of the veins of the head and face, as well as over the sternum and the anterior part of the left side, where the upper four cartilages are rather more prominent than natural, and the corresponding subcutaneous cellular tissue is slightly œdematous. There is no other visual deviation from what is normal.

Percussion is dull and resisting over the *whole of the sternum and anterior surface of the left side*, except immediately under the clavicle, where some slight resonance is still perceptible. In the axilla of the same side percussion is *clearer* than natural, as is also the case over the *inferior and posterior* third. The sound is much less resonant in the supra and infra-spinous regions, but not absolutely dull. On the *right* side percussion is very clear, and the vertical diameter of the lung is increased.

Auscultation detects *no respiratory murmur, no bronchophony, no rhonchus or vocal resonance over the whole of the left side*, but immediately under the sternum small subcrepitus and sibilant rhonchi are heard during the expiration. The heart's sounds and impulse are very distinctly audible, and felt throughout the same side; but neither the rhythm nor sounds are anormal. The organ is not laterally displaced, but its pulsations in the epigastrium are more distinct than natural. On the right side the inspiration is every where feeble or inaudible, and the expiration is accompanied by prolonged sonorous and sibilant rhonchi. The intercostal spaces are not filled up or dilated, and no enlargement could be detected in either hypochondria. We do not find the state of the pulse noticed, and we neglected a comparative examination of the radial arteries. Our apology must be our only having seen the patient once, and his distressing state not justifying a prolonged examination. There was no lateral displacement of the trachea, nor any unusual fulness round the base of the neck.

The treatment recommended consisted of alternate leeching and cupping every three days for a fortnight, and afterwards a seton in the left side. Opiates and antispasmodics were prescribed at night. He was advised to have the mouth kept tender by mercury for some weeks, and to adopt perfect rest and a mild diet.

The diagnosis, as written down at the time, was—"Tumor, probably carcinomatous, in the mediastinum, compressing the trachea, left bronchus, and neighbouring blood-vessels. Chronic bronchitis and vesicular emphysema." The prognosis was necessarily fatal.

From my friend, Mr. Cox, of Aldermaston, I have lately heard that Mr. H. died rather suddenly, about a week after I saw him, and he very kindly procured for me the following account of the post-mortem appearances, as detailed by Mr. H.'s medical attendant in Oxford. They confirm the diagnosis in every essential particular, and they possess the merit of not being described to suit the writer's purposes.

Post-mortem.—"About ℥iij. of serum were found in the cavities of the pleuræ, and the pericardium contained about ℥ij. of a similar fluid.

"The left lung was compressed, and of a blackish colour, from very numerous and small carbonaceous, or melanotic depositions. The right lung was in some parts congested with dark blood, and in other parts parts emphysematous. The mucous membrane of the trachea and bronchi was highly vascular.

"The morbid appearances which served to explain the cause of the dyspnœa and the suffering of the patient, was a *large tumor weighing about two pounds*, extending from the lower two-thirds of the trachea to three inches on the bronchi, pressing on the pericardium and lungs, and surrounding the larger vessels connected with the heart. A section of the tumor exhibited the upper portions of a vascular sarcomatous structure, with small cavities containing bloody pus; but on the lower portion it had all the characters of scirrhus. The bronchial glands were involved in this morbid mass, and some other small tumors were also connected with it, as well as attached to the arch of the aorta, and to the roots of the pulmonary arteries.

"The abdominal viscera were not unhealthy, nor the body emaciated."

Observations.—The fearful extent of the local disease, contrasted with the state of the general health, and the intermitting character of the symptoms, are facts of no inconsiderable interest, and most strikingly illustrate the superior value of physical signs over constitutional symptoms, which could never, in the instance before us, have afforded

any very satisfactory clue to the real nature of the affection. It would in all probability have been regarded as an aggravated case of asthma, with extensive cardiac or vascular disease. The rapid progress of the symptoms the last few weeks, may no doubt be attributed to the increasing volume of the tumor more and more interfering with the innervation and circulation; and from the encephaloid nature of the upper portion we may conclude that its recent growth had been proportionately rapid. That it was originally exterior to the lung, is proved not only by the post-mortem examination, but also from the fact of tracheal compression being one of the earliest symptoms, and continuing throughout the entire progress of the case. With the exception of the slight elevation of the upper cartilages of the left side, and the œdematous condition of the integuments, there was no external indication of the state of the parts within; and the absence of any external tumors deprived us of any assistance we might have derived from such a fact. In all probability the tumor originated in the cellular tissue of the posterior mediastinum; that at first it was of a simple scirrhous character, and comparatively slow in growth; while it afterwards assumed the medullary structure, and advanced with corresponding rapidity.

The diagnosis was evidently founded upon a series of negative considerations, and the nature of the enlargement was inferred from the fact of the great majority of intra-thoracic tumors presenting the carcinomatous form.

The absence of pulsations distinct from those of the heart, and the non-existence of bellows murmur, rendered the supposition of aneurism unnecessary.

The history and progress of the symptoms—the state of the general health, combined with the substernal dulness—and the absence of all the respiratory phenomena usually attendant upon a hepatized or tuberculated lung, did not for a moment favour the idea of our having to do with a case of pneumonia or phthisis; and the only alternative remaining was to regard it as an example of empyema or tumor.

The complete dulness on percussion under the sternum, extending over the anterior portion of the left side, but absent in the axilla and posterior and inferior regions; the natural situation of the heart; the normal condition of the intercostal spaces; the absence of all respiratory sounds; the undilated state of the left side; and the natural condition of the hypochondrium, coupled with the progress of the case—the asthmatic paroxysms—vascular obstruction, and tracheal compression—all tended to invalidate the idea of empyema, and to confirm the supposition of the symptoms depending on the presence of a large mediastinal tumor, the exact nature of which, as we have already remarked, could only be inferred from the general results which pathology, under similar circumstances, has afforded.

Repeated examinations might have led, perhaps, to still more accurate conclusions; but we cannot help feeling the progress which medicine has made, when we find ourselves enabled, by the exercise of a single sense, and after a single examination, to arrive at a diagnosis which no sagacity or experience, unaided by this important means of investigation, could ever have enabled us more than doubtingly to form.

The following case, though not of a similar nature, yet forms a valuable term of comparison with the preceding, presenting, as it did, many circumstances in common, while it essentially differed in others. It is a good instance of the caution we should exercise in the interpretation of physical signs, and of the singular combinations which they occasionally exhibit. We shall limit our description to what more immediately refers to the diagnosis.

Richard Adams, a servant, æt. 29, applied for relief about two years ago, under the following circumstances:—For the last twelve months he had been suffering from dyspnœa on exertion, slight cough, and mucous expectoration, with an occasional pain in the left side. In other respects he did not consider himself ill, nor did he interrupt his usual occupation, though he was sensible of gradually increasing debility and emaciation. Three weeks prior to his present application he was suddenly attacked with acute pain in the left side, and increased difficulty of breathing; this was succeeded in two or three days by a sense of burning at the sternal extremity of the left clavicle, quickly followed by a swelling, which gradually extended laterally and downwards, parallel with the sternum, as low as the

mamma. Since the appearance of the swelling he can only lie on the affected side, the opposite position producing a sense of dragging and oppression. His general health had materially suffered, and he looks thin, hectic, and distressed. The cough had not been much aggravated, but he had suffered frequently from acute shooting pains across the upper portions of the left thorax. He has never had hæmoptysis, diarrhœa, or night perspirations. His other functions are all tolerably healthy.

Physical signs.—On the left side, and extending from the sternal end of the clavicle to the mamma, was a smooth oval swelling, about five inches long by four wide, most prominent towards the middle, and confounded in its circumference with the surrounding integuments. It presented to the touch a deep-seated fluctuation, while the upper fourth felt rather hard and nodulated, and was marbled by a blue network of veins. No effect was produced on the contents by coughing, or during inspiration or expiration. The side is not dilated, and the movements of the ribs very slightly diminished. The intercostal spaces, not covered by the swelling, were distinctly drawn inwards by each inspiration.

Percussion gave a dull sound, with moderate resistance over the upper third of the sternum; under the clavicle it was rather less dull, though far from clear, while no resonance at all could be detected over the remaining portion of the side anteriorly. In the superior half of the axilla the sound was *clear*, which was also the case over the middle and posterior region, as far as the spine. Below this it became quite dull, and very little resonance could be detected in the supra and infra-spinous regions.

Auscultation could detect no respiratory murmur, no vocal or other resonance, except in the axilla and middle and posterior third. Ægophony was tolerably distinct at the lower angle of the scapula. On the right side the respiration was slightly puerile.

There was no evidence of venous engorgement or tracheal compression, nor was the heart displaced.

Observations.—The loss of sound on percussion, the ægophony, and the absence of respiration over the posterior and inferior region of the left side, clearly pointed out the presence of liquid effusion, but it was not so easy satisfactorily to interpret the condition of the upper portion of the chest. Our first impression was that there was an encephaloid tumor, either external to or embedded in the lung, which had perforated the intercostal space; and this supposition was founded upon the chronicity of the case; upon the nodulated and discoloured appearance of the swelling; upon the fact of clear percussion in the axillary and scapular regions; upon the natural condition of the intercostal spaces, and the mobility of the ribs. Subsequent examination, however, proved the fallacy of this opinion, and left no doubt upon the mind that the case was one of empyema, complicated with extensive adhesions of the middle portions of the lung.

The contents of the abscess were evacuated without any evidence of communication being established with the pleural cavity, though there is no doubt that originally such was the case. All appearance of tumor quickly subsided, and was unaccompanied by any sensible change in the condition of the interior of the chest: the nodulated and vascular state of the integuments very soon disappeared. This last effect had evidently depended upon simple cellular induration, with venous obstruction.

In the course of a few weeks the respiratory murmur became gradually more and more distinct in the upper portions of the chest, and the percussion was proportionately clearer. Under the sternum the sound became normally resonant. The evidence of effusion in the lower portion of the pleura still continued, though the extent of dulness had somewhat diminished: and the patient had so far recovered health and strength that change of air was recommended, and we had no opportunity of becoming acquainted with the ulterior progress of the case.

The fact of fluid accumulation occupying the upper and lower regions of the chest, while the central portions of the lung remained extensively adherent, and the combination of these conditions with an external abscess of the parietes, were circumstances well calculated to produce a puzzling association of physical signs; but the subsequent progress of the case removed every uncertainty in the diagnosis, and forcibly illustrated the great interest and satisfaction which an attentive physical examination of the chest is often capable of affording. It is probable

that the lower effusion had existed for many months, and that the rest of the lung was firmly adherent. In consequence of inflammation accidentally attacking the upper surface of the pleura, the adhesions were partially destroyed, pus was secreted, and perforation of the upper intercostal space (which afterwards closed) was the secondary consequence. The limited amount of the effusion accounts for the chest not being dilated, and the extensive adhesions of lung satisfactorily explain the mobility of the ribs. The loss of sound under the sternum was caused by mediastinal displacement, while inferiorly the quantity of fluid was too inconsiderable to produce any lateral deviation of the heart. We have, therefore, a case of empyema in which the heart was not displaced, the side not dilated, the intercostal spaces not filled up, the movements of the ribs only slightly impaired, and the respiratory sounds extensively present. Yet, notwithstanding these anomalies, the diagnosis was still possible, and after watching the progress of the case, no second opinion could be entertained.

It will be observed that there was a remarkable similarity in the results of percussion and auscultation, yet the peculiar and distinctive signs of extra-pulmonary-thoracic tumors were distinctly present in the first instance, and absent in the second. Their juxtaposition is, we think, well calculated to fix the attention upon the means by which our opinion should be determined, and to impress the student of physical diagnosis with the necessity of carefully and minutely examining every case that is presented to him; for without this, much that is interesting will escape, and much that is obscure will remain unexplained. It is almost unnecessary to remark, that in the case of aneurismal tumors we have the additional indications of anormal pulsation, in conjunction with the bellows sound, though it should also be remembered that two cases of carcinomatous tumors are recorded, one by Dr. Graves and the other by Dr. Stokes; in the first of which a diffused pulsation was distinctly perceptible, and in the second a deep-seated bellows murmur. These peculiarities were dependent upon the peculiar connexions of the tumors with the larger vessels, and in both instances the diagnosis was erroneous. Under similar circumstances, the pulsation would probably be less localized and more feeble, and the bellows murmur deeper and more obscure, than what we could easily reconcile with the state and locality of the percussion, supposing the loss of sound to be dependent upon aneurismal enlargement. We are, however, much in want of additional observations on the subject.

We now conclude these rather lengthened remarks, hoping that they will not be found wholly destitute of interest to those who are persuaded of the great importance of correct physical diagnosis, and of the difficulties which not unfrequently are still attendant upon its practical application.

Reading, April 30, 1838.

ON THE WEST INDIAN FEVER.

To the Editor of the Medical Gazette.

Sir,

In the 18th No. of the last volume of the Medical Gazette, there appeared a paper on the "Malignant Fever of British Guiana," the united composition of Sir A. Halliday and Dr. William Fraser, being begun by the first, and finished by the last of those gentlemen. On first glancing at the contents of that paper, they appeared so vague, inconclusive, and unimpressive, that I did not think them worthy of consideration; but it has been suggested to me that, as they represent the opinions of men of high official station, they may mislead the uninformed and unwary; and that it would not be unbecoming in me, as a person who had taken some part in the controverted question respecting the contagious power of West Indian fever, to notice them, however briefly, through the medium of the journal in which they were published. Briefly, therefore, I propose to notice the contents of the paper in question, so far as they are tangible, and profess to have relation to facts: the declamatory part, forming a large portion of the whole, is beyond the reach of reason or of refutation.

On the general question of the origin and mode of propagating West Indian, or, as it is generally called, yellow fever, I will not enter here; neither your space nor my time would admit of such a discussion; nor, after what I have written on the subject elsewhere, is it necessary. But after all the argu-

ments which I*, and other writers, have accumulated, to shew that the disease cannot be communicated by one person to another, I am surprised to find the almost, I had thought altogether, exploded doctrines of Blane and Chisolm revived, or attempted to be revived, at such a time, and by such means.

Sir A. Halliday says, "The facts brought forward by Dr. Fraser are, perhaps, the strongest that were ever adduced in confirmation of a certain variety of the yellow fever being highly contagious; at least they leave no doubt in my mind that the late epidemic in Demerara was not only contagious, and propagated by contagion, or infection (I know not which is the best term), but that it certainly was introduced into the colony in consequence of the quarantine laws being suspended." . . . . "Now I believe, sir, there is no doctrine more firmly established than this, that a man may convey contagion from one person to another, without in any way suffering from the disease himself, and therefore maintain that we ought never, in any instance, to allow the crews of ships arriving from infected places, to have free intercourse with the inhabitants of the port at which they arrive, until at least some precaution has been taken to render the danger of their causing infection less probable."

The facts referred to in the first part of the extract we will attend to presently. The last sentence is comprehensive, both doctrinally and practically; but the soundness of the doctrine, and the propriety of the practice arising out of it, neither I, nor the majority of your readers, it is to be presumed, will admit. On the subject of fomites, as applied to plague and some other forms of febrile disease, I will not enter here; but I ask, in reference to the disease in question, when or how has the doctrine been firmly established—when or how has it been rendered probable—when or how has it had a single feature of feasibility attached to it? It is difficult to deal with the practical part of the sentence, from the very general terms in which it is couched, and the obscurity of the phrases, "infected places," "free intercourse," and "until at least some precaution has been taken to render the danger of their causing infection less probable;" but if it be meant to be affirmed that no intercourse shall be permitted between the crews of ships coming from what are called "infected places," and the inhabitants of the part at which they arrive, it is plain that intercourse between the islands mutually, and between them and the contiguous continent, must soon entirely cease. Scarcely a season passes during which, in some portion of the Caribbean chain, or adjacent shores, more or less of a fever will not be found, which, according to the teaching of Sir A. Halliday and Dr. Fraser, is contagious, and can be propagated by contagion only; from one such case the cause of the disease may be dispatched by a ship, through the medium of persons whom it does not affect, but who have the power of carrying it to a healthy place, and depositing it among the citizens, whom it shall affect without limit. Then there appears no method of ascertaining how long sailors from "the infected place" may carry the cause of the disease about with them—tied up in a handkerchief, or rolled up in a tobacco pouch—without detriment to themselves, but ready to scatter death and devastation among the shore residents. If such were the law and the practice, interinsular commerce must soon cease; the merchant might shut up his store, and the planter rest from his labours, the business of the port being confined to the duties of quarantine officers.

But Sir A. Halliday's doctrines and deductions have detained us too long, perhaps, from Dr. Fraser's "facts and illustrations." The facts are few and far between, and so destitute of point and precision, that men looking beyond the very surface of things, and searching for truth in the spirit of rigid and uncompromising inquiry, will scarcely be disposed to place them in the scales of evidence. Such as they are, however, I will now advert to them, taking them in the shape and order in which they are arrayed by their author.

The first *fact* in the historical account of the fever is this, that "it found its way" from Kerinane to Barbados in the summer of 1836, where, in a few weeks, it destroyed many hundreds of the inhabitants. Where the said Kerinane is, and through what channel the disease "found its way" from that place to Barbados, we are not told. Jumping to a conclusion is an easy affair in some minds; it is not so easy to lead other minds up the ladder of induction, step by step, till the chain of evidence be complete, and the conclu-

* Memoirs of West Indian Fever.

sion irresistible. Dr. Chisolm did not ask his readers to admit the importation of his nova pestis, from Bulam, quite so readily; but let that pass.

The next *fact*, relating to the geographical progress of the fever, is its transport from Barbados to Demerara. Did Dr. Fraser ever meditate on the paradox about "false facts?" Not sufficiently, it is suspected, or he would be more scrupulous in the application of the term. "From Barbados it found its way to this place" (Demerara.) So writes Dr. Fraser. That is the fact, and these are the words in which it is announced. Where are the circumstances carefully scrutinized, nicely balanced, and minutely detailed, which are to give to that naked assertion the form and pressure of a fact. Not in the paper before us; not one jot of evidence is adduced there in support of the so-called fact. There is Dr. Fraser's declaration, *and that is all.* He has a laudable horror of sceptics and unbelievers of all kinds, whether in ethical or physical science. If he find people willing to adopt his creed, as to the importation of fever from Barbados to Demerara in 1837, on his shewing, they must be people with a large capacity for believing, indeed.

I pass Dr. Fraser's theological episode, because it has no very evident connexion with the subject; nor will I waste time in denying the fitness of the comparisons he has thought proper to institute, to the disadvantage of us the non-contagionists, asking him simply, in passing, whether it was courtesy to liken us to Deists—nay, to the maddest of the mad, to Atheists? We are accustomed to have hard words applied to us by our antagonists of the pro-contagion school, and are therefore apt to become rather callous. Sir Gilbert Blane, the Magnus Apollo of the party, in a charitable temper, and with a nice discernment of corresponding qualities, compared us to certain Asiatics, who, after they have become frenzied from the use of opium, rush from their horrid dens, and *run a muck* against the peaceable citizens, killing every one on whom they can lay their hands: hard words, making us like Atheists for unbelief; like assassins in our murderous propensities.

The introduction of the fever being settled so summarily, Dr. Fraser proceeds to the proof of its contagious properties after it was landed at George Town, Demerara; and he assures us that he will make the proof so complete, as to convince the most "bigoted non-contagionist" "that *infection alone* formed the source and entire medium of its existence and propagation." And what are the materials of that proof? The following:—

1. Two missionaries from Berbice visited George Town during the prevalence of fever, one of whom felt unwell while at George Town, and both died from fever after returning thence to Berbice. The wife of one, being the sister of the other, and in attendance on them, also died from fever, after which fever prevailed in the town and district, and caused much mortality.

2. M. Schomburg returned to George Town in June, from the interior, bringing with him an Indian attendant. He slept in a bed where, two or three weeks before, a person had died from yellow fever; and, in a short time, he and his servant were attacked by fever. Certain Indians who had been in the town "caught the infection, returned to their sylvan villages, and not only fell victims to the disease which they had thus casually caught, but communicated the infection to others of their tribe, who had not on that occasion left their homes, and of which some also died."

3. A lady visited a neighbour whose husband had fever; after which she, her maid, the clerks, and other persons in the house, had fever.

These are the proofs, *all the proofs*, of the contagious character of the Guiana fever, 1837. They satisfied Sir A. Halliday; they do not satisfy me, and I venture to affirm that they will not convince my brother bigots. By the by, would the epithet not be quite as well applied to the pro-contagionists as to the anti-contagionists? But for the exact truth of how many of those statements, such as they are, will Dr. Fraser answer? In the case of the Berbice missionaries, supposing they imbibed the cause of their disease in Demerara, does he know the precise relation which that disease bore to the fever which afterwards prevailed in Berbice, in respect of time, position, and character? Does he know that the fever of that district was not the product of its own soil?—and has he arrived at that knowledge through a process of inquiry so close and careful as science shall deem sufficient? All common-place, hearsay,

patchwork evidence, I put aside; knowing, as every one who has undertaken such an inquiry must know, how utterly worthless, when sifted and properly tested, it almost invariably proves. I ask the same questions respecting the fever alleged to have been carried by the Arrowaek Indians to their sylvan villages, and propagated there by personal contagion. If Dr. Fraser will furnish adequate evidence that the disease was carried to a place where it had not been before, and afterwards extended to one person beyond the person who carried it there, that plaee being incapable of produeing the cause of the disease, I will admit that the proof of contagion is complete. I shall then be satisfied, and I will answer for the other bigots being satisfied and dropping the contest; but till that shall be accomplished, all the labour of Dr. Fraser and the other pro-contagionists will only be so mueh labour lost. We know that *post hoc* is too often, in sueh cases, put for *propter hoc;* we know also how little scrutiny is often thought necessary to distinguish coincidental from causal ageney.

The ease of M. Schomburg, and of the "lady" who visited her neighbour, were, it is presumed, within the reach of Dr. Fraser's observation; but what do they prove? This: that persons living in the centre of the *diffused* cause of fever were affected by that fever, wheresoever its cause was observed. Beyond this they do not afford a tittle of evidence; and how they could be put forth as eonelusive arguments, and capable of establishing the personal contagious power of the disease, I cannot imagine.

It is stated by Dr. Fraser, that concurrently with the *intruder* from Barbados, remittent and intermittent fevers prevailed at George Town. But why an intruder? Not so mueh because it was unwelcome, as beeause it came from a foreign land. And why is it stigmatized as an alien? Chiefly, it would appear, beeause it was not a familar acquaintance, and because Dr. Fraser had not met it before in Demerara. Rarity of occurrence is no proof that a malady is imported, and that it is the product of some far-off eountry. At Belize, remittent and intermittent fevers are common, while the coneentrated continued fever is rare; yet it breaks out there at times, without sign or suspicion of being brought from other places. The same is true of certain localities on the west coast of Africa. Ten years ago, agues were common in South Wales, as well as in many parts of the South of England, where they had not been known during a long series of years; but no one thought of trying to trace them to the arrival of persons from other places, whether marshy or dry. They were satisfied that the disease had a home origin, and acquitted all other places of the evil.

Seeing that the attempt to prove the importation of this fever is a failure, or rather that the attempt is not made, would it not have been well that Dr. Fraser, instead of telling us that the "*intruder found its way*" from Kerinane to Barbados, and thence to Demerara, had employed himself in investigating the cause of the disease on the spot where he found it? The common fevers of the country, he admits, have an endemic origin; did it never occur to him that this fever also might have an endemic, a similar, though not the same, origin? If it had, he should have sought for it diligently in the soil and its produets—in the subsoil, in the wood and wooden materials of the plaee, under the influence of the sun, and certain meteoric agencies. He would thus have been oceupied more practically, more philosophically, and with better chances of suceess, than in his attempt to trace the disease through a long and invisible line of personal communieation; and he might have discovered, without the aid of any such line, why it appeared at George Town, Berbice, the sylvan villages of the Indians (if it did appear there), and other places. To look far away for an evil which is under our feet and in our chambers, is not the part of wisdom; it is like squabbling about a distant boundary line, while the enemy is in possession of the citadel. Trying to traee this fever from Demerara to Barbados, and thence to Kerinane, is like the Indian looking for the end of the earth, or the poet in quest of the north; which

> "At York is on the Tweed; at Tweed is in the Orkneys; and there
> At Nova Scotia, Nova Zembla, or the Lord knows where."

I am, sir,
Your obedient servant,
JOHN WILSON, M.D. R.N.

London, March 19, 1838.*

* We have to apologize to Dr. Wilson for the delay in the publication of his paper; it was accidentally mislaid.—ED. GAZ.

OBSERVATIONS

UPON

VENTILATION AND WARMTH;

COMPRISING STRICTURES ON DR. ARNOTT'S WORK UPON THESE SUBJECTS.

By Julius Jeffreys, Esq.

[Continued from page 56.]

3. *On Ventilating and Warming by the aid of Combustion.*

Having in my last paper notified my intention of meeting the expectation of readers, who are desirous that the subject of ventilating and warming by the aid of combustion should be early discussed, I proceed to offer such remarks as the excited state of the public mind upon these subjects, and the author's writings and plans, appear to call for. As leisure at the present period is not afforded me for extending these papers to the length required for the full investigation of so wide a subject, and as such an extension of them might neither be acceptable to the journal which has favoured them with much space, nor to its readers—the subject becoming, from its length, one which cannot properly be discussed but in a connected treatise—it is my purpose to condense the subject matter as much as possible, by touching on those points only, correct views of which are indispensable for our guidance in ventilating and warming our houses. I proceed, therefore, at once to state the data or maxims, which, I presume, every reflecting person must admit to be those upon which we ought to proceed.

1st. When the temperature of the air is mild, we find, almost without any exception as to age, sex, or health, that the human frame is benefited by a frequent renewal of the air in which it is placed; so that when this change shall have been brought up to a degree fifty-fold greater than all the vital functions appear to require, more benefit still is to be obtained by a boundless flowing of fresh air past the body, provided always the temperature be suitable. Hence, in ventilating our buildings, in proportion as we value health, should we be ready to afford ourselves a supply of fresh air, properly warmed, the quantity of which needs no limit excepting that the current should not be so strong as to produce a sensible draught. And it may be shewn that a prodigiously greater quantity of air than we ever have, might be introduced, without any unpleasant draught, if warmed first, and at a cost very much below that which we are accustomed to expend upon our fires. I rest not upon the arguments contained in my first papers only, but on the habits, feelings, and observation of Englishmen, for support in these views; in favour of which, sound arguments might be multiplied without number. Among them, how remarkable is the following one, which the general reader may not know, but which the observant physician cannot overlook?—that in the treatment of diseases of the lungs, injurious as is the application of cold air to them during respiration abroad, the confinement of the patient within doors, however spacious the mansion, is often still more injurious. It is not the mere confinement, but the want of fresh air, that is the cause of the debility and functional disorders that arise; for the social circle, or place of public amusement, though often much more inviting, have not within them the renovating power which the outer (that is, boundless) air possesses. Hence in pulmonary consumption, even before the respirator was thought of, it was often necessary to require the patient to face the cold air abroad; the direct injury of which to the lungs was not so great as the indirect aggravation of the disease by confinement in a house, however spacious. If this did not obtain in all, or in a majority of cases, it was only because the destructive effect of the coldness of the air on the diseased organs was an evil which more than counterbalanced the highly renovating power of its freshness.

2dly. The means which effect the ventilation should act with due force, vigilance, and constancy. The means we employ must be either mechanical, or what may be termed natural. Mechanical ventilation, well applied, would prove highly advantageous for public buildings and large assemblies, though, in the instrumental means and the course of the current, experience compels me to differ greatly from the author. For domestic purposes, which are those I desire to consider at present, ventilation by mechanical means—that is, by any apparatus to be kept in motion—ought not to be thought of. It would not be economical, and it would be certain of interruption from derangement or neglect.

It would possess neither the simplicity nor vigilance of the natural means which result from the buoyancy of air warmer than the atmosphere. The atmosphere we know to be ever active in endeavouring to press upwards a body of air heavier than itself. This constant tendency ought, therefore, to be employed for drawing the air out of our rooms; and its power ought to be such as to exchange it as fast as we are able and willing to give a fresh and warm supply. Now the power is in proportion to the excess of the warmth of the buoyant air over that of the atmosphere, and to the height of its column. If greatly heated after leaving our rooms, a very short column, and therefore a short channel for it, would do; or if not heated above the warmth of the room, a very long column, and long channel for it, would be required. By heating it a little as it leaves our rooms, a column of the height of our houses, or even shorter, will suffice, if the channel and its opening into the room be of due capacity. Viewing this channel only as the instrument of ventilation, apart from any other office which may be given it as the conveyer of smoke, it ought not to open outwards on any one side of the house. Even though its mouth should be upwards, the smallest wind setting upon that side of the house would reverse the current, and blow cold air down the channel into our rooms. But by carrying this channel to the tops of our houses we remove, as far as possible, all interference from walls, and give the surest vent and draught to the current which is practicable. Opening above the house-top, this ventilating channel may also be made the channel for the smoke and other products of the combustion, which must go on in our houses for the purpose of warming the air. Now, these waste products of the combustion give the very warmth for buoyancy which the ventilating principle requires. We have here length, capacity, warmth, and an outlet above walls, for it as a ventilating instrument, and its same capacity and outlet as a smoke channel. A more perfect, simple, and constantly active instrument could not be devised; and this instrument is the Englishman's open chimney. This open chimney is not in fault when he lights a fire under it, so as to let seven-eighths of the heat pass up unemployed; for as it has a capacious opening below, one-eighth of the heat would be abundantly sufficient to give it the necessary draught. I will not here endeavour to shew that seven-eighths of the heat, as stated by the author and other writers, are not always lost, and that some fire-places send, at least, one-third or more of the heat into a room; because I know that the principles laid down by Count Rumford and other judicious persons, are rarely carried out, and that not a few grates, among which are some of the most expensive, are so set as to allow even more than seven-eighths of the heat to go up the chimney. It is, however, my belief that an open fire may be so burnt as to render effective at least three-fourths of the heat of combustion; and this without any fear of smoke or of tainted air entering the room. If this shall prove to have been effected, one of the common objections to open fires, which is, no doubt, of weight, and which has long been complained of by improvers of stoves—namely, the waste of the fuel, will be removed. But, nevertheless, rather than that the comfort and salubrity of open fires should be sacrificed, I would hope that most persons, after a while, would return to their use, even if the open grate did not admit of being thus improved.

In bed-rooms, indeed, I would by all means use a close stove, but I would never close the chimney, and would have an *especial construction for bringing in fresh air*. I think, also, the stove should certainly be a water, and not an air stove. The question of fuel being supposed at rest, the other chief arguments against open fires, are the cold draughts to supply them, which enter our rooms through the crevices, especially near the floor; and the drawing off of the air from the lower part of the room instead of from the top, where the foul air most readily collects. Now it may easily be shewn, that just as the open chimney is not in fault when seven-eighths of the heat are sent up it from an ill-constructed fire-place, so neither are the chimney and fire together in fault because the fresh air is let in to the room cold, and in an improper direction. Let a comparison be drawn between an ordinary open fire and a close stove, the air being allowed to enter the room cold through similar crevices in both cases. As the stove is placed near the closed chimney, the inmates are between it and the sources of the draught. If as much air were pressed by some external

force in at the crevices of the door, in the latter case, as the open fire would draw in, it is plain to every one that all the same draughts would be felt in the one case as in the other; and that when such draught is commonly felt with the open chimney, and very little with the close stove, it is because much fresh air is coming in in the former case, and very little fresh air in the latter. And if a stove be made to admit no more air up the chimney than a gentle combustion requires, which is the case with the thermometer stove, the quantity of air entering by the same crevices will be a trifling fraction only of that which the open chimney ensures. Let it be granted that the air is entering in both cases at the bottom of the room, constituting what the author calls the "cold foot-bath," when the quantity is large as in the case of the open fire. Is the mere nominal fraction that comes in when the chimney is closed, so well circulated about the room, and the large quantity, when it is open, so entirely hurried up the chimney as the author thinks, that the former pittance can be as effective in ventilating as the latter quantity? Very far from it. Adhering to the faulty and extreme case of the air coming in only at the bottom of the room, a great part will no doubt pass up the chimney unemployed, but a large portion also will rise near the mantel-piece, and circulate about the room, there being a variety of ascending currents near the chimney, near the walls and furniture, radiated upon by the fire, and of currents downwards near the windows and parts not radiated on, which, by chilling the air, cause it to descend. Where the fresh air entering is so large, there is a considerable quantity thus mixed up with the whole air of the room, even in the extreme case of its all coming in at bottom. But the open fire has the power of drawing air in at crevices at the tops of the windows with nearly equal force as at the bottoms of the doors. If the crevice under the door be closed by any means, as by a dropping, or by the simple plan of carrying the carpet underneath it, and thickening it with list below, the air is drawn in in very thin laminæ, or strips, through the broad and fine crevices of the windows, and *top and sides* of the doors. Now it cannot have a better form than such thin strips for ready diffusion with the air, and the greater part is entering much above the level of the chimney draught, so that it is well mixed with the air of the room, and performs the duty of ventilating it before it descends to pass up the chimney.

I would recommend a more methodical way of ventilating a room; but if any thing can give good effect to the common crevice ventilation (saving only the easily closed crack under the door), it is the open fire. With a close stove, *if the windows and door are closed*, not only is the quantity of air that enters the room pitifully small when compared with that of the open fire, but its tendency is rather to lose the little fresh air that comes in than to render it effective. The chimney draught being destroyed, the little draught there is, is from a tendency, very feeble indeed, but still some tendency, of air to come in at the bottom of the door, while air passes out at the crevices near the top of the windows. The air entering at the bottom of the room, and being heavier than the warmer air of the room, must pass along the floor with the set, or draught towards the stove; here the current becoming an upright or vertical one, the fresh air next the floor becomes, of course, when the direction is changed, the stratum applied to the stove, which directly converts it into hot air. Such air must rise to the top of the room, and be therefore the first to pass out through the upper crevices of the windows, with little tendency to mix with the air of the room. Such, certainly, is the general tendency of currents entering a room with a close stove, where the only vent is the upper crevices of the windows. The general circulation of air in the room ascending near the stove, and descending near the walls and windows, would not include the air *immediately applied* to the surface of the stove, which being much the warmest, would tend to remain at the top of the room until it could escape through the windows. Now the freshest air, as already shewn, would be in this position. Hence, upon the whole, there is, without doubt, a less power in such a stove to insure the mixing even of this pitiful quantity of fresh air with the rest, than in an open fire, which is drawing air through narrow fissures at all heights of the room, at the top as much nearly

as below, and with a pressure which carries the fresh air into the heart of the other air.

These remarks apply to any close stove in a room of which the chimney is closed, and where there is no particular provision for bringing in the fresh and carrying out the stale air. For the current through the fuel of any such stove, however wastefully rapid the combustion, must not be named as affording any supply adequate to satisfy the salubrious habits of Englishmen. But with a stove, the properly restrained combustion in which requires so very little air as does that of Dr. Arnott, these remarks apply themselves with especial force. It is proper, however, in referring to this stove, to quote the author's estimate of its power of ventilating ordinary apartments, Art. XC. p. 77:—

"Sufficient ventilation for an ordinary sitting room will be insured, in a cold winter day, 1st, by the demand of air for the combustion of the stove; 2dly, by the considerable change occurring through the crevices around doors and windows, which may be taken at about six cubical feet a minute for each; and 3dly, by the hundreds of gallons of fresh air which, every time the door is opened, enter and displace an equal quantity of the air previously in the room. In warmer weather, when the difference between the external temperature and that of the room is less, and there is, therefore, less tendency to spontaneous change, some additional means may be used from among those to be described hereafter. But the three already mentioned have so considerable an effect, that even in Russia, where they are the only means in common operation, and where they are counteracted very much by double doors and windows, and the closest fittings every where. In rooms without an open fireplace, and heated entirely by the action of stoves which are fed with air from the lobbies or passages, they are still sufficient."

In my first paper I had to notice this repeated argument, drawn from Russian customs, as manifesting a surprising departure from the doctrine in favour of copious ventilation he had so well laid down in the early part of his treatise; and in that paper the utter inapplicability of any argument drawn from Russian ventilation to the case of our English climate was proved, by shewing that as a hundred-fold more ventilation was necessary in tropical climates than in England, and this irrespective altogether of coolness, so a manifold greater ventilation was needed in England than in Russia, and this even granting that it is not through scarcity of fuel and rigour of climate that the Russians, the poor especially, are subjecting themselves to an atmospheric condition which must be deleterious to any human beings in any part of the globe.

I proceed to notice the three ventilating powers mentioned by the author in the above quotation:—

"1st, The demand of air for the combustion of the stove."

As already stated, no close stove demands air enough for effecting ventilation. The demand of the thermometer stove is so very small, that Dr. Arnott, in naming it as a means of ventilation, does, in a most striking manner, make his practice to conform with the second views he had taken up, namely, those in favour of very scanty ventilation, contended for in Art. 82. By his own shewing the stove is so economical of fuel, and the draught through it so small, that the ventilation of an apartment must be imperfect, indeed, where this forms a fraction worthy of notice. Moreover, if this draught were such as to justify his placing it foremost, it would in an eminent degree be subject to the chief objection he urges against an open fire, that of drawing off the air at the lower parts of the room; for the stove draws air off from the very bottom of the room. If the ventilation through the stove were worth naming, it would create the "cold foot-bath" of the author in a most perfect manner. There is no cold foot-bath, because, and only because, in effect there is no ventilation through the stove.

2dly, We are said to have "the considerable change occurring through the crevices around doors and windows, which may be taken at about six cubical feet a minute for each." Engineers and architects are under the necessity of striking some average, among the various cases they have to deal with, as the leakage, supply of air through doors and windows. Any such estimate, however, is so very vague and imper-

fect, that they can no more intend it to be received as a practical rule for any one case, than would a statistician mean that his estimated average of the ages of a whole population should be declared to be the age, whether they were infants or old men, of the inmates of every room throughout the land. But, besides this, the average of six cubic feet has reference to rooms with open fires.

When the chimney is open, air is pressed in, as already stated, at all crevices, at the top as well as at the bottom of a room, and to an amount the value of which is to be measured by the square root of the buoyancy of the column of hot air in the chimney, and by all these crevices taken conjointly. But with the stove and closed chimney we lose half the crevices as inlets of fresh air, for the upper half of them become, of course, the only outlets. The draught is no longer *in at all the crevices* and out at the chimney, but it is *in at the lower crevices* of the room, *and out at the upper*. On this account, any estimate has to be halved. But the *resistance* is doubled in the case of the closed chimney, for the outlets are narrow crevices, offering as much obstruction as the inlets; whereas the open chimney, in the other case, offers practically no obstruction at all. The half supply has, therefore to be halved again. One quarter, then, is the utmost that could enter, if the pressure of air from without equalled that in the case of the open chimney. But the measure of this pressure is the buoyancy of the column of air to be displaced, and this buoyancy depends upon its *height* and its *heat*, above the outside air. In the case of the open chimney, the height is that of the whole chimney. In the case of the stove, the height is that only of *half* the altitude of the top of the windows above the bottom of the doors; for half the crevices are the inlets, and half are the outlets. About five feet will be the value of this column, while fifty feet is often that of the column in the open chimney. Again, as to heat, *the excess of the warmth* in the chimney over the outer air, is often five times that of the room over the outer air; as when the outer air is at 50°, the chimney at 100°, and the room at 60°. Now the estimate of the buoyancy is very nearly correct, when we multiply the height of the column by this difference of temperature*. In the case of the open chimney, we have ten times more height into a five times greater difference of temperature—that is, a fifty-fold greater pressure, bringing in air at every crevice; and the quantity that does enter is in the proportion of the square root of this pressure. The square root of 50 being about 7, the chimney, through equal crevice openings, would draw in seven times as much air as the stove. But, for reasons above shewn, this seventh has to be reduced four-fold; so that one twenty-eighth of the quantity which the open chimney would draw in (the door and windows being shut), is all that, in the same circumstances, could come in when the chimney was closed, and a stove in use. If, owing to variations in the *relative temperatures and heights*, the difference was less than this in favour of the open chimney in some cases, in other cases it would be much more. 3dly. As to the "hundreds of gallons of fresh air which, every time the door is opened, enter the room," this may be an allowable way of ventilating a hall, or lobby, and in halls and lobbies only should such close stoves, with closed chimneys, ever be used; but in a dwelling apartment, so far from being calculated on, it ought, and is, where double doors are used, to be avoided as far as possible. Though the entrance and exit of the inmates render some ventilation of this kind inevitable, there ought to be no case in which the assistance of so severely sudden, and so fluctuating and uncertain, a supply was acceptable. Sorry are the ventilating means to which this convulsive action has to lend its aid.

In concluding this paper, while I would desire to apologize to my readers for the length of the foregoing demonstration, I think that at the present time, questions connected with it must oftentimes be put to medical advisers, whose leisure does not permit them to trace out the investigation, and who may, therefore, find it convenient to have it prepared so as to require merely a perusal.

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* The bases being the same—namely, the area of the crevices.

TETANUS LIMITED TO ONE SIDE.

To the Editor of the Medical Gazette.

Sir,

If you consider the following case sufficiently interesting to permit a place in your journal, you will oblige by its insertion,

Your obedient servant,
W. P. Barlow.

Writtle, Chelmsford, April 26th, 1838.

Timothy Ruck, about 40 years of age, complained Feb. 13th, of soreness of the integuments of the left side of his head and face, and was surprised the following day with an uneasy sensation at the pit of the stomach, which extended thence towards the spine. This was rapidly followed by a tetanic seizure of half of the body. When I visited him I found that he was unable to speak, a circumstance of no import as far as the diagnosis was concerned, since the language of the disease was not to be mistaken. The attack was extremely sudden, and his wife (who was with him when it occurred) could give no other account of the matter than that her husband began to complain of a pain at the pit of his stomach and side, and was soon afterwards "taken stiff and speechless."

The attack confined itself to the left side of the body, the muscles of which were tense and rigid, and so strongly contracted, as to resist the application of any force to alter the direction of, or to relax their action.

The muscles of the face were in a state of contraction, and the lips, which were firmly closed, did not yield readily to the endeavour to separate them. The right side of the countenance was natural, and the mesial line formed the limit of the tetanus here as elsewhere. There was complete trismus: nothing could be introduced into the mouth, except a little fluid which found its way between the teeth, and induced deglutition, which was duly performed, and did not in the least exasperate the symptoms. The muscles of the back were tetanic, but those contributing to the respiratory movements had escaped completely: the ribs were equally well elevated on the one side as on the other, and there was no difficulty of breathing whatever.

The fore-arm was bent upon the arm, the thumb and fingers upon the hand, the whole member was forcibly approximated to the side, from which an inconsiderate by-stander, as I was informed, had used much and ineffectual force to separate it.

It gave evident pain to the patient to attempt to alter the direction of the fingers, or to press upon the contracted parts.

The lower extremity presented the reverse position of the arm, being perfectly straight, but was quite as rigid and unyielding. The abdomen was hard and tense. All the symptoms mentioned confined themselves to the left half of the frame; and the relaxed condition of one side of the body formed a contrast with the tetanic state of the other, which rendered the features of the affection yet more striking.

I need not observe that the symptoms I have endeavoured to sketch are common to tetanus, but I must add to the detail another which one would little expect to meet with in such a case. A fixed condition of the pupil of the left eye was observable, and not the slightest mobility was evinced when a candle was held near it. Vision seemed quite gone.

No cerebral disturbance was evinced: the patient seemed alive to his situation, and was conscious of what happened around him; his power of voluntary motion over the muscles of the right side was unimpaired, and he exercised it by moving the right arm and leg as if to signify they had escaped the attack. No doubt could of course be entertained as to the nature of the disease; the next consideration was how to treat it. In the treatment of tetanus, the most rational measures seem to be the removal of, or the allaying, if possible, any source of irritation which can be detected externally, or presumed to exist internally; to administer antispasmodic remedies, particularly opium, in the quantity proportioned to the extent to which the system tolerates them in this affection; to pay attention to the condition of the digestive organs; and the more so where the disease is not consequent upon injury, but owes itself to some internal and unassigned cause. Bleeding, from its great power in lowering nervous energy, and its influence thence derived upon muscular action, is

deserving of a trial in many cases. The judgment of the practitioner must guide him under what circumstances he must avail himself of, or reject the operation. In this instance there was no indication against it, and it was deemed an advisable proceeding. A large quantity of blood was drawn, not enough to cause syncope, which, as he could not be raised from the recumbent position, it would have been rashness to induce. Scarcely had a minute or two elapsed after the blood-letting, when to my surprise I observed the fingers relaxing, becoming released from their constrainedly flexed position, and gradually getting straighter. The patient was soon able to move them, but volition recovered its control in the most gradual manner possible, and the tetanic contraction quitted first the fingers, then the fore-arm, and afterwards the arm. As the contraction ceased, the power of the will regained its due influence over the muscles, and it was most interesting to observe the slow yet easily perceptible degrees by which the tetanus subsided.

Whilst the arm was gaining its natural condition, the leg was also recovering, the jaw was being unlocked, and was soon restored to its proper extent of movement.

The affection subsided in every part it had attacked, leaving a soreness and stiffness which lasted a few days. The vision of the eye I have noticed as being affected, remained dim for a short time, but recovered gradually. The patient took some aperient medicine, and some opium was given him. He has had no return of the disease. Thus much with respect to the issue of the case, which was of eight hours' duration, and ended so suddenly and unexpectedly. The question arises, was the favourable result owing to the treatment which was adopted? I do not think we should be warranted in saying that the bleeding cured the disease, though the alarming symptoms vanished so speedily after the operation. Shall we admit the curative influence of the remedy, or conclude that the disease desisted—that it went as it came, without being able to give any satisfactory reason for its attack or departure? We have to consider the evidence the case itself affords as to the value and efficacy of the treatment, and these are, whether the results of experience permit us to draw a deduction, which observation, if limited to the case itself, would seem to sanction.

The record of cases, which have been rightly styled "as being to us what precedents are to lawyers," does not, I believe, offer an example of the disease coming to a termination so soon after depletion; and it would be a mere assertion to say, that because the disease ceased after the blood-letting, that therefore it was cured by it. More cases would be wanting of a similar nature and result to establish this conclusion. It is a careful deduction from many cases, not an opinion founded on a few instances, that stamps the value of a remedy. The numerical method, pursued with so much zeal and success by Louis, with regard to the diagnosis, must not be lost sight of with reference to the treatment of diseases. But the value of this method depends entirely upon the accurate observation and faithful report, and upon the care and acuteness with which the deductions from the record are arrived at. Where diseases are related of the same character, the favourable issue of which is ascribed to totally opposite remedies, we are at a loss to know which class to prefer, or whether to attach to them any efficacy; on the other hand, where affections are observed to run a certain course, if suffered to progress, without any effort being made to check them, and where certain remedies are known to arrest symptoms, which if those remedies are omitted, or means of an opposite kind employed, rage uncontrolled and become aggravated, we cannot but acknowledge their efficiency, and admit that the administration of the remedy and the subsidence of the disease bear the relation to each other of cause and effect.

In considering the various kinds of treatment which have been tried in tetanus, we cannot select any one which can be employed with the expectation, much less the confidence of success; and where we find an isolated instance in which a remedy seems to afford advantage, which has been tried again and again, under various circumstances, without good effect, we ought not, on such a ground, to place on it an undeserved reliance, much less ought we to reject it altogether; but we should be induced, by the hope that it has been of service, to give it another trial.

THE MORBID ANATOMY OF SCROFULOUS FORMATIONS.

By F. W. Grant Calder,
Assist.-Surgeon, 2d Regiment of Life Guards.

Communicated by Sir James M'Grigor, Bart. Director Gen. Army Medical Department.

The following observations on morbid structures were collected during the time that I had charge of the Anatomical Museum of the medical department of the army at Fort Pitt, Chatham. I use the word scrofulous tubercle, *not* because I consider it to be the invariable result of what is called the scrofulous diathesis, but merely in contradistinction to the terms scirrhous tubercle, encephaloid tubercle, melanoid tubercle, and so on.

Scrofulous formations.—The term scrofulous formation is used to denote a subtance of a peculiar nature, different in structure as well as in appearance from any of the organs which compose the healthy anatomy of the human body. It is an adventitious production, and constitutes the basis of phthisis. It occurs in four principal forms.

1st, In distinct round bodies, to which the name tubercle is more properly applied.

2nd, In masses which vary in size, and are commonly of an irregular shape.

3rd, It becomes sometimes so diffused into the structure of an organ, as to have received the name of "tuberculous infiltration;" and, lastly, either a whole organ, such as the testicle, or a portion of an organ (for instance, the liver), becomes occasionally so completely converted into this morbid product, that the name tubercular conversion may with some propriety be used.

I. *A tuberculuted form.*—Tubercles, when first distinctly recognizable, are of the size of millet-seeds or pin-heads, are of a semi-transparent greyish appearance, irregular in their outer aspect, of firm consistence, with difficulty compressed between the fingers, and invariably adhering pretty firmly to the surrounding tissues. When seen in this state, the term miliary, granular, or unripe, is very applicable; either term, in my opinion, may be used indiscriminately, either being sufficiently characteristic of the state described; and however this opinion may be at variance with some of the best morbid anatomists of the present day, yet, from extensive observation, I feel convinced that it neither answers a practical nor a scientific end to make a division *. In this state they may remain for a length of time, varying in size from that of a millet-seed to that of a grain of wheat, seldom, however, attaining this latter size; afterwards they enlarge, become of a whitish yellow appearance, and rarely increase beyond the size of a pea. When in this condition they often very much resemble cheese, and may now be considered as ripe. The term crude has generally been applied to this latter state of tubercle. I think, however, that the word crude might with more propriety be applied to it while in the miliary state, for while in that stage its firmness and its general appearance sufficiently characterize its unripeness; while its after progress from a harder to a softer state, its enlargement and change of colour, rather demand the name of *ripe* than any other. Moreover, when tubercles have once arrived at this latter stage, they are then on the very eve of breaking down, and, indeed, more frequently than otherwise present a small speck of softening either in their centre or in some part of their circumference. Finally, their fourth course is one of softening and decay, becoming converted into a heterogeneous fluid, which much resembles, both in appearance and consistence, pus. Such is a general description of scrofulous tubercles; yet, when they are observed to be few in number, they now and then attain the size of a cherry.

I likewise fancy that I have seen them in a stage even earlier than that described as the miliary state. I am led to make this observation from having remarked, that in the bodies of some who have died of phthisis the abdominal cavity has been found so extensively studded with tubercles, that it was difficult to say whether it or the chest contained most; and when its anterior parietes were turned back, the peritoneal lining was to be seen striated by

* Andral combats this opinion, that tubercle is at first a semitransparent granule, as Laennec maintained, and asks, "If tubercle commenced necessarily by being a greyish semitransparent granule, this latter should have been observed in every part of the system where tubercles have been found. Now, I would ask, have such granules been observed in the lymphatic ganglions, when tubercle can be followed in all the phases of its development?" In answer to this, I should say, that I have never seen tubercles of any description in lymphatic ganglions, and that when these bodies do become affected it is altogether from a different process.

tubercles both in the miliary and ripe state; and along with these, but separated from them, many greyish coloured spots of the size of pin-heads, not sensibly elevating the peritoneum, but distinctly visible through it. These spots, when more minutely examined, have a roundish shape and a distinctly circumscribed edge; and when divided with a firm cutting instrument, can be satisfactorily ascertained both by sight and touch to be a substance, and not a mere appearance. I have repeatedly seen this change in the subserous tissue of the lungs and intestinal canal; I have likewise met with it in the substance of the spleen, and I have never seen it but when extensive tubercular disease existed in these parts. Although we have hitherto been accustomed to look upon the miliary state as the primary stage of tubercle, yet it is clear that there must be a stage previous to this. Both Cruveilhier and Andral believe that tubercles are liquids at first; and Dr. Elliotson, of the London University, in one of his lectures in the MEDICAL GAZETTE, has remarked on this head "that one cannot conceive that vessels will pour forth solids; they all appear destined to hold liquids; however short a time the matter may remain so after its escape, it may become hardened immediately."

Tubercles, when met with in the chest or abdomen, can be very satisfactorily traced to have their origin either in the submucous or subserous cellular tissue, and therefore, reasoning from analogy as well as from observation, I am inclined to look upon this tissue as the general seat of tubercles. I have no wish to deny that tubercles may not be found on secreting surfaces, or that they have not been seen in the blood; but, without splitting a hair too nicely, it may be stated as a fact, that we have no difficulty in proving them to have their origin in by far the greater majority of instances in the cellular membrane of organs. Of course I allude here to tubercular matter occurring in the form of tubercle, and not to tubercular infiltration or exudation, which is another thing in so far as regards its primary origin.

I am quite at a loss to conceive how Dr. Carswell should consider the free surface of mucous membrane as the chief seat of tubercle, for I am not aware of a single preparation being at Fort Pitt, corroborative of this statement, nor have I ever seen a tubercle on the free surface of any membrane, whether mucous or serous. I am therefore inclined to remark that a certain degree of obscurity must necessarily be attached to the expression "free surface;" for if by this he means that the tubercle is actually lying on the surface of the mucous membrane, then indeed is the case rare; but if, on the other hand, he means that they are still covered with membrane, then is the case altered; for if at all covered with membrane, they will on further examination be found to have their seat in the cellular tissue beneath.

In some cases I have seen the lungs and intestines thickly studded with tubercles in a ripe state, and many already in a state of softening, and when not a single tubercle could be descried in magnitude greater than half the size of a pea: in cases of this nature it seems as if the miliary tubercles at once ripened, becoming yellow and soft, but without any increase in size. I have not indeed seen this state often, but when I have witnessed it the tubercles existed in myriads, and the lungs were completely beset*.

Laennec was of opinion that tubercles begin to soften first in their centres, and there is no doubt but that they do; but I agree with Andral in asserting that this softening does just as often commence on their surfaces. In this process a great deal likewise depends on the state of the adjacent tissues, for I am satisfied that the majority of intestinal ulcers in those who die of phthisis are owing to the tubercle acting as a local irritant, thereby occasioning inflammation and ulceration, by which processes the tubercle is frequently set at liberty in a partly broken down state. It is, indeed, no uncommon occurrence, in examining recent ulcers of the intestines with elevated edges, to verify this fact; for in such a case the tubercle can be readily turned out of its bed whole and apparently untouched, or with its surface being merely somewhat irregular from the action of the puriform fluid which surrounded it. The same changes take place in the lungs, and

* Louis suggests the probability of tubercles being sometimes primarily deposited in the lungs in a caseous state, and without their undergoing the gradation from the miliary stage. The reverse, however, of this in almost every case of phthisis is so easily proved, that I apprehend the remark has very little to support it: yet more of this anon.

this accounts for the tuberculous masses which are now and then coughed up.

Tubercles are generally described as unorganized bodies; I am not aware that their vascularity has ever been proved, but I take it for granted that until they arrive at the stage of maturity, *i. e.* until they have acquired the yellow cheesy appearance, they do possess life and organization, and that they increase in size by a power inherent in themselves*.

Andral is of a different opinion, and considers that a tubercle increases by additions being made to it by the parts which first secreted it. But we know that tubercles after a time totally change in their character, and how these additions should be of a different nature from the primary deposit, appears to me, from this theory, unaccountable; and I confess that from analogy I should rather be inclined to suppose that if they are primarily seen in a semi-transparent opaque state, the additions should rather be of the same nature. Moreover, even should we admit this theory of Andral's, it is to be presumed that we should now and then meet with a miliary tubercle in the centre of a cheesy one, a circumstance which I am not aware of ever having witnessed. When first seen they are firmly connected to the adjacent tissues; they regularly grow from the smallest point to the size of a pea, as the case may be; and they afterwards sooner or later decay. Analogy would thus lead us to decide that they are organized, however difficult it may be to verify the assertion; for I am satisfied that although by juxta-position tubercles frequently form larger aggregate masses, yet I cannot see how this alone should influence their progressive changes from a greyish transparent body to one of a yellow cheesy consistence, nor can I see how they should not occasion just as much irritation while in the miliary stage as in the ripe one, provided that from the first they were purely inorganic.

The subject, it must be confessed, is an extremely difficult one, but I think that some light is thrown upon it by the following cases which I have witnessed sufficiently often to prejudice my mind in favour of the statement which I have made. Solitary tubercles, situated immediately under the pleura pulmonalis, are met with under these circumstances; thus, within an area of two or three inches we shall have three tubercles: the first will be an unripe one—a greyish semi-transparent body, about half the size of a grain of wheat; the second will be of the same magnitude, but it will be ripe, it will have acquired the yellow cheese-like character; and the third will likewise be ripe, possessing the characteristics of the second. The pleura, covering the first, will have its healthy, smooth, and glistening appearance; while that covering the second will be vascular, and may be moreover coated with lymph; and, again, the pleura over the third will be ulcerated. Now this is a case by no means uncommon, and provided that the greyish semi-transparent body was equally unorganized with the others, how should it not have been occasioning the same changes; for they are all of the same size, and therefore this can have nothing to do with it. The other two, however, having arrived at that stage when they no longer possess life and organization, are acting the part of foreign bodies; and nature, ever anxious to exert her best influence for the preservation of her noblest work, has, in the first example, done nothing; in the second she has succeeded in presenting a very sudden and a very fatal event by the thickening of the pleura, and the timely exudation of organizable lymph on its surfaces; while in the third she has failed. I can, therefore, see no good reason why she should not have been directing equal attention to the greyish semi-transparent one, provided that they were all equally inorganic—all equally destitute of vitality; for if so, it is but fair to conclude that they should have been all equally prejudicial.

Some may be inclined to attribute the change from a greyish to a yellowish state altogether to chemical decomposition; and, moreover, Dr. Elliotson offers this as an explanation for the central softening which is sometimes met with in them. In answer to this, however, I would say that this central softening is to me one of the best proofs of their organization; for medullary tubercles, which are indirectly organized,

* Dr. Cowan, in his excellent translation of Louis' work on Phthisis, has well remarked "that all secretions, not organizable, must be expelled or become encysted, therefore; every secretion which does not tend to encyst itself, and which can exist a long time without producing irritation, shares in the common life." And he asks, "Is not this very often the case with tubercles?"

are frequently met with, having little central cavities. And what is worthy of remark is, that the cavity in both these kinds of tubercles is sometimes found empty, and this, too, when no outlet can be detected. The *rationale* of such a case appears to me to be, that the contents must have been absorbed; and should this be admitted, then this central softening, instead of being brought forward as a proof of their want of organization, would at once be one of the strongest reasons for believing in the opposite doctrine.

An interesting question here presents itself; but one, however, which appears to me to be exceedingly difficult of solution, viz. the tubercles having once acquired their yellow cheesy character—in short, when they have, according to the language of the text, become ripe—how long do they, or how long may they, remain so? I believe there is no evidence on record of tubercles, while in their greyish semi-transparent condition, being found either in a state of softening or apparent disorganization. And it, therefore, appears clear, that until they have acquired the yellow cheesy characteristics alluded to, they neither occasion inflammation, nor, in fact, do they produce any changes in the animal economy, which very egregiously interfere with the health of the individual. For how often do we find the lungs of individuals who have been cut off by some accident, or acute disease of the head or abdomen, minutely enough studded with miliary tubercles, and who, during life, exhibited no symptoms of disease. I believe that it is impossible to say how long they may remain innocuous in the body after having acquired their yellow cheesy consistence. And all, I believe, that can be said on the subject is, that to all appearance they are still organized even when they have acquired this character; but that there is a step a little beyond this induced by the tubercle having less firm connexions to the surrounding tissues, by being less consistent in itself, and by its acquiring a peculiar friability which it did not previously possess; all of which indicate that it has now arrived at that stage when it no longer possesses life and organization; and that it is now about to separate from the body by acting the part of a foreign substance, occasioning inflammation and its consequences *. But what those laws are which influence or govern these succeeding changes of softening and decay, I am equally unable to give a satisfactory explanation of. We know that scirrhus has a disposition to assume an ulcerated state; and we likewise know that medullary tumors have a disposition to throw out a fungus; but as to the *ratio operandi* by which these processes are accomplished, I am afraid we know little. The same may be said of scrofulous tubercles; we know that they are at first clearly distinguished as small semi-transparent bodies; that from this state they, for the most part, enlarge, and afterwards assume a yellow appearance, and then ultimately decay. There is here clear enough evidence of their being at least attended by an action of a specific nature; but I have never seen any proofs to convince me that that action was inflammatory. Tubercles, both in the miliary and ripe state, are daily met in the tissue of organs which present no evidences of inflammation, or of any other unnatural appearance, save the existence of the adventitious production in question. Hence there is little doubt but that in the first instance they are the result of secretions subsequently becoming hardened, and susceptible of those changes already described—changes the nature of which are to be attributed to the power inherent in themselves. And when Laennec, Bayle, and Louis say that their first appearance is owing to a peculiar diathesis, they have said every thing that, in my opinion, is worth attending to. The following case may be shortly stated as illustrative of this:—An old German died at Fort Clarence, who, for the greater part of his life, had been subject to repeated attacks of chronic bronchitis. On dissection, the bronchi and their mucous lining bore strong testimony of the repeated attacks of inflammation during life, yet the pulmonary structure was perfectly healthy; and Dr. Davy†,

* Louis talks of tubercles taking twenty and forty days to soften; but really nothing, I conceive, can be more difficult to decide than this.

† This gentleman, who is already so well known to the profession, and whose industry and unwearied application to the cause of science form such a bright example to the youth of the medical department of the army, has in a very marked degree lessened the deaths from phthisis, which used to occur in the Military Lunatic

the principal medical officer at the establishment, who superintended the post-mortem examination, well remarked, that if mere irritation had any thing to do with their production, this was, indeed, a fair case.

Tubercles, when they have once left the miliary stage, do not always assume the yellow cheese-like character which has thus been described; on the contrary, they now and then assume quite a different appearance: they undergo what has been called the cutaneous transformation, *i. e.* they become converted into a substance of a white putty-like consistence. Andral accounts for this change by saying, that after they (the tubercles) have existed in the constitution for a considerable time, they lose part of their animal constituents, and acquire an excess of the phosphate and carbonate of lime (which only exist in small proportion in their primary state), and thereby attain a degree of hardness depending on a gritty matter, which may be squeezed out by pressure. I have many doubts as to this being *bonâ fide* a transformation at all; on the contrary, as I have never met with a tubercle which was partly composed of this chalky substance, and partly of the true tuberculous matter (a circumstance which, it must be confessed, should be now and then seen, provided that the one really did pass into the other,) I am, therefore, more inclined to attribute it to the result of a peculiar diathesis existing in the scrofulous disposition. I have attributed it to the result of a peculiar diathesis, because I have never met with a single tubercle in a lung, or any other part, so affected; and I have always observed, that when this state existed, there were just as many possessing this peculiar characteristic as not.

The following case, which is very much to the point, I give at large; it possesses some other marks of interest besides the one in question, but which I shall refer to in another place:—Private John Adam, 1st bat. 60th Rifles, a Scotchman, and somewhat under 20 years of age, was admitted into the General Hospital, at Chatham, from the depôt of his regiment at Newcastle. Eighteen months previously he had enlisted, but he had never been able for his drill, and consequently had done very little duty. For five months before being sent to Chatham, he had been in his own hospital with symptoms of mesenteric disease, but had received no decided benefit. On his admission into the General Hospital his abdomen was rather tumid; he complained of great pain across the umbilical region, which was increased on pressure; he also complained of pain in the regions of the liver and spleen, and of tenderness on pressure in the epigastric region. He had likewise frequent palpitations of the heart, and now and then flying pains over the chest generally. Pulse small and quick; tongue furred and foul; bowels irregular, and with hectic symptoms at night; body much emaciated; he had no cough, but he was sometimes troubled with sudden attacks of difficulty of breathing. In this state he continued for some weeks, being sometimes better and sometimes worse; ultimately, well-marked symptoms of peritonitis supervened, and he died delirious after a few days of acute suffering.

Body examined twelve hours after death. Head.—About two drachms of fluid, of a serous character, were contained in each lateral ventricle, and about half an ounce of fluid of the same description was found at the base of the brain. The brain and its membranes generally presented nothing worthy of particular remark.

Chest.—Heart natural. In the upper lobe of each lung were a few tubercles; some adhesions, which, although they could not be described as old, were yet not of recent date, and which connected the lungs to the parietes of the thorax, were minutely studded with them. Beneath the pleura, which lines the convex surface of the diaphragm on each side, many tubercles of rather large size were situated, and, with the exception of the few which I have already mentioned as being met with in the upper lobe of each lung, these organs were very healthy.

Abdomen.—On opening this cavity, about two or three pints of a turbid serum escaped; the intestines were found matted together by adhesions, both of ancient and recent date; and the other viscera generally had contracted adhesions to their neighbouring organs. In the subserous cellular tissue, both of

Asylum, at Chatham, by having its inmates removed from the low and dark cells which they used formerly to occupy, into others of a warmer, more airy, and more comfortable nature.

the peritoneum covering the viscera, as well as of that lining the walls of the abdomen, immense numbers of tubercles were situated. In the adhesions generally which connected the viscera, tubercular deposits were likewise found. Tubereular matter was likewise deposited in layers under the serous coat of the liver, and a large collection of this sort was being evacuated into the stomach by means of an ulcerated opening in the lesser curvature of this organ. Beneath the peritoneum lining the walls of the abdomen, in some places layers were likewise deposited in a similar manner; indeed, the most extensive tuberculous disease existed in the cavity which can be well conceived. But what I wish partieularly to direct attention to is the fact, that with the exception of the few miliary tubereles in the upper lobe of each lung, which were still in the miliary or unripe state, all the others were converted into the white putty-like production under consideration.

It would be dealing too lightly with the authority of Andral to say that such a state may never be the result of transformation; but I hope at least that I shall not be considered arrogant, if I attribute the change in this case to a peculiar diathesis.

Tubercles thus affected, and congregating together in a portion of lung, commonly present a dirty white-coloured mass, of the consistence of wet plaster of Paris; and if allowed to dry on the fingers have quite a chalky appearance. Often in the centre of such a mass concretions of a firm bony nature are found: they vary in magnitude, and seldom equal in size the tip of the little finger: they resemble most those concretions which are called *bronchial*, and which are sometimes spit up by individuals who are not phthisical*.

Sometimes the tubercles at the apex of a lung will be so affected, while those in the middle lobe will have their usual cheesy-like appearance, and *vice versâ;* and again, the whole of the tubereles of one lung will possess this characteristic, while those in the other will present what is considered their more common appearance. Moreover, a single bronchial gland, or a solitary tubercle, may now and then be found so converted, but this must be considered rare. It ought, moreover, to be particularly kept in remembrance, that tubereles do not always present, even before they begin to soften, the yellow cheesy appearance which has been described; but, on the contrary, do now and then, throughout the body generally, exhibit a peculiar dirty-white colour, and this, too, independent of any chalky diathesis being apparent in those already softened. More frequently, however, the reverse is the case; and this white-coloured tubercle, on breaking up, gives rise to the cutaceous appearance which we are describing. These white-coloured tubercles are sometimes few in number, and of large size, and in addition possess a peculiar cartilaginous hardness*.

I have seen the absorbent glands of the abdomen thus affected, and presenting, on a section being made, a snow-white appearance.

I am not inclined, from all that I have observed, to attribute this chalky appearance of tubercle to transformation; *i. e.* I do not conceive that a tuberele, which at first possessed the yellow cheese-like character, subsequently changes into this white putty-like production; because we know that the vessels of the lining membrane of a tuberculous cavity secrete a substance essentially tuberculous; and in those cases where a part of a lung has been affected with these chalky tubercles, I have seen the lining membrane of a cavity partially coated with a substance in every respect similar to them. In such a case as this it is just possible that the cavity may have been coated with the softened remains of those tubercles which had previously occupied it; but as a pretty good proof of tuberculous matter of this nature being secreted in this manner, I may mention that there is a preparation at Fort Pitt which I made, and which is

* Dr. Davy, who so much excels in chemical investigations, analyzed a few of them for me; and he gave it as the result of his inquiries, that they were chiefly composed of carbonate of lime, with a little phosphate of lime, and some animal matter.

I have sometimes met with a solitary concretion (of this nature to all appearance) intimately adhering to the substance of the pulmonary tissue, and occurring in a lung which was otherwise healthy. This I had lately an opportunity of pointing out to my kind and much-esteemed friend, Mr. Bett, surgeon of the regiment in which I have the honour to serve. The subject was a young soldier, who had died of fever, and whose lungs were merely congested.

* I once met with a tumor the size of a walnut, of a regular bony nature, in the centre of a phthisical lung. I presume it had been previously something of this nature. It was made into a preparation.

part of the upper lobe of the lung, exhibiting a small cavity, the size of a hazel nut, lined by mucous membrane, and into which three bronchial tubes enter, or rather terminate. The cavity contains tuberculous matter of this peculiar white colour, and two of the tubes are crammed with it.

[To be continued.]

CLINICAL REPORTS.

By John Burne, M.D.

Physician to the Westminster Hospital.

[Continued from page 242.]

The Adynamic Spotted Fever with Pneumonia.

The occasional presence of pneumonia in cases of low fever has been noticed by many writers under the term "pneumonia typhodes," and inasmuch as this inflammation is masked and obscured by the torpor and dulness of the senses peculiar to the adynamic fever, it has been termed also "latent pneumonia."

That this pneumonia is frequently overlooked there can be little doubt, the condition of low fever engrossing the attention of the practitioner. Dr. Addison remarks, "the respiration is somewhat hurried, but there is seldom any obvious cough or expectoration, and sometimes none at all; in short, the whole assemblage of symptoms bears a most striking resemblance to those of a severe attack of common continued fever of the typhoid type, for which it is so repeatedly mistaken." He regards "a pungent heat" of the surface as the most constant and conclusive symptom in a diagnostic point of view*. Frequent as this latent or typhoid pneumonia may be, I have never known it prevail as in the present epidemic spotted fever, scarcely a case of which has come under my own care in which it did not exist in a greater or less degree.

The prevalence of an adynamic pneumonia, on the authority of Dr. Stokes, is not uncommon in Ireland. Under the head "typhoid pneumonia," he says, "we find a pneumonia more or less latent, and accompanied by extreme prostration. This disease is frequent in Dublin; at times, indeed, almost epidemic*."

The manner in which this spotted fever with pulmonary inflammation develops itself should not be passed over without remark. Low fevers develop themselves in two ways: *one* spontaneously, when the condition of the body, however brought about, is of itself sufficient to stir up and give rise to all those actions and phenomena which constitute the adynamic fever, the development being slow and progressive; the *other* accidentally, when, from exposure to cold or other vicissitudes, the patient is seized suddenly, the fever is fully formed in a few hours, and with it springs up an internal inflammation†.

In describing "the adynamic fever accompanied with local inflammation," I have said, "when inflammation accompanies this fever from the commencement, it is very generally in those cases in which the way of attack was through the intervention of an accidental cause: thus, if a person who has been subjected to the influence of the primary cause, a contaminated atmosphere, and so has a condition of body predisposed to the adynamic fever, catches a violent cold, this character of fever is immediately developed, and together with it pneumonia, bronchitis, or some other organic inflammation‡." It is precisely this way of attack which characterizes the invasion of the spotted fever when accompanied with local inflammation, the patients being attacked suddenly after having taken cold, and the fever and the inflammation being developed forthwith,—a manner of invasion very similar to that of the ordinary phlegmasiæ.

I agree with Dr. Stokes that this typhoid pneumonia is one of the diseases in which the practical utility of auscultation is strikingly obvious. Without this means of diagnosis there can be no question but that the inflammation would escape detection—if I said generally it might not be far from the truth,—and allowed to proceed undiscovered, and consequently unresisted, the sacrifice of the patient's life would too often be the penalty of the oversight. An instance in point has come to my knowledge within the last week. It happened in a man who had been under

* Guy's Hospital Reports, No. IV. pp. 62 and 63.

* Stokes on the Diseases of the Chest, p. 338.

† Treatise on the Adynamic or Typhus Fever, by John Burne, M.D.; p. 17.

‡ Op. cit. p. 40.

treatment for extensive disease of the kidneys and bladder, and who died, as it was believed, from a low fever which had supervened, his medical attendants entertaining no suspicion of latent pulmonary inflammation. On dissection it was discovered that the immediate cause of death was an intense broncho-pneumonia, the bronchi of both lungs being found full of pus, and the inferior lobe of the left lung in a state of recent hepatization and purulent infiltration.

Unless the attention is awakened to the fact of the great frequency of this latent inflammation in adynamic fevers, it may be overlooked by the auscultator himself; for not only may the absence of severe cough and of much trouble in the respiration render him unsuspicious, but a casual examination of the fore parts of the chest, as the patient lies on his back, will yield no pathological phenomena, the anterior parts of the lungs not being the seat of the disease. It requires that the prostrate patient should be raised up in bed, and the posterior parts of the thorax examined, when the unequivocal signs of dulness on percussion, and of the crepitating wheeze, will reveal to the auscultator a pneumonia of greater or less extent and intensity. Without the aid of auscultation the state of the lungs might be surmised by a penetrating judgment, but it could not be determined positively.

Purulent Infiltration.—In adynamic fevers accompanied with pneumonia, it has occurred to me on former occasions to find purulent infiltration the anatomical character, having its seat in the upper lobe of the left lung, without a trace of engouement or hepatization, exactly as in Case III. (of the nurse.) This occasional disposition of the adynamic pneumonia to seat itself in the upper lobe, and in the left in preference to the right, is noticed by Dr. Stokes*, "but in the typhoid pneumonia the left is most often the seat of the lesion;" "and it is a curious fact, that we have observed an epidemic tendency to pneumonia of the upper lobes."

In the present spotted fever, I cannot go so far as to say that the pneumonia has been generally seated in the upper lobes, the lower lobes having been affected more frequently; but that a pneumonia may invade the upper lobes, and exist in them independently of any similar affection of the lower lobes, there can be no doubt. "It has been said," writes Andral, "that the superior lobes are seldom or never attacked with inflammation. We can affirm that they are inflamed frequently; less so, however, than the inferior lobes. In fact, of 88 cases of pneumonia, we have found 47 of the inferior lobe, 30 of the superior, and 11 in which the whole lung was inflamed*."

At the same time it is certain that the more usual seat of pneumonia is the inferior lobes, the right particularly. Laennec says, "Les parties inférieures du poumon sont le lieu qu'occupe le plus ordinairement la péripneumonie; et lorsqu'elle envahit successivement tout le poumon, c'est encore dans ce point qu'elle commence presque toujours†." To this opinion there is perhaps nothing to object; but when he goes further, and states that "Rien n'est plus rare que de rencontrer une inflammation bornée au lobe superieur du poumon‡," I should venture to differ with him; for, independent of those instances of purulent pneumonia confined to the superior lobes, a tubercular pneumonia commencing in the upper lobes, and continuing limited to them through its whole course, is far from uncommon. Laennec, however, adopting the opinions of Bayle, does not admit that tubercles can be regarded as the effect or termination of inflammation§, which one would suppose led him to deny the occurrence of a tubercular pneumonia.

Broussais, in ascribing "la phthisie accidentelle" to a "pneumonie phthisique ou tubereuleuse," does in fact insist strongly on the frequency of pneumonia in the upper lobes, while Laennec, affirming that nothing is more rare, says, "Ce fait incontestable est propre à montrer combien est peu exacte l'opinion emise dans un ouvrage (Traité des Phlegmasies Chroniques, par M. Broussais) d'ailleurs estimable‖;" a reflection unworthy of Laennec, and which the evidence in Broussais' work shews to be unfounded.

It is not here the place to agitate the

* Op. cit. pp. 318, 319.

* Clinique Médicale, tome ii. p. 317.
† De l'Auscultation Médiate, tome i. p. 167.
‡ Op. cit. p. 167.
§ Op. cit. p. 51.
‖ Op. cit. p. 167.

question of tubercles being the product of inflammation, or it would not be difficult to establish the fact that they are so frequently, even generally; which has, indeed, been done by Broussais.

It is interesting to the pathologist, and eminently useful to the practitioner, to observe how various affections of the lungs localize themselves habitually in particular parts: tubercular disease in the summit of the upper lobes, the right most frequently; pneumonia in the inferior lobes, also the right more frequently; purulent infiltration in the upper lobes posteriorly, the left perhaps more frequently; lobular pneumonia (which I believe to be the anatomical character of hooping-cough) in the anterior angle of the middle lobe of the right lung, and the anterior inferior angle of the upper and anterior superior angle of the lower lobe of the left lung.

The expression "*purulent infiltration*" conveys to one's mind the notion of pus effused into the intervesicular pulmonary tissue, infiltration being the "action d'un fluide qui s'insinue dans les pores des parties solides. L'infiltration de l'eau dans le bois*;" in which sense it appears to be used, on many occasions, by Laennec and others, who speak of "tubercular infiltration—l'infiltration cadaverique sanguine—infiltration of serosity in the pulmonary tissue," &c. With this meaning the term infiltration, as descriptive of any anatomical character of pneumonia, is incompatible, the pus not being *infiltrated* into the tissue, but *effused* into the air cells; a question, however, on which some authors speak with great ambiguity.

The dissection of lungs in a state of purulent infiltration leaves no doubt that the anatomical characters depend on the vesicles and minute bronchial tubes leading to them being gorged with pus; and hence the grey granular aspect of the pleuritic surface—hence the oozing of pus from innumerable points of the cut surfaces of the diseased lung, on pressure being applied; and hence a section of this diseased portion being washed, the pus may be removed, and the pulmonary tissue be restored nearly to its normal state. It is difficult to conceive without actual examination, the density, solidity even, of lung infiltrated with pus, justifying, so far, the term "grey hepatization" given it by Laennec, though in no other particular has it any claim to this appellation.

Whatever may be the difference of opinion of the seat of that pneumonic inflammation which produces the engouement and red hepatization, none can exist here. Pus is the proper product of an inflamed mucous membrane; and as the vesicles are merely the terminal tubes of the bronchi dilated in form of *cul-de-sac*, and lined consequently by continuous bronchial membrane which is mucous, one would expect that inflammation of the terminal tubes and vesicles would, as in bronchitis, induce an effusion of pus from the surface of the membrane, and that this, by filling and distending the vesicles, would form all the peculiar characters of the lesion. In no other way can one explain the presence of pus in the cells as the product of pneumonia; a fact verified by many cases of pneumonia in which the purulent infiltration is unattended either with engouement or hepatization in the slightest degree.

The purulent infiltration is, nevertheless, frequently found in conjunction with red hepatization; the seat of these combined lesions being, for the most part, in the inferior lobes: but even here, dissection will discover that the pus is in the cells and bronchial tubes, and not in the inter-vesicular pulmonary tissue. In a recent dissection, the coexistence of these two lesions in the same part of the lung gave it a remarkable appearance. The diseased lung, viewed on the pleuritic surface, presented the aspect of red hepatization dotted with numerous white spots, from half a line to a line and a half in diameter, under the pleura, elevating it slightly. They appeared and proved to be little collections of pus in the midst of extensive hepatization, communicating directly with the bronchial tubes, which were also filled with pus, and could be traced leading to these collections continuously, without interruption. These little collections of pus resulted from cells ruptured by the excessive distension having broken down several into one, as in emphysema.

The purulent infiltration is regarded by Laennec as the third degree of pneumonia; the word *degree*, as used by him, meaning *stage*. He describes

* Dictionnaire de l'Académie.

the three degrees of engouement, hepatization, and purulent infiltration, as succeeding stages of pneumonia, the engouement passing into hepatization, and the hepatization into the purulent infiltration; so that the engouement must preeede hepatization, and both must precede the purulent infiltration; a view decidedly erroneous, as will appear.

That the engouement and red hepatization stand in the relation of stages to each other one can understand, and that red hepatization is always preceded by engouement is more than probable; but the purulent infiltration (grey hepatization) is altogether independent of either of the preceding lesions, and never takes place as a sequenee of hepatization. Hepatization cannot pass into purulent infiltration as engouement passes into red hepatization; these two are properly stages of the same lesion; purulent infiltration is an independent state and produet.

The relation of these three degrees to each other is not described by Laennec with his usual perspicuity; he evidently felt himself embarrassed on this point. Not so Dr. T. Davis. He distinctly ranks the purulent infiltration as the third *stage* of pneumonia, in the description of which he says, "The pulmonary tissue preserving still the same firmness and granular structure as described in the second stage (red hepatization,) becomes of a pale or light straw colour. At first, slight yellow spots of concrete pus are disseminated here and there, and render the shades upon the hepatized surface still more varied, irregular, and mixed*." Again, "Usually the disease (pneumonia) runs the following course: the engorgement lasts from twelve hours to three days before the hepatization is complete; the hepatization lasts three days before points of purulent infiltration show themselves, and from that time until the pus is softened, it varies from two to six days†." How is it possible to understand this? solid red hepatization converted into *concrete* pus! What is "*concrete* pus"? What is meant by "the pus is *softened*"? On what evidence rests the assumed fact, that purulent infiltration succeeds hepatization; that hepatization passes into pus? I discover none.

Dr. Stokes speaks of "the suppurative *stage*," but does not discuss the subject of purulent infiltration. He writes, "I have nothing of importance to offer on the subject of the interstitial suppuration of the lung*." Andral also has adopted the notion of purulent infiltration being an advanced degree of pneumonia, which renders his account of this lesion confused and embarrassed. He has proposed to call it grey softening (ramolissement gris), an epithet less objectionable than grey hepatization, though not altogether appropriate. The nomenclature proposed by Andral, for the different anatomical characters of pneumonia, is certainly an improvement upon Laennec. In the acute pneumonia he admits three states of lung, which he designates *engouement, ramolissement rouge* (the red hepatization of Laennec), *ramolissement gris* (the grey hepatization of Laennec), *with simple purulent infiltration or formation of abscess.* In the chronic pneumonia he recognizes the same states, and, in addition, two others, which he calls *induration rouge* and *induration grise*†.

Concluding that the purulent infiltration is an independent product of pneumonia not preceded by engouement or hepatization, nor in any way related to these lesions—that it can take place as the only product or anatomical character of a pneumonia from its commencement, exactly as in bronchitis—may it not be proper to designate the disease *purulent pneumonia*, in accordance with the received term *tubercular pneumonia*, the cases being strictly analogous? The tubercular pneumonia representing pulmonary inflammation in which the product is tubercular matter; purulent pneumonia representing a pulmonary inflammation in which the product is pus.

24, Lower Brook-street,
May 1st, 1838.

* Dr. T Davis's Lectures on Diseases of the Lungs and Heart, p. 165.
† Ibid. p. 172.

FRACTURE OF THE PUBES.

To the Editor of the Medical Gazette.

Sir,

Should you consider the following case worthy a place in your valuable journal, the insertion of it would much oblige

Your constant reader,

William Gosse.

Hoddesdon, Herts,
April 25, 1838.

* Op. cit. p. 312.
† Clinique Médicale, tom. ii. p. 306.

January 17, 1837, John Carter, æt. 25, a stout healthy young man, was buried up to his middle, whilst at work in a gravel-pit, by the falling in of some earth. On being taken out he was unable to stand, and finding no fracture of the limbs, I was struck on examining his back by the protrusion of the left ilium. At first I thought there might be a separation of the ilium and sacrum, but these I found firmly united to each other, and my attention was then directed to the pelvis anteriorly. As he turned over on his back, I observed the ilia unusually separated, and on tracing the left os pubis I found a fracture of its body and ramus, about an inch from the symphysis pubis. When lying flat on his back, the fractured extremities were separated to the extent of an inch, so that nearly two fingers could be placed between the divided portions of bone; but when turned on his left side, the anterior part of the bone overlapped considerably its posterior parts. The injury to the soft parts was but slight, and required no active treatment. In order to keep the fractured extremities of the bone in apposition, I found all that was necessary might be attained by position—viz. keeping him lying somewhat on his left side, but not much. A broad belt was passed around the hips, to prevent any separation, and he was strictly enjoined to keep in one position; which he very carefully attended to, and recovered without one bad symptom. At the end of six weeks he was permitted to get up, and soon managed to get about, at first with the aid of crutches, but he soon threw these aside and walked with a stick only. For some months he waddled a good deal in his walk, but this he gradually lost, and at the present time he walks as upright and firm, and is as strong as ever he was.

TRACHEOTOMY—REMOVAL OF A PEBBLE FROM THE AIR-PASSAGES.

To the Editor of the Medical Gazette.

SIR,

THE following case, (occurring in the London Hospital, and drawn up by my friend Mr. Cook,) appears to me of sufficient interest to the profession to merit a place in a public journal, as it affords another illustration of success, in operating when pulmonary symptoms are of so slight a nature as to be well calculated to lull the practitioner into an imaginary security, when in reality an irremediable mischief might be extending itself over the structure of the lungs.

I shall feel obliged by its insertion in your GAZETTE, at your earliest convenience. I am, sir,

Your obedient servant,

J. LUKE.

39, Broad Street Buildings,
April, 1838.

John Tyler, æt. nine, admitted February 6, 1838, from the country, reports, that having been subject to a pain in the right side, he was recommended to keep a stone in his mouth, and about a month ago it slipped into the trachea. It caused, at first, great pain over the right mamma, and on the right side of the chest, and violent attacks of cough and dyspnœa, with acute inflammatory symptoms, for which he was leeched, blistered, and purged, with considerable relief. An attempt was made in the country to remove the stone by suspending him in the inverted position, but it nearly produced asphyxia.

He says that at present he cannot walk a mile without stopping, or even lying down, several times, from the violence of the cough induced; and his mother reports that he has frequent paroxysms of suffocating cough, especially at night.

He is a stout, strong-built little fellow, of a florid complexion, which becomes frequently purple after the attacks of cough. The clavicles are raised considerably even in ordinary respiration, and apparently all the assistant muscles of respiration brought into play, producing a slight general heaving, and a throwing back of the shoulders at each inspiration. The voice is slightly *cracked*.

On examining the chest, a very loud sound is heard on inspiration, and less distinctly on expiration, a little above the right mamma. It varies much at different times, both in character and intensity, sometimes resembling a steady sibilous râle; at others, excessively tumultuous, and more resembling the sonorous râle. The respiratory murmur is considerably obstructed in the lower part of the lung, and is masked by the râles. On the left side there is a much slighter wheeze, and the respiratory murmur throughout the lung is puerile.

The sound of the voice is very loud and distinct in the situation mentioned, on the right side.

The sound produced by coughing is very peculiar. It resembles the sudden and violent click of a valve; and gives one the idea of a large globule of hardened mucus being very suddenly stopped in its progress upwards by the closure of the rima glottidis. It is attended, as the boy tells us, by a feeling of suffocation; and there

is occasionally to be heard a croupy inspiration following it. The boy says that he feels something move when he coughs.

Sunday, Feb 11th.—A consultation of the surgeons was held on Thursday last (8th), but the boy not having had since he came into the hospital any urgent symptoms, it was agreed to watch the symptoms for a few days longer. On Friday night he had a very severe fit of coughing of nearly an hour's duration; and a consultation was held to-day, and attended by the physicians; but in the absence of more urgent symptoms, nothing was done. The sounds are at present much as when he came into the house.

Wednesday, Feb. 21st.—During the last few days considerable alteration has taken place in the character of the sounds. The sounds heard over the right mamma are certainly not quite so loud, nor so tumultuous, but on the left side are much louder, and in fact there is scarcely any difference between the two sides. The respiratory murmur is now masked by the râles *on both sides of the chest*. There is now, however, excessive noise in the trachea, and the same valvular click is heard on coughing, in the direction of the larynx, (which should have been mentioned before as its apparent seat.) As it appeared from this that the disease was extending, it was agreed on all hands that there was no reason for delaying the operation.

Friday, Feb. 23d.—The operation was this day performed by Mr. Luke. The boy was placed on the table in a semi-recumbent posture, the shoulders brought forward, and the head held backwards. An incision, one and a half inch long, was made through the skin and fascia, extending from the cricoid cartilage nearly to the interclavicular ligament: several layers of cellular membrane, and the isthmus of the thyroid body, were divided, and about an inch of the trachea exposed. These little vessels, which bled freely, were tied, and a delay of about ten minutes now took place, in order that all bleeding might cease before the trachea was opened. The trachea was then divided from above downwards, to the extent of nearly an inch; and the boy at once turned over on his side, towards the edge of the table. The clicking of the stone was heard loudly, and violently, during the coughing which the opening had induced; but as the opening did not seem sufficiently large to allow of its exit, Mr. Luke proceeded to cut out a portion of the trachea on one side of the incision. The stone, with the removed portion of trachea, was instantly blown out with considerable force, and to some distance, by a violent cough. The boy at once recognized the stone as that which he had put into his mouth, and seemed much pleased at the result of the operation.

All oozing of blood having ceased, the wound was carefully strapped up, and the boy placed in bed, supported nearly upright by pillows.

The stone was a transparent yellow pebble of somewhat of the shape of a kidney-bean, 9-16ths of an inch in length, and 7-16ths broad.

Evening, 9 o'clock.—The boy has not had a single fit of coughing since the operation, and is now quietly asleep, no air having come through the wound. He had fortunately taken a hearty meal before the operation, and has not required to be disturbed since.

24th.—Has passed a tranquil night, and slept a little: has not coughed since the operation. Pulse natural; face pale, but without any expression of anxiety. On applying the stethoscope to the chest we found that all the unnatural sounds had entirely ceased* on both sides of the chest, and the respiration might be described as *perfectly natural*. No air has come through the wound. Bowels confined. Ordered a senna draught.

25th.—To-day he is flushed; pulse is quick, and skin hot and dry; wound dressed and looking healthy, though no union has taken place.

Ordered to leave off milk, and take saline medicine with antimony.

26th.—Febrile symptoms relieved, and he is altogether doing well.

On the next day or two he had a little cough, (owing to a cold caught by the window having been left incautiously open), and a slight return of the wheezing. Small quantities of air and of mucus passed through the wound till about the 3d of March, but the wound gradually closed, and by March 12th was healed over.

MEDICAL GAZETTE.

Saturday, May 12, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

ON THE SYSTEM OF GRANTING FALSE CERTIFICATES.

In our No. for 28th April we said it was "notorious that certificates do not always mean in fact what they express

* A remarkable illustration of the rapidity with which chronic inflammation ceases, when the cause is removed.

in words;" and we used this as part of the grounds on which we proposed a system of school examinations, to be regularly arranged so as to meet the prevalent and most mischievous practice of preparing for the examinations for diplomata only during the month or two immediately preceding them. When we spoke thus of the erroneous statements very often contained in certificates of attendance on lectures, we had especially in mind the careless and unconscientious manner in which many teachers bestow their assurances of *diligent attendance* on pupils who they know have been most inattentive, but who, they say, had entered at the commencement of the season, and therefore might have come to all the lectures, if they had chosen to do so. We are aware of the extreme difficulty under which a teacher labours, when he feels that by refusing a certificate to any pupil who has attended but few of his lectures, he, in fact, defers the possibility of his passing his necessary examinations for a whole season, and thereby causes him most heavy expense of both money and time. We know, too, that every attempt to enforce attendance has very generally been annoying to all parties, and has done good to none; but still we contend that every teacher should consider himself bound in honour to certify only what he believes to be true, and should not give way to the habit of asserting most equivocal facts, if not actual falsehoods, merely because it is customary.

Since writing the sentence which we have above quoted, a case of a far more flagrant character—in fact, one of the most gross fraud—has come to our knowledge. We had long suspected that there were in the metropolis several persons, long since degraded from any feelings of shame or honour, who for a few guineas would sign certificates that pupils, whom they had never before seen, had attended lectures, which they had never delivered; but it is only within the last week that we have been able to establish a case clearly, and without the possibility of error. We have only to regret that the information we received was so far confidential that we are not at liberty to mention the name of the principal; but both he and every member of the gang (for there are several) to which he belongs may depend on it, that if we ever hear of a case of the same kind, in which a regard for our own honour does not keep us silent, no other consideration shall induce us to conceal his name, nor prevent us from exhibiting him, as we now shall his plans, to the scorn of those who are disgraced by belonging to the same profession.

The system which is pursued by these swindlers, who call themselves Lecturers, and are so registered at both the College and Hall, is this: — An intermediate person is generally employed, to look out for students who, by idleness, have failed in obtaining certificates at the schools at which they entered, or who are anxious to complete their education (as the filling up of the schedule is called), earlier than would be possible by fair means. As soon as he hears of such a case, he offers his assistance, and the money being handed to him, he posts away with it and the schedule to his principal, and presently returns, with the latter filled up to the full amount required by any Board of Examiners under the sun, and signed in due form. Thus, in the majority of instances, the teacher and the pupil never see each other at all; the coin of the one is merely exchanged for the signatures of the other. The rate of payment is about as follows:—Surgery, or Anatomy, or Medicine, for any length of time, 3 guineas each—or the

two first together for 5 guineas; Chemistry and Materia Medica, 2 guineas each; Botany and Medical Jurisprudence, 1 guinea each; and for a round sum of 20 guineas any one may have certificates of having gone through the whole curriculum of medical lectures, and by a few months' *grinding* may be fitted to pass all the ordeals to which he need be subjected.

Such is the nefarious system by which the regulations of the Boards of Examiners are evaded, and such is the plan by which, while serving only to fill the pockets of those who deserve no better treatment than common swindlers, some pupils allow their indolence and ignorance to be served before their better interest. For the punishment of those who write these certificates, we again must regret our inability to give up their names, but we will point out the kind of persons, and whereabouts they may be found. They always live in the neighbourhood of the large schools, where they have a dirty little room which they call a theatre, and pretend to lecture in, delivering, however, often not a single lecture on the subject they profess: their only real occupation is grinding and signing certificates. They are always amongst the loudest in their outcries against the restrictions made by the College and Hall—the system of registering—of demanding attendance in each course on separate days—of limiting the number of professorships which one person may hold,—all which, it is evident, puts their craft in imminent danger. They are the most virulent, too, in their abuse of the "hospital monopoly," the large schools, and all other things which place some parts of medical education out of their reach, and prevent them from certifying to every thing required of a candidate for examination. But they may be easily distinguished from the general class of medical destructive reformers, by never saying a word against "the certificate system." On this alone they split; by this alone the union of these equally worthy sects is prevented from being cemented: it is their only specific distinction.

The extent to which the obtaining of false certificates is carried seems to be quite unknown to the Boards of Examiners, though they have endeavoured to prevent it by numerous restrictions, which else, and to those unacquainted with the ingenious methods used to evade them, must seem unnecessarily close and complex. So common, indeed, is it, that many pupils now talk about it as if there were nothing at all dishonest in it: even the most respectable teachers have applications not unfrequently made to them to sign certificates for a year or two past, on the pupil entering to their lectures for an unlimited number of years to come; or, with an ambiguous degree of conscientiousness, on condition that the pupil should call at their door and leave a message or a card every morning, for a certain number of days. In the same manner, some enter to the cheap lectures or hospital practice, and attend them at the dear ones—not attending where they pay, and not paying where they attend; as, for instance, taking tickets and obtaining certificates at Glasgow, after having seen no practice except in London. We could point out schemes almost without number, with which we have at different times become acquainted, by which the most ingenious restrictions are still more ingeniously slipped through; but this, for the present, may be sufficient to put the Boards on the look-out to detect the culprits, from whom they should at once refuse ever after to accept any certificates whatever. They may be found out most easily at the beginning of the

season, when they always issue false advertisements of cheap lectures, two or three by each person; and by comparing the certificates presented to the Army and Navy Medical Boards with the dates of registration at the College of Surgeons or Apothecaries' Hall, when it will often be seen that there is contradictory evidence as to the time at which the attendance, if in London, commenced.

We are, of course, aware that what we have here exposed will be at once met by the assertion, that if the whole certificate system were done away with, and pupils allowed to go up to their examinations simply on the strength of the knowledge they have acquired, it matters not where, all these frauds would cease, as the occasion for them would end. This might, and doubtless would be; but we should require much better evidence than has yet been presented, or rather we should need *some* evidence, for none has yet been offered, that by this means there would be any chance that the pupils, even if as well fitted for their examinations, would be at all better qualified for practitioners. We have often said, that as yet a single examination, or even a series of examinations on following days, for which a person might not obtain a sufficient amount of spurious knowledge by grinding for a few weeks or months, has never yet been invented; and considering the pains that have been bestowed in attempts, we might almost conclude such a consummation to be impossible. Look, for instance, at the examinations for medical degrees in Paris, of which so much is boasted, and which are held up as models: the curriculum of medical education is there just as strictly laid down as here; no idea of dependence on examinations alone is suggested: and no where, perhaps, does more grinding go on. In short, look where we will, we shall find no place where the examination alone is trusted to; but, on the contrary, we shall see that where a set course of education is least insisted on, there proverbially the amount of knowledge possessed by the generality of candidates is also least.

We sincerely believe that the plan to which we alluded the week before last would contribute more than any other to the main end which every regulation should have in view, viz. to secure a greater amount of knowledge in practitioners, rather than any quantity of spurious information in candidates for examination. The frequent repetition of examinations, both in the several colleges and in the whole university, which are held during the entire period of keeping terms, both at Cambridge and Oxford (and, we believe, in some other universities,) certainly shews that every candidate who passes the examination does know what he seems to know; that the demanded degree of information, whatever be the standard fixed upon, has been attained; that degrees are not bestowed on those who have merely paid their fees, kept their terms, and negociated with a tutor. This is what we would see in medical examinations. Let the standard be fixed at any height that may seem advisable; but force that to be attained. See that the course of education prescribed be that which may best fit the pupil to become a practitioner, and force it to be followed in an honest and straight-forward manner, by refusing certificates from all who are not proved competent to teach, and by keeping a sharp eye to prevent the frauds which we have to-day exposed, and which, if one or more examinations were required to be passed in the course of the attendance at the schools, could scarcely be practised.

DECEASE OF MR. T. BLIZARD.

The notice inserted in our last number, though coming from a quarter we deemed authentic, was incorrect, both as to the decease of Mr. Blizard and the place of his late residence. Nevertheless, it is our painful duty now to record the death of this highly-respected surgeon; with regard to whom, no part of the eulogy we bestowed requires to be in any degree modified.

Mr. Thomas Blizard died on Monday, the 7th instant, at his house in Cumberland Terrace, Regent's Park, of inflammation of the lungs, originating in a cold, which he had in the first instance neglected.

DEATH OF DR. EDWARD HARRISON.

Dr. Edward Harrison died on the 6th instant, at Marlborough, on his way to visit a friend at Devises.

Dr. Harrison had long been before the public, having many years ago written on the subject of medical reform, and altogether led a very bustling life. He practised, during a considerable time, at Horncastle; but came to London about fifteen years ago, and commenced practising in spinal cases. In this he was so far successful as to obtain a good deal of business and considerable notoriety, though he did not succeed in gaining the confidence of his professional brethren. He was loud in his declamations against the College of Physicians, and offered to try the validity of its power to prevent him from practising as a physician without their license. The challenge was accepted, and the case brought into the Court of King's Bench; when the defence set up was, that he practised as a surgeon, not as a physician. By this shabby proceeding he gained a verdict, at the price of his consistency. After this he meddled no more with medical politics, but became a very busy reformer in his parish (Marylebone), and was one of the most active of the vestrymen under the new act.

Professionally, he bordered very close on the empiric, and had the character of being rather *exigeant* with respect to his patients, yet he was on other occasions liberal, having given 300*l.* to the University College Hospital, and 1000*l.* towards establishing an Infirmary for Diseases of the Spine.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

April 24, 1838.

The President in the Chair.

On Black Expectoration, and the Deposition of Black Matter in the Lungs. By William Thomson, M.D., Fellow of the Royal Colleges of Physicians and Surgeons of Edinburgh.

In a former communication, published in the 20th volume of the Society's Transactions, the author brought under the notice of the Society a number of instances of black discoloration of the sputa observed during life, and of black infiltration of the lungs and bronchial glands ascertained after death, in persons who, from their occupations, were particularly exposed to the inhalation of carbonaceous powders or gases, such as coal-miners and moulders in iron-works. It being his intention in a future communication to state the general conclusions relative to the occurrence of these morbid appearances in this particular class of persons, which seem deducible from the information that has hitherto been obtained, and to point out those circumstances which still require further elucidation, it appears to him that there are several collateral topics to which it is necessary previously to call the attention of the Society. It is to the discussion of these topics that the present communication is devoted.

The author, in the first place, briefly recapitulates the observations and opinions of authors respecting black sputa and black deposition in the pulmonary organs, previously to the time when the class of persons to whom he alluded began to attract attention, thus taking occasion to shew that black deposition may occur in the pulmonary organs, independently of the habitual inspiration of an atmosphere peculiarly loaded with carbonaceous matters.

Among those who have noticed the presence of black matter in the expectoration the author alludes to Hippocrates, Willis, Morton, Haller, Withers, Portal, Bree, Pearson, Chomel, and Laennec. The phenomena has been differently accounted for by the several authors who have noticed it; for example, by Morton and Senac it has been supposed to owe its origin to a secretion from the bronchial glands; by Morgagni, Haller, Withers, and Portal, to a secretion from the glands of the mucous membrane of the trachea; by Bree, to a secretion from the pulmonary air-cells; while Pearson and Chomel have regarded it as altogether of extraneous origin, as a

simple deposit from the atmospheric air. Of the first, or those who have considered the bronchial glands as its source, some have regarded it as a natural secretion of those bodies, whilst others have looked upon it as a morbid production. Portal distinguishes three kinds of black expectoration; the first depending on inhalation of carbonaceous matter; the second, on a fluid secreted by the bronchial glands; and the third, on blood extravasated into the air-passages; the last of these alone he considers to be attended with danger. They may be readily distinguished, he says, by subjecting them to the action of boiling water, which dissolves with far less readiness the black product of hæmorrhage than either of the other varieties.

Dr. Bree's opinion of its origin seems to have altogether resulted from the consideration of the phenomena of asthma; but though he denied the possibility of its being secreted by the bronchial glands, he thought it possible that it might in some instances arise from the glandules which open into the trachea and bronchi.

A like difference the author finds to exist in the opinions held by different authors on the same cause of the presence of black matter in the bronchial glands; some supposing it to be a secretion of the glands themselves, and poured by them into the trachea; others assigning to the matter a course directly the reverse, and alleging that it is conveyed from the lungs to the bronchial glands by lymphatic absorbent vessels. Of those, too, who have held the latter opinion, some have considered that the carbonaceous matter originates within the lungs; and others, that it is introduced into these organs from without. Reisseissen and Soemmering may be mentioned as the advocates of the former opinion, and Pearson of the latter.

The author next enters at some length into the consideration of the causes of the presence of black matter in the lungs, detailing in succession the opinions held by Dicmerbroeck, Haller, Morgagni, Reisseissen, Pearson, Bayle, Laennec, Andral, Becker, Mericdec, and Fawdington. Of these, some, as Reisseissen and Pearson, considered it to depend on the inspiration of minutely divided charcoal, which they supposed to be deposited in the air-cells, and thence very slowly absorbed by the mouths of the innumerable lymphatic vessels in the air-tubes and cells. M. Bayle, on the other hand, regards the black matter met with in the lungs as a product of morbid action, and consequently as generated within the body. M. Laennec, in his work on Mediate Auscultation, published in 1819, treats of melanosis of the lung and black pulmonary matter, as distinct in their nature and effects, his account of the latter corresponding very much with that of Dr. Pearson, to whom, however, he does not refer. According to him, we find more or less of the black matter in the lungs of almost every adult, its quantity *generally* increasing with the age of the patient. He believes that the two varieties of black matter may be distinguished not only by their chemical but also by their more obvious classes; and remarks, in confirmation of this opinion, that the colouring matter, or melanosis, although it stains the skin black, is very easily removed by washing; whereas the matter expressed from the black bronchial glands takes such a hold of the skin, that if we allow it to dry before attempting to remove it, it remains attached to this texture for several days.

M. Andral considers the nature of the black matter to be always the same; the only difference in the appearance produced by it being that, in the case of melanosis, it co-exists with an indurated state of the lungs, which is not found to prevail in ordinary discoloration.

In illustration of the part of his paper in which he speaks of the chemical evidence bearing upon the subject, the author cites the experiments of Dr. Henry (p. 168), as throwing light upon the question.

The author, in conclusion, introduces a few more cases, that have been communicated to him by others, illustrating the occurrence of black expectoration, or of black deposition in the lungs, in individuals whose occupations did not appear to render them peculiarly liable to an accumulation of carbonaceous matter in the respiratory organs; and in whom, at the same time, there existed no traces of a melanotic diathesis.

FREEZING OF CARBONIC ACID.

In the course of the evening meeting at University College, April 25th, a highly interesting experiment was exhibited by Professor Graham—viz. the solidification of carbonic acid. This was accomplished by means of an instrument invented by M. Thilorier, which we shall take occasion to describe in a future number. The gas is first reduced to the fluid form by pressure, and then, the pressure being suddenly removed, so as to allow a portion to escape, this becomes instantly vaporized; by which an intense cold (estimated at not less than *minus* 148°) is produced, with the effect of converting the carbonic acid into solid flakes, exactly resembling snow. This may be regarded as one of the most interesting experiments in modern chemistry.

MEDICAL CONTRACTS BY TENDER.

To the Editor of the Medical Gazette.

SIR,

I AM requested by the medical officers of the Edmonton and adjoining Unions, to beg you will favour them with the insertion of the following circular, in your highly-esteemed weekly periodical, as early as possible; inasmuch as it concerns the vital interests of the licensed general practitioners throughout the kingdom.

I am, sir,
Your obedient servant,
J. WATKINS.

Cheshunt, Herts,
May 8, 1838.

A meeting of general practitioners in medicine, residing in the Edmonton Union and its neighbourhood, comprising contiguous parts of Middlesex, Essex, and Herts, was held at the Angel Inn, Edmonton, on the 19th of April, when the annual recurrence to medical contract by tender, practised under the new Poor Law Act, was amply discussed; and it was unanimously agreed that the principle was prejudicial to the best interests of the medical profession, and of the poor, upon whom, under the provisions of that act, they are called upon to attend. It was resolved to petition Parliament on the subject, and that the following resolution constitute a principal prayer in the petition:—

"Resolved,—That from the operation of this principle of an unfair competition, your petitioners are obliged, in order to keep their own practice from being injured by adventurers in medicine, to offer tenders below the value of the services required of them, and thus incur an annual expenditure disadvantageous to their families."

Another meeting was held at the same Inn on the 3d of May, when the petition being numerously signed, it was resolved that the members for the counties of Middlesex, Essex, and Herts, be requested to give their support to the prayer of the petitioners; and it was presumed, by giving publicity to the proceedings of their meetings, the attention of the medical profession generally would be directed to the subject.

PRIZES AT THE MEDICAL SCHOOLS.

To the Editor of the Medical Gazette.

SIR,

MY former conviction that the real and best interests of medical students have always met with the support and advocacy of your journal, was only strengthened by the perusal of your leading article last week; in which you clearly exhibited the beneficial effects resulting from school examinations. That in any one instance these effects should be lost, and the interests of students disregarded, will, I know, cause matter of regret, both to yourself and the medical world in general; but such an instance has occurred, and in order that it may never take place again, I am induced to request the favour of its publication.

In the London Hospital, as in other schools, the system of distributing prizes to the most proficient students in the various branches of science, has for some time been adopted; and indeed it has become such a generally understood thing, that at the beginning of the season many of the teachers omit to mention that such is their plan; but when the session is ended, the examinations are held, and the prizes are awarded as usual. Among those, at this school, who have omitted to *mention* their usual practice, I may particularize Mr. Scott, Drs. Ramsbotham and Cobb; but among those who have omitted to *adopt* their accustomed plan, I can only mention Dr. Cobb. Some students of his class (that of Practice of Medicine) not having been informed that no prize would be given, devoted their attention to this branch of their profession, and when the course of lectures was concluded, were in the daily expectation of being called upon to exhibit their respective merits. But no notice was given. At length one of the candidates waited upon the Dr., to know when he intended his examination to take place; and to his astonishment he was informed that no prize would be contended for. The reason was inquired? And the answer given was, that no notice was given of it at the commencement of the season.

Such are the circumstances of the case, and the consequence of them will be, that all faith in the teachers will be destroyed; and that the stimulus to study being thus removed, the beneficial effects resulting from that incitement will be lost.

Believing that you will regret, and do your best to prevent, this unhappy consequence, allow me to subscribe myself,

Yours respectfully,
STUDENS.

April 30, 1838.

[The above letter is authenticated.—E. G.]

COLLEGE OF PHYSICIANS.

THE last of the evening meetings was held on Monday, the 7th instant; on which occasion a very interesting paper on *Bronchial Polypus*, from the pen of Mr. North, was read. We shall insert it at full length in a future number.

DEVELOPMENT OF THE GENERATIVE ORGANS.

M. Coste has made some observations on the development of the generative organs in the sheep and human species; which, if correct, add much accuracy to the knowledge hitherto possessed of the analogies of the different organs in the two sexes. The following is the substance of his description:—In the fœtal lamb, at a certain period, the external organs of generation consist of, 1st, a central asperity, or elevation, immediately below the anus, marked by a transparent line in its whole length; 2dly, a slight fold of skin, forming a corona round the base of the eminence; 3dly, two rounded eminences, placed one on each side of the fold of skin, and a little anterior to the base of the asperity. This median transparent line is not yet the orifice of a canal, but is only a groove leading to the common orifice of the bladder and uterus in the female, and of the bladder and vesiculæ seminales in the male, as in the tortoises. In the latter, its edges growing downwards, soon meet and unite, so as to form a complete canal, the urethra, which elongates as the body of the eminence grows, and forms the penis. In the female, the sides of the groove do not unite so soon, but continue to grow so as to surround the increasing common aperture of the genital and urinary organs; and they thus form, in women, the labia minora, and in ewes by uniting, the orifice of the vulva, while the eminence above and between them is developed into the clitoris. The coronal fold of skin is in each case developed similarly, but in different proportions; in the male forming the preputium penis, in the female the preputium clitoridis. The permanent openness of this groove in man is, of course, the cause of hypospadias.

The two rounded eminences by the side of the fold, which is afterwards developed into the prepuce, continue to grow, and as it is carried on with the penis, they approach each other, and at last unite below it, forming the scrotum in the male, which is, indeed, completed before the testicles have acquired their true sexual character. In the female they enlarge in the same manner, but here the eminence forming the clitoris, instead of advancing as the penis does, rather retreats into the vagina; so that it becomes more or less covered in front by the enlarged lateral eminences, which now form the labia majora, the analogues of the scrotum.—*Annales d'Anatomie et de Physiologie*, t. ii. N. 2. Mars 1838.

APOTHECARIES' HALL.

Thursday, May 3, 1838.

Robert Humphrey Cooke.—Charles Hallett, Axminster, Devon.—Francis John Gillard.—William Williams, Tetbury.—James Harrhy, Newport, Monmouthshire.—William Cowell Funge, Cambridge.—Thomas Evans, Cardiff, Glamorganshire.—Thomas Harrold Fenn, Neyland, Suffolk.—John Havers, Shelton Hall, Norfolk.—William Pyke, Wootton Rivers, Wilts.—Charles Lodge, Barking, Essex.—Richard Lewis Perkins, Newport, Monmouthshire.—Henry Greenup, Garston, Lancashire.—Charles Nind.—Henry Middleton, Loughborough.—Thomas Abbotts Warren.—George Dixon Hedley, Bedford.—Richard Chambers Roberts, Ruabon, Denbighshire.

WEEKLY ACCOUNT OF BURIALS.

From Bills of Mortality, *May* 8, 1838.

Abscess	1	Inflammation	15
Age and Debility	34	Bowels & Stomach	5
Apoplexy	1	Brain	5
Asthma	11	Lungs and Pleura	7
Cancer	1	Insanity	2
Childbirth	2	Measles	7
Consumption	44	Mortification	2
Convulsions	20	Paralysis	1
Croup	1	Small-pox	21
Dentition or Teething	1	Sore Throat and Quinsey	1
Dropsy	8	Stone & Gravel	1
Dropsy in the Brain	10	Thrush	1
Erysipelas	1	Tumor	1
Fever	18	Unknown Causes	56
Fever, Scarlet	3		
Fever, Typhus	6	Casualties	8
Heart, diseased	2		
Hooping Cough	8		

Increase of Burials, as compared with the preceding week . . . 52

METEOROLOGICAL JOURNAL.

May.	Thermometer.	Barometer.
Thursday . 3	from 45 to 63	29·75 to 29·91
Friday. . . 4	32 69	29·94 29·92
Saturday . 5	42 67	29·98 30·11
Sunday . . 6	39 63	30·17 30·19
Monday . . 7	32 74	30·19 30·22
Tuesday . 8	39 76	30 22 30·18
Wednesday 9	33 70	30.13 30·10

Winds N.E. and S.E.

Except the mornings of the 3rd and 5th, generally clear.

NOTICES.

Dr. R. (May 9). We had no intention whatever of making the omission to which he alludes; the change of designation was an oversight.

The communications of Mr. Alcock, Mr. Jeffreys, Dr. Macfarlane, Dr. Griffin, Dr. Reid, Mr. North, Mr. Smee, Dr. Fer-Fergusson, Mr. Calder, Mr. Watkins, "A Retired Physician," Tritus, Observer, M. D. Erlangæ, and K., shall appear as early as possible.

Erratum.—In Mr. George's paper, p. 253, for "as many bad men may prosecute a good one, so many good ones may persecute a bad one," read "as many bad men may *persecute* a good one, so many good ones may *pro*secute a bad one."

Wilson & Son, Printers, 57, Skinner-st., London.

(*Extra Limites.*)

IRISH COLLEGE OF SURGEONS.

DR. ALCOCK'S FINAL REPLY.

To the Editor of the Medical Gazette.

SIR,

I CANNOT but congratulate you, as well as myself, on the prospect that the controversy in which I have been engaged, seems, at length, about to draw to its close. I have read in your number of the 21st April, the manifesto of our Pharmacy Committee, in which, with equal good sense and good taste, they have interposed to rescue their champion from a contest which he had shewn himself unequal to maintain. I thank them for it. I little thought, when writing my last letter, that the confirmation of my statement, that "he had chosen the alternative of sheltering himself under the protection of his College party," would be furnished by themselves; and since they have thus admitted their own inability, as well as his, to redeem the pledge which was given voluntarily, I leave all the parties to congratulate themselves upon the honourable position which they have taken, and to enjoy their "otium" with its "dignity."

But now a word for the manifesto itself:—First, sir, it was an illegal act on the part of the Committee, for they received no instruction or authority for it; the matter was not referred to them by the College; on the contrary, it was made the subject of a special proceeding in the College itself; and thus the Committee have not only acted without authority, but they have presumed to take the matter out of the hands of the College. Secondly, it is an absurd act, for while the Committee profess to concur with Dr. Maunsell, they charge me only with "language offensive to the Committee," and "unfounded statements with respect to their proceedings;" whereas he charged me with having "calumniated" the College itself. Again, the word of the party arraigned is not sufficient proof that the "statements respecting their proceedings are unfounded;" and, lastly, it is their business, not mine, to prove that they are so—and I challenge them to the proof. Thirdly, it was an act as unmanly as it was absurd and illegal. The Committee had no authority from the College to do it, and yet they proceeded to do an *official* act, directed against an individual member of the body by which they were appointed, without having given him any notice of their intention. I knew not of it until I read the manifesto in the GAZETTE. Fourthly, it is an unseemly and uncandid act. "The President, W. H. Porter, Esq. was in the chair." Mr. Porter is president of the College, not of the Committee. He is a member of the Committee, not as president of the College, but only in his individual capacity. As president of the College, he is bound to maintain an independent, impartial, and unprejudiced character: as such he may be called on to preside, in the College, over the decision of the body upon that very act, which, as president, he countenanced in the Committee; nay, he may be required, by his casting voice, to determine what that decision shall be,—and who, in such case, can now give him credit for impartiality? I should be sorry to deal harshly with Mr. Porter, as I feel assured that nothing but an error of judgment could have betrayed

him into such a position, but he must not forget that he has thereby exposed himself to the suspicion of having made the authority of his station subservient to the purposes of a party. 2. The manifesto asserts that "I concealed my name." As you, Mr. Editor, will be admitted to be an impartial witness, I take the liberty to quote your words in reference to this matter. "On the present occasion, the principle only can be established, because it so happens that the gentleman whose name was sought, and which was always privately attached to his papers, has come forward *spontaneously*." I have *not* concealed my name." 3. The manifesto is subscribed "J. W. Cusack, Secretary." Secretary of what? Of the Pharmacy Committee, you will, doubtless, reply. No, sir, Mr. Cusack is not secretary of the Committee; Dr. C. H. Orpen is its secretary. Why then was not his name subscribed? Mr. Cusack is secretary of the *College*, and why did *he* subscribe himself "secretary" to the act of the Committee? I will not assign the motive under which an act so singular was perpetrated. I leave it to his own breast to answer the question; but the profession at large can now decide how much of sincerity there has been in the declarations made by that gentleman without the College, concerning the proceedings of the Pharmacy party within it.

But, sir, the manifesto is, to me, a source of pride, for it contains a practical acknowledgment of the irregularity of the past conduct of the Committee, and of the propriety of my own. In January, when they put forth a manifesto, it was an anonymous advertisement, headed "College of Surgeons," as if it were a College act. Now it has assumed the becoming form of an act of the Committee, signed, and having "College of Surgeons" in its appropriate place. But they have made a more important admission still. At first, they charged me in the College with having calumniated *the body*, and they sought, under that pretext, to put me down by means of College punishment. I set them at defiance, and now they have changed their position and their tone, and with more propriety have stated that I have "used language offensive to the Committee!" Doubtless, it cannot be pleasing to any body of men to have their conduct arraigned; but is not their new-born sensibility as dignified as it is entitled to sympathy? Where was it when they countenanced and circulated the offensive libel of their secretary? Where was it then? Let them answer. However, since the matter is now reduced to a question between me and the Committee, and since they have complained that "I have not adopted any course calculated to remove the impression made by my communications," there remains for me but the one, which of course I adopt, viz. to declare myself personally responsible for my statements, to such of its members as may feel themselves personally aggrieved.

Permit me now, sir, to turn to another subject. I have also seen, in late numbers of the GAZETTE, two letters from C. H. Orpen, Esq. M.D., Secretary to the Pharmacy Committee, and author of an injurious imputation, published in the Dublin Evening Mail, of 1st December, against myself and other professors in the School of Apothecaries' Hall. Sir, the canting calumniator, who almost in the same breath talks of his conscience, and slanders his neighbour, is not entitled to other notice than the law allows. I therefore hold myself exempt from the necessity of following that gentleman through the farrago of misunderstanding, misrepresentation, and absurdity, which he has inflicted upon you, and upon our body, for I believe that his effusions are published at its expense; but there remain some points connected with this controversy which I feel bound to clear up fully before I take leave of it; and, as the strongest proofs which I can adduce upon some of these points are furnished in the letters in question, you will permit me to take advantage of them without being deemed to have contradicted myself. First, then, have I been guilty of inconsistency in this matter? Do I now oppose what before I advocated? Dr. Orpen (I ought to

thank him for it) has furnished the answer—I have not. He has informed you that I was one of, I believe, the first Pharmaey Committee appointed by the College in 1832; that on the 17th February, 1834, the College finally deeided upon the Committee's report; and that on that occasion "some person was in a glorious minority of 1 to 29!" Sir, I was that person. I opposed the plan of dispensing and selling medicines then, and I oppose it now. Therefore, in 1838 I maintain the opinion which in 1834 I adopted after due deliberation. Secondly—Am I ehargeable, as he has charged me, with interested motives for my eonduct, and with a design to serve the views of the apothecaries. Doubtless I am no more exempt from interested feelings than others, but my aceuser has told you that I opposed this measure before I had any connexion with the School of the Hall; and, to crown the matter—I entreat your attention to his own words—"By the by I pity him, when they (the apothecaries) read this, his opinion, which will probably make them distrust or dismiss him." With the design, therefore, of promoting my own interests, and serving the views of the apothecaries, I advocate an opinion which is to make them distrust and dismiss me!—A novel plan that! worthy of the sapient Doctor!

I turn next to the confirmation of some of the statements made in my former letters, for which also the Doctor happily furnishes the means. I have said, "that the proposed change has not reeeived due consideration, or sufficient sanction from the College or the profession." My first, and in my mind a sufficient proof of this, is, that "on no occasion has one-third of the body ever voted upon the question." This the Doctor was determined to place beyond doubt, and accordingly he has given a detail of the numbers present at all the meetings at which the question has been ever entertained; and, with a single exception, the greatest number ever present, was only 39; while, on the other hand, they fall so low as 28. On the day on which the present Committee was appointed, there were 51 members present; but that meeting was held for two purposes:—1st. To ereate a new professorship of anatomy; 2d. To appoint the Pharmaey Committee: and it is reeorded in the College minutes, though the Doctor has omitted to mention it, that there were but 38 members in the room when the latter question eame on. Therefore, 39 are the greatest number which can be said ever to have voted at once upon the question, and that is not one-third of the entire body. Dr. Maunsell has stated, in his *pledge* letter, that "the report of the Committee was unanimously agreed to at one of the largest meetings of the College *known for years.*" Now, the number at that meeting was, according to Dr. Orpen, only 37; whereas the latter gentleman also admits, that at the five last January meetings there have been 55, 45, 58, 89, 79 members. Therefore, the number of members at the meeting whieh agreed to the report was not *one-half* of that whieh has been present within the last five years. But, says Dr. Orpen, "It is notorious that, except at the annual election (the January meeting), there is searcely ever so large a meeting as on the above days" (those on which the Pharmacy question was entertained). I have taken the trouble to consult the College minutes upon this point, and I find that at ordinary meetings during the months of November and Oetober last (previous to the appointment of the Committee), there were 53, 51, and 48 members; and that during the past month (April) were two ordinary meetings, at which were 49 and 55 members, while the meeting which adopted the report was only 37, and that which adopted the petition only 33. Doubtless there have been many smaller meetings, but it is not by reference to such that the question at issue can be decided. It is plain, therefore, that these gentlemen are altogether in error, and that, whether unintentionally or otherwise, the fact has been misrepresented.

I have also said, in my reply to Dr. Maunsell's *pledge* letter—"But, sir, the truth, according to his own state-

ment (for out of his own mouth I'll judge him) is, that the proposed change had not received any consideration at all." For what does he say? "Every one, save Dr. Orpen, must perceive that in this I proposed to argue from Dr. Maunsell's own words, and that the conclusion referred altogether to the present Pharmacy proceeding; and it is strictly true, for the proposed change had not received any consideration immediately prior to the appointment of the Committee for its accomplishment: on the contrary, more than three years and a half had at the time elapsed, since the College had adopted (in 1834) a resolution to permit the general practitioner system at all. Therefore the question was to have been deemed lost or abandoned, and ought to have been reconsidered and reaffirmed. But, sir, the proposed change not only had not been considered immediately prior to the appointment of the Committee for its accomplishment, but it is actually opposed to and forbidden by a resolution of the College adopted in 1834, which remains still unrepealed, and by which the College are consequently still bound; for by the change now proposed, all the members and licentiates of the College, whether in Dublin or the country, are to become general practitioners, and to sell medicine if they please; whereas a resolution of 1834 forbids members of the College, present or future, from selling medicine in Dublin, or within ten miles thereof; and this resolution has never been repealed, nor a contrary one adopted.

Next for the report of the licentiates. Dr. Orpen asserts that it does not contain a protest against "the plan of dispensing and selling medicines," as I have asserted it does. I will again submit the extract, which I before adduced, and let your readers judge: — "They must, at the same time, express their deep regret that, to the discredit of the medical profession, instances are not wanting where a licensed practitioner has combined the profession of the physician with the business of the apothecary—a union which your Committee *strongly deprecate*, as one disreputable to our profession as at present constituted—unjust to the apothecary—and highly dangerous to the interests of society." This, according to Dr. Orpen, is not a protest, because, he says, the Apothecary "Act of 1791 defines the apothecary as a person to make up the prescriptions of physicians," or "of others" not apothecaries. Such may be his opinion, but there is no such definition in the Act, which says, (sect. 1), "That there shall be one company of judicious apothecaries, well skilled in *preparing and compounding of medicines.*" The preparation and compounding of medicines, therefore, are the business of the apothecary, according to the Act, and such, together with the sale of medicines, is the business which the proposed measure would combine with that of the physician and surgeon. It is not less the business of the apothecary, because the practitioner may be only apothecary to his own patients. Again, it is not a protest in the opinion of Dr. Orpen, because the words "who does not interfere with practice" are in the copy of the report which the Pharmacy Committee have, and are not in my extract. To this discrepancy I will refer presently again, but how do these words make it less a protest? He admits that without these words it may be one; how is it less so with them? Is it not a protest against the union on the three following grounds: — 1. That it would be disreputable to their profession, as at present constituted. 2. That it would be unjust to the pure apothecary (the apothecary who does not interfere with practice;) and, 3. That it would be highly dangerous to the interests of society. If a more explicit or stronger protest can be produced I give up the point; the sole difference between the two former being, that in one the injustice is restricted to the pure apothecary, while in the other it applies to all apothecaries; but the injustice is equally maintained. Now for the discrepancy. I took my extract from the original copy of the report, as adopted by the general meeting of the licentiates, and which does not contain the words. The latter

fact was admitted by the Secretary of the Licentiates' Committee to the Vice President, and by him communicated to the College. I make this statement merely for explanation sake; for the words are quite immaterial so far as the force of the protest is concerned; and I have the authority of Dr. J. H. Corbet, member of the Licentiates' Committee, who, in the exercise of an unquestionable right, permitted me to take the extract, for saying that the words were introduced into the copy presented to the Pharmacy Committee only to render the meaning of the licentiates more explicit, and no more.

I have asked, in my letter of the 10th February, "Why have the Committee not even alluded to it (the protest of the licentiates) in their report? Dr. Orpen's explanation of this fact is, that the report of the Pharmacy Committee was agreed to on the 5th January, and that that of the licentiates, which was not presented to the Committee until the 8th, therefore came late for insertion; but that he himself stated orally the substance of the licentiates' report, when presenting that of the Pharmacy Committee on the next day to the College. If the foundation upon which this explanation rests be untrue, of course the entire is unworthy of belief. Now I have consulted the College minutes since reading the Doctor's letter, and it is there recorded that the report of the Committee was agreed to on the 8th Jan.; and that my individual assertion may not be questioned, I subjoin the following testimony from another member of the College:—

"My dear Alcock,—In compliance with your desire, I have examined the minute-book of the College, and ascertained therefrom that the Pharmacy Committee agreed to their report on the 8th Jan.

"Yours very truly,
"ANDREW ELLIS."

Therefore, the statement that the report was agreed to on the 5th is untrue, and the whole explanation is consequently unworthy of belief. I have only to add that, though I was present when the report of the Pharmacy Committee was presented to the College, and though I paid the utmost attention to what was said by Dr. Orpen, I heard not a word of the substance of the licentiates' report, or of the objections made by them to the union of the practice of pharmacy and medicine. How could the man state what he says himself he did not know that the report contained? Nor did I discover them for some time afterwards, when the licentiates' report came incidentally into my hands, and I informed myself of its contents. There are some other points to which I wish to refer before I conclude, for which, therefore, I beg your further indulgence.

I have asserted in a former letter that the school of the College of Surgeons is not a chartered school, and I repeat it. A school is not a chartered school which has not a charter, and which was originally established without any authority of charter; and such is the case with the school of the College. It was originally established while the first charter of the College was in force, and that charter contained no mention whatever of a school. About ten years' since (the school having been in existence nearly half a century) a new charter was obtained for the College, into which there was introduced an authority (not contained in the original one), to "create a fund sufficient for keeping the several buildings and schools of said College in proper repair, enlarging them when required, and supplying the library and museum thereof, &c., and defraying all other expenses which the said College may incur." This, which with its ratification is the only notice of a school contained in the present charter, is a mere authority to the body to create such fund, and apply it so and so as it may think fit; there is no obligation to do so, nor does the charter any where ordain that the College shall have a school, nor even empower it to create a single professorship; while, on the other hand, it empowers it "to make or annul, from time to time, such by-laws as to them shall seem requisite for the government and advantage

of said body, and the *application* and administration of the funds and property thereof," &c. So that the College may, whenever it thinks fit, hold its hand, and refuse to apply its funds any longer to the maintenanee of a school. And what, I beg to ask, beeomes of its charter then?

Now, sir, being a member of the College, I feel bound to state my reason for pursuing this course with regard to our School, that my conduct may not be misinterpreted or misrepresented. I disapprove altogether of the College School, as at present constituted, and I regard it an incubus upon the institution, against which the only hope of delivery which remains to it, is in the fact that the School is not chartered, and that it is completely in the power of the College to do with it as it may think fit—to continue, to remodel, or to suppress altogether; and I am of opinion that if the body at large be so unwise as to permit what, I believe, is at present in contemplation, viz. to obtain, under the name of a revision of the charter, a guarantee for the independence and permanence of the School, that it will not matter much how soon the doors of the College shall be closed, for ultimately they will be; and the sooner in such ease the better. I cannot at present enter upon a full discussion of this very important question; but I will state one all-sufficient and insuperable objection to the present constitution of the school. Its professors are allowed to be legislating members of the body; nay, they are permitted to be members of the very Court of Electors to which they are *themselves* responsible, and by which they are *themselves* "elected, on the first Monday in June in every year." And to such length has this (in my opinion) mischievous practice proceeded, that, at this moment, two of the six members of the Court of Eleetors are professors in the School; the president of the Court, and of the College, is a professor of the School, and the assistant-secretary of the College, to whom, for the greater part, are committed the conduct of the correspondence of the College, and the regulation of its business, is a Professor in the School. The School and its professors are every thing—they are the College! and he who has the boldness to "enter the lists" with them must do so at his peril.

The question has been asked, What possible selfish interest could the advocates of the change have in the matter? For the gentleman by whom the question has been asked, I have only to say, that it is now about three years sinee he was candidate for a professorship in the School of the College; *i. e.* when it was last vacant; and it is just *possible* that he may contemplate being so again. For the rest, I think it neeessary only to ask and answer two questions. The answers shall be your premises, and you may draw the conclusion yourself.

First, Who are the leaders in this movement? Doubtless, young men neeessitous and impatient for practice. No, sir; the young men of Dublin, at least, have deelared "that it will be repugnant to their feelings to be obliged to bend" to this course. Who then? Certain professors in the School of the College of Surgeons, who have been seized with a veritable mania "for the good of the profession, and the safety of the public, and against their own pecuniary interest" ever sinee the apothecaries have had the unpardonable hardihood to establish a School, where anatomy, surgery, pharmacy, and midwifery, are to be taught.

Secondly, What result must flow, of necessity, from the change, if accomplished? By it, the College of Surgeons would become not only the licenser of the legalized general practitioner for Ireland, but also the arbiter of the claims of schools and teachers to be recognised as fit and competent to educate such practitioners; in consequence of which authority, all those teachers whom the College at present refuse, at the instigation of the school party, to recognize, viz. the professors of the School of Physic, and those of the school of the Apothecaries' Hall, would be then exeluded from participation in the education, not only of the surgeons, but also of the general practitioners of Ireland; and the

immediate effect must be, the closure and extinction of the Hall school.

Perhaps, sir, from these data you can discover a *possible* selfish interest for the advocates of the change.

Permit me now, sir, in closing this controversy, to which I feel that I am not called on to return, unless in reply to an official communication, to convey to you my thanks for the consideration and the impartiality with which you have treated me, and to express a hope, that while I have felt bound to substantiate my statements, I have in your judgment failed neither in that degree of moderation to which my adversaries are entitled to lay claim, nor in that respect for truth and independence which I desire to be awarded to myself.

I have the honour to remain,
Your obliged and
Obedient servant,
B. ALCOCK.

Dublin, May 7, 1838.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 19, 1838.

LECTURES ON BLOOD-LETTING,

Delivered from time to time,
At the General Dispensary, Aldersgate Street,

By Henry Clutterbuck, M.D.

Formerly one of the Physicians to that Institution.

Lecture V.

On the different modes of Blood-letting.

Different methods are in use for taking away blood from the system, for therapeutic purposes; as, 1st, by phlebotomy, or venesection, where the blood is drawn from one or more of the larger external veins that are conveniently situated for the purpose; 2dly, by arteriotomy, or the puncture of an artery, which is sometimes resorted to; 3dly, by scarification of the superficial vessels, with or without the aid of the syringe, or cupping-glass; 4thly, by leeches, which are likewise in frequent use. These different modes of abstracting blood from the system have their respective advantages and disadvantages, which require discussion, the one mode being often applicable, where the others are less appropriate.

Now if it were simply by diminishing the quantity of blood in the system altogether, (as in the supposed case of plethora), or even by reducing the general strength of the body, that blood-letting effects its purpose, it would be a matter of little moment by which of the ordinary modes in use the blood were drawn; for, in either way, the object sought for might be obtained; quantity alone being the thing required. But if blood-letting act, as I have endeavoured to shew, by a kind of counter-impression, (not very unlike what is called counter-irritation,) the diseased part being influenced only in common with the rest of the system, or through the medium of the constitution, as it is termed, then something more than mere quantity is to be looked to: the blood should be so drawn as to make that impression upon the general system, from which the remedial effect, in most cases, appears to proceed.

In regard to this point, I wish to observe, that it is by no means always necessary, nor even desirable, that the evacuation of blood should be carried so far as to induce actual syncope or fainting, or even an approach towards it. The system may be sufficiently impressed, on all ordinary occasions, without going to such a length; as we learn from the result. There are in fact weighty objections to the practice of purposely inducing faintness, unless under peculiar circumstances. The occurrence of syncope is not only attended with distressing feelings to the patient himself, but is also apt to excite alarm in the bystanders; thereby making it the more difficult to recur repeatedly to the operation, as is often requisite. In the next place, fainting puts a sudden stop to the flow of blood, sometimes before a sufficient quantity is obtained to produce a permanent effect; for this depends much more upon the absolute quantity lost, than upon the rapidity with which it is taken. It is only in cases of great emergency, therefore, as where it is important to arrest the progress of the disease as quickly as possible, or to relieve some excruciating pain, that syncope should be purposely induced.

The different modes of drawing blood above enumerated, have been commonly arranged under the heads of General and Local. The former, or general blood-letting, consists in taking blood in any manner, and from any convenient place, without regard to the part immediately diseased; the remedial effect, in this case, taking place either by sympathy, or through the medium of the general system, as before stated. In the latter case, local

or topical bleeding, the blood is taken, or presumed to be taken, immediately from the vessels of the diseased part, or those in its immediate vicinity. To the former head belongs phlebotomy, or venesection, and perhaps arteriotomy, though this, as ordinarily practised, has been looked upon more in the light of a local remedy; while scarification, with or without cupping, and leeches, are especially included under the head of local or topical bleeding.

It is natural enough to imagine that the taking the blood immediately from the part affected, must be more efficacious than when it is drawn remotely, as by venesection. But experience proves that in most cases diseases are more effectually influenced by general than by local remedies of any kind; that is, by acting upon the sound and healthy parts of the system, rather than by immediate applications to the diseased part itself. And it is fortunate that such is the case: for the most important and dangerous diseases are out of the reach of direct or local remedies, as with respect to inflammations of the different viscera. If these affections were only curable by means applied immediately to the affected part, we should be left on numerous occasions almost without a remedy. Instead of this, however, more advantage is found to be derived from the employment of venesection, purging, blistering, and sweating, (all of which are indirect or general remedies), than by any of a local or topical nature.

In strictness of language, local or topical bleeding means the taking away blood, either from the vessels of the part affected, or from those in immediate connexion with it. But this can be done in very few instances. We may puncture or scarify the parts within the mouth and nostrils, the tunica conjunctiva of the eye, the extremity of the rectum, and a few others; but what is often termed topical bleeding, (as distinguished from general), has no title to be so considered; there being in most instances no direct communication of vessels, (none, at least, worth mentioning,) between the diseased part and that from which the blood is taken: as in the instances of cupping behind the ears, or at the back of the neck, or on the temples, in diseases of the brain; or on the skin of the chest, or abdomen, in diseases situated within those cavities.

But experience, we are told, proves that greater benefit is often derived from local than from general bleeding, although the quantity drawn be actually less; and with the further advantage of the general strength being less impaired by the former than the latter practice—a point, doubtless, of some importance where the patient is weak, and consequently unable to bear any considerable loss of blood. In such cases, therefore, topical blood-letting, in one or other of the modes mentioned, is often resorted to without hesitation, while venesection is rejected as injurious. Now if experience really prove this to be the case, you ought, unquestionably, to be guided by it. But it is difficult to say what really merits the name of experience; than which, as generally relied upon, nothing is more fallacious. There are so many obstacles to the arriving at a just and satisfactory conclusion on such subjects; so many sources of error in deciding upon medical facts, or what are so termed, that there is little ground for confidence in any case. It may be considered, I think, as sufficiently proved by experience, that, generally speaking, the system altogether is more quickly and sensibly impressed by venesection, than when the blood is taken by the slower modes of scarification, or leeching; and, which is of greater consequence, that the immediate effect on the disease is in the same proportion, although the quantity of blood drawn in the two cases be equal. In other words, the immediate, and, for the most part, the remedial effect, will be the greater, the more rapidly the blood is discharged. The permanent effect, however, is rather according to quantity. This, therefore, must be attended to, as well as the rapidity of drawing.

Topical bleeding, although far inferior to venesection, in the treatment of acute diseases in general, has nevertheless advantages that are peculiar to it; and which, on some occasions, entitle it to a preference. Scarification, with cupping, produces effects beyond that of merely discharging a certain quantity of blood from the system. It determines a flow of blood to the part and its neighbourhood; as is apparent from the redness and tumefaction occasioned by it. These effects continue for some time afterwards, and are followed by more or less of inflammation in the part. Changes of this kind, acting upon the principle of counter-irritation, may influence the disease as much as, or even more, than the actual loss of blood. If, then, more relief be really derived at times from topical, than from general blood-letting, (and which I am not disposed to question), the superiority must be referred to a different principle, namely, that of revulsion, as it was formerly termed; or, as now better understood, the principle of counter-irritation—a matter quite independent of the mere loss of blood, which has been the point chiefly looked to.

Considering blood-letting, then, as acting, for the most part, upon the principle of counter-impression, as before explained—

and that we sometimes wish to produce an immediate, but temporary—sometimes, a more durable, or permanent effect, (the former depending rather upon the rapidity of drawing, than upon mere quantity,) let us next inquire how far the different modes in use for abstracting blood from the system are adapted to these different ends.

1. *Venesection*, or *phlebotomy*. — The various purposes of blood-letting, as a medicinal agent, are, upon the whole, much more effectually answered by venesection, than by any of the other modes of drawing blood. If, for instance, the object be to take away blood so rapidly as to make a great and sudden impression on the system, with the view of producing actual syncope or fainting, for any particular purpose, venesection is the most sure and effectual means we possess of so doing. If, on the other hand, we be desirous of obtaining a large quantity of blood, in order to produce a more lasting effect, without inducing syncope,—in this, as well as in the former case, venesection is still the preferable mode; because it is almost always in our power to regulate the velocity with which the blood flows from a vein; at the same time that we are pretty sure of obtaining the desired quantity. By dexterously opening one or more of the larger external veins, either in the upper or lower extremities, or in the neck, (the external jugulars, which are both large in size, and of ready access), it is in general an easy matter to obtain blood, both in quantity, and with all the rapidity, that can be required. In a very few instances, however, it happens, that, from the smallness of the veins, or their being deeply-seated, or from a dread on the part of the patient it is difficult, if not impracticable, to obtain blood in this way. In such cases, recourse must, of necessity, be had to some of the other modes in use for the purpose.

Much importance was formerly attached to the part from which the blood was to be drawn, in particular cases of disease. Bleeding in the foot, for example, was supposed to be better adapted to diseases of the head and neck, than the taking blood from the arm; while the latter was deemed more effectual for the relief of the thorax and abdomen. Drawing blood from the lower extremities, again, was thought to increase the action of the uterine vessels in a particular manner, and thereby to favour menstruation; and, on the same ground, it was thought, that in cases of uterine hæmorrhage, bleeding in the foot rather tended to increase the malady, while bleeding in the arm would have the opposite effect.

I mentioned in a preceding lecture that it was formerly made a subject of fierce and lasting contention among the faculty, whether, in cases of pleurisy, the blood should be taken from the arm of the side affected, or from the opposite side. Sydenham himself was imbued with this prejudice, for he insists on the superiority of taking the blood from the affected side. It is not difficult to trace the origin of these disputes. The discovery of the circulation had at first the effect of making the human body be viewed in the light of an hydraulic rather than an animated machine; and it was argued accordingly, that whereas if one of two tubes, derived from the same common trunk, be opened, the momentum and velocity of the fluid passing through the corresponding branch is diminished, so the opening a vein in the affected side in pleurisy, would tend to diminish the force of circulation on that side altogether. The error here consists in supposing the blood-vessels to be mere passive tubes of conveyance, instead of being endowed, as they doubtless are, with an active, contractile, or, as it might be justly termed, a muscular power; by which, they contribute largely towards the movement of the blood.

These notions are now, I believe, pretty generally disregarded among British practitioners, as being inconsistent with sound physiology and pathology, while they are certainly not supported by any well-founded experience. They still appear, however, to be maintained by some of our continental neighbours; and, as usual, experience is appealed to in their behalf. The following may be taken as a specimen of what is deemed experience in such matters, as furnished by M. Freyteau, in an article in the *Dictionnaire de Médecine*, recently published in Paris:—

"A young female, eighteen years of age, subject to frequent and violent attacks of cerebral excitement, was seized with sudden blindness. Two bleedings in the foot having produced no good effect, she was bled in the right arm; when, immediately, the sight of the right eye was restored. She was then bled in the left arm, with the same good effect on the left eye. On a return of the blindness, the same plan was pursued, and with the same good result." From this it was inferred by M. Freyteau, that in various disorders of the head and neck; as hemiplegia, inflammation in the internal ear, ophthalmia, &c. the blood ought to be taken from the side affected. Now, admitting that the facts here are fully and correctly stated, what do they prove, but that, where a first, or even a second, bleeding has failed to cure, a third may succeed? The effect in the case described is with much more reason attributable to the quantity of blood lost, than

to the part from which it was taken. It is the last bleeding always that cures.

Bleeding from the jugular vein has been said to induce fainting, sooner than bleeding from the arm; and this has been accounted for by the existence of anastomosing branches, between the external and internal jugulars; which latter, you know, derive their blood direct from the brain. If the fact be really so, it would give a preference to the taking blood from the external jugular vein, for the relief of brain affections, rather than from the arm, or other part. It has also been said, that bleeding in the foot favours the production of syncope. This, if true, may, perhaps, be ascribed, (in part at least), to the combined use of the pediluvium, in most cases of the sort, by which a greater determination of blood is made towards the inferior parts of the body.

The old practice of taking blood from a particular vein, as from the cephalic vein of the arm in affections of the head—from the right basilic, in those of the liver—and from the left vein of the same denomination, in those of the spleen, was evidently founded in an imperfect knowledge of the circulation.

2d. *Arteriotomy.*—There are but few places where an artery can be safely opened, or, at least, where the opening would not be followed by more or less of inconvenience. The temples, indeed, are almost the only part where arteriotomy is now practised; and here the branches of the artery are in general so small in size, as to make it by no means an easy matter, at all times, to obtain blood in quantity sufficient for our purpose; and still less in the way that is required to make a sudden and general impression on the system; while, in order to restrain the bleeding after the operation, it is often necessary to have recourse to so much compression of the external vessels of the head, as is likely to disturb in some degree the circulation of the brain. It is at the same time clear, that the taking blood from a diminutive branch of the external carotid artery, can have little, if any, direct influence over the circulation of the encephalon, the supply of blood to which is derived from such different sources. On these grounds, therefore, arteriotomy seems to have no claim to the confidence that has sometimes been placed in it, as if it possessed advantages that cannot be obtained from venesection, or other modes of drawing blood.

A degree of importance has, by some, been attached to arteriotomy on another ground; namely, that the vitality of the system is more affected by the loss of arterial than of venous blood; the former being of a more stimulating nature than the latter. But this is probably not a matter of much moment. From whatever part the blood be taken, it will come to the same thing at last; when, for instance, a few ounces have run off from a vein, it is evident that what follows must be, as it were, arterial blood.

3. *Scarification*, with the aid of the syringe, or cupping-glasses, ranks next to venesection, in point of efficiency, though still greatly inferior to it as a general remedy; because it is seldom that the blood can be so quickly obtained by this mode of drawing, as to produce a sudden effect, for the purpose of counter-impression, in urgent cases of disease, though a sufficient quantity of blood may often thus be procured for ulterior and more durable objects. On some occasions, however, the general system is quickly and powerfully impressed by this mode of taking blood, and that even to the extent of producing syncope; and, of course, with all the advantages derivable from venesection. This is sometimes the case in weakly adults; and still more frequently in infants, in whom veneseetion is not always practicable.

Simple scarification is sometimes practised with advantage, as a topical remedy, on parts where the vessels run superficially, and are easy of access. As, for example, on the tonsils, and other parts within the mouth; the inner nostrils; the tunica conjunctiva of the eyes and eyelids; and at the anus for the relief of hæmorrhoidal affections. It is always a question, however, in these cases, whether more good or harm is done by the operation, on account of the local irritation produced by it. Upon this point, practitioners are found to differ; and experience, which is usually appealed to in these cases, as in many others, is not altogether so satisfactory and conclusive, as the advocates of different opinions are apt to imagine.

It is not altogether foreign to our purpose, to remark here, that cupping without scarification, or dry cupping, as it is termed, is an agent of no small importance, and applicable in many instances where loss of blood is objectionable. It acts powerfully as a counter-irritant; as is proved by the pain attending the operation, and by the increase of action it excites in the vessels of the part, and to some distance around. To this is attributable, probably, much of the advantage derived from the ordinary use of cupping with scarification, which may be said to possess at once the advantages of counter-irritation and loss of blood. Admitting this to be a satisfactory explanation of the matter, the end, perhaps, might be still better answered in such cases by blood-letting in the ordinary way, with

the addition of blistering, or other mode of counter-irritation, in the vicinity of the diseased part.

Dry-cupping alone is a useful means of producing counter-irritation on a variety of occasions, and is less resorted to than it deserves to be. It is, indeed, a very ancient practice, and is recommended both by Hippocrates and Galén, as well as by many modern writers. Vanswieten says he cured a violent inflammation of the eyes by this remedy, when all others had failed.

4. *Leeches* furnish another mode of drawing blood that has its advantages, as being applicable on some occasions, where other means cannot be conveniently resorted to. Generally speaking, however, as in regard to the other modes of local or topical bleeding, the blood drawn by leeches is discharged too slowly to have much effect on the general system, in the way of counter-impression. Infants, however, are sometimes quickly and powerfully affected, even to fainting, by the application of two or three leeches, which, so far, in them, may answer the purpose of general blood-letting, producing all the effect of venesection in the adult. There is one objection, however, to the use of leeches in children, which deserves attention; namely, the terror they sometimes occasion, with a continuance of angry feelings for an hour or two, while the operation lasts. This is a cause of aggravation in many brain affections of children, where the sensibility and irritability of the system are already greatly in excess. In such cases, therefore, scarification with cupping is often preferable to leeches; and, still more, venesection, if it be practicable, as it often is, either in the arm or neck. By this, not only is the irritation here spoken of avoided, but the disease itself more effectually relieved.

In adults of ordinary strength, leeches, in whatever number applied, are rarely effectual, as a substitute for general blood-letting, in cases of active inflammation, especially if attended by febrile symptoms. By the application of a sufficient number of leeches, a large quantity of blood may, no doubt, be drained from the body, and a corresponding degree of weakness induced; but still often without making that general impression on the system which is necessary to interrupt the progress of the disease. I may mention the following as an example:—

A gentleman, whom I attended not long ago, when suffering under active inflammation in the abdomen, had had 250 leeches applied, in relays of fifty or sixty at a time, and all within the space of three or four days. On a moderate calculation he could not have lost less, in this way, than five or six pounds of blood. The face had become pale in the extreme, and there was a great reduction of the general strength; still no impression was made upon the disease. But upon taking away eight ounces of blood rapidly from the arm, by venesection, slight faintness ensued; the disease immediately began to decline, and quickly subsided altogether.

Of the Circumstances usually deemed favourable, or otherwise, to the Employment of Blood-letting.

It is absolutely necessary that you should become acquainted with the indications and contra-indications (as they are technically termed), which govern the use of blood-letting; for without attention to these, the remedy can neither be properly nor safely administered. Suppose, for instance, a person to be seized with apoplexy,—you are not immediately, and without consideration, to bleed him largely; because there are cases of this disease in which such a remedy is not at all requisite, and where, in fact, it would do harm rather than good. It always depends upon the circumstances of the individual case before you, and not upon the name merely, whether the remedy be proper or otherwise.

Among the circumstances especially relied upon, as furnishing a guide to blood-letting in general, strength and weakness may be mentioned. Now with respect to these I may observe, that general strength of system is so far favourable to its employment, that where the disease is of such a nature as seemingly to call for this remedy, we feel little hesitation in resorting to it; and, on the other hand, general weakness of system is always, of itself, a discouraging circumstance. But although both strength and weakness serve at all times to regulate and modify more or less the use of blood-letting, they are neither of them wholly and exclusively to be relied upon. It is not every strong subject that requires this evacuation, even although the disease be what is termed inflammatory; for the nature of the disease may be such that, under ordinary circumstances, it may be safely left to itself, as where it is seated in parts of secondary importance, and where its known tendency is to subside spontaneously after running a certain course. This is the case very generally with inflammation of the mucous membrane, as in catarrh, diarrhœa, and others of the same description. On the other hand, it is by no means always necessary, or even safe, to abstain from blood letting merely because the patient is weak; for the same forms of disease occur in the weak and in the strong, and the same general principles of cure are appli-

cable in both, though, of course, with proper qualifications. In illustration of this I may mention the following case:—

A weak and delicate girl, 17 years of age, had been affected for some weeks with a severe dry cough, with great heat and soreness over the whole chest. The face was pale and bloated; the flesh soft and flabby; the tongue white, but moist; the pulse frequent, and rather weak than strong; the breathing not laborious, but short and quick, in correspondence with the pulse; and the lower extremities were generally cold, and disposed to swell.

The disease here, you perceive, was catarrhal, *i. e.* the mucous membrane that lines the air-passages was inflamed. This variety of inflammation, though not, generally speaking, dangerous in itself, is very apt, when neglected, in young persons more especially, to spread to the substance of the lungs, so as gradually to undermine their structure, and thereby lay the foundation of future phthisis. In the present case the feeble and chlorotic state of the patient seemed to call rather for the use of tonics than of debilitating remedies of any kind; and it was not, in fact, till the employment of chalybeates, and others of the like description, had produced a decided aggravation of the cough and febrile symptoms, that I ventured to prescribe bleeding from the arm, though only to the extent of four ounces, and in the recumbent posture. The blood drawn exhibited an enormously large proportion of serum; yet the coagulum was firm in consistence, and both buffed and cupped on the surface. The result was as favourable as could be wished. The heat and soreness of the chest disappeared, expectoration took place, and the cough gradually ceased. The same tonic remedies that had before proved injurious were now recurred to, and with the best effect, and the health was quickly restored.

The *pulse* at the wrist has been more appealed to, perhaps, than any other sign, as indicating the propriety of blood-letting, or the contrary. Yet there is hardly any that is more equivocal; or about which, more erroneous notions seem to prevail. The condition of the pulse is influenced by so many circumstances, some of them of a very trivial nature, that little reliance is to be placed upon it, without the concurrence of other signs. In the first place, I may remark that the pulse at the wrist by no means always represents the state of the general circulation; and still less is it to be relied upon as indicative of the general strength of the system. You will often find, for instance, the heart acting violently, and driving the blood with great force into the large arterial trunks immediately connected with it, without at all affecting the pulse at the wrist, which may be, at the very time, small and feeble. The same state of pulse, with cold extremities, often occurs where the arteries of the head, both internal and external, are throbbing violently, with a great increase of heat in the whole head.

A full and strong pulse is generally considered as a justification of bleeding, in cases apparently calling for this evacuation: yet this, of itself, is a fallacious sign. Such a state of pulse may be temporarily produced, at almost any time; as by immersion for a few minutes in the hot bath, or even by the pediluvium, if continued for a longer time; by violent exercise; or by the use of various internal stimulants, especially if combined with opium: and this will take place under circumstances that neither require nor admit of the use of blood-letting. In apoplexy, also, the pulse often retains its strength and fulness, long after all hope of recovery has vanished, and when bleeding would only tend to accelerate the fatal event. In the hot fit of an ordinary intermittent, likewise, and in various instances of what has been called Ephemera, or simple inflammatory fever (synocha,) blood-letting, though well enough adapted to the nature of the complaint, may often be dispensed with, as not being essential, though it is sometimes called for by the urgency of the symptoms. In the small-pox, again, even when confluent, the pulse, in the early stage, is generally strong and full; yet, as the disease is specific, and has a peculiar and determined course to run, which art has little or no power to prevent (though it may impede and disturb,) it is questionable whether injury, rather than benefit, would not be the result of bleeding, unless used with much reserve, and as a palliative merely, at the outset of the disease.

The pulse sometimes gives an idea of strength that is fallacious, and apt to mislead us in practice. I allude to that apparent fulness and strength of pulse, which are observed in old people of emaciated habits; in whom the artery passes near to the surface, and is at the same time indurated by age. This you are always to make allowance for. On the other hand, a pulse that is deficient in fulness, and even in strength, is not always prohibitory of bleeding. Exposure to a cold atmosphere, and, still more, the cold bath, will produce such a degree of constriction of the external vessels, as will render the pulse at the wrist, for the time, so small as to be nearly imperceptible. Terror has a somewhat similar effect. In the erect posture, also, the pulse will often be small and feeble; where, on lying down, it immediately acquires both fulness and

strength, especially if seconded by the warmth of a bed.

The pulse is found to vary considerably at different periods of the day, from causes that are not always perceptible. Thus it is sometimes small and feeble in the morning, while in the evening it may have acquired both strength and fulness. Such alterations of the pulse take place without any material change in the general strength of the system, or in the state of the disease itself; they afford, therefore, no conclusive reason either for or against bleeding. In such cases, a more general view of the subject must be taken.

If, for instance, the disease present be evidently such as generally requires bleeding for its relief—if it be recent, and taking place in a habit of known vigour, and where no considerable loss of blood or other evacuation has already occurred, so as to have reduced materially the general strength—the mere feebleness of the pulse affords no decisive objection to blood-letting, as being rather a sign of irregular distribution of the blood, than of real debility.

Again, at the commencement of many fevers and inflammations, the pulse is observed to be small and weak, even in habits of known strength previously. And this is more especially the case if the patient be in an erect posture, and placed in a cool atmosphere. Such a state of the pulse is apt to convey to the mind of the practitioner an idea of extreme debility, which the feelings of the patient appear to confirm. This, however, often disappears after bleeding, and the pulse rises, as it is termed. The same thing frequently occurs after the infliction of severe injuries of any kind on the body; and also from great and sudden emotions of mind. This state has been called collapse, (though without any distinct meaning affixed to the term), and has been regarded as altogether adverse to bleeding, the state of re-action being waited for, in order to justify the employment of the lancet; danger, even, being apprehended from the use of it under these circumstances. Such an apprehension, however, is for the most part groundless. The state of collapse here mentioned is not a state of absolute weakness, or an actual loss of vital power, but a temporary depression or prostration of strength, or rather of action, the result of a general disturbance of functions, produced by the disease or injury, and which generally subsides of itself after a time, or as soon as the disturbance is quelled by any quick-acting stimulus, such as ammonia, or a glass of brandy. That the apprehension entertained with regard to the danger of this collapse is for the most part groundless, is further shewn by the safety with which blood-letting may be had recourse to in the cold fit of an intermittent; as I well know by my own experience, and as has been sufficiently proved by others. The object of blood-letting, in such cases, is not that of calming the general disorder, but rather to prevent future ill consequences, especially inflammation, which is almost sure to follow, in greater or less degree. The reaction, as it is called, that succeeds such injuries, is generally nothing more than the febrile state produced by the inflammation, which early blood-letting is calculated to mitigate, if not wholly to prevent the occurrence of. By delaying the bleeding, therefore, till the inflammation is fully established (as is often the case where re-action is waited for), it becomes more difficult to arrest the progress of this afterwards. The extent to which it may be proper to carry the blood-letting in such cases, is, no doubt, of importance, and must, of course, be governed by the actual strength of the system. But this is to be estimated by other signs than the pulse; especially by the known state of the patient, immediately before the receipt of the injury.

The pulse is sometimes obscure, a state that may mislead us, by suggesting the idea of weakness where, in reality, it does not exist. This state of the pulse occurs where the artery is deeply seated, being thickly covered with adipose substance, so as to be difficultly felt, or distinguished as to its properties, either in regard to fulness or the strength with which it is beating. In order not to be deceived in this case, it is necessary to press strongly with the points of the fingers upon the artery, and to examine it with close attention for some time; by which its real condition, in regard to strength and fulness, may generally be ascertained.

Frequency of pulse, when considerable, has often been supposed to indicate the propriety of blood-letting; and preternatural slowness, the contrary. But neither of these is of much weight, without attention to other circumstances. Unusual frequency or slowness of the pulse, are often constitutional, and unconnected with disease of any kind; and the same may be said of irregularity of pulse, which is often met with in persons enjoying the best health. Sometimes, too, this irregularity disappears under disease, and recurs again as health returns. Nor must it be forgotten, that the size and distribution of the arteries at the wrist are subject to great variety, in different individuals, so as to prevent our drawing any indication at all from the pulse in such cases. I find a striking instance of this recorded in my note-book, where the patient had been labouring for some days under great diffi-

culty of breathing, with severe pain in the chest, and a flushed face. No pulse was to be felt in either wrist; and this circumstance had naturally enough deterred the practitioner in attendance from having recourse to the lancet. This apparent inconsistency of the pulse with the other signs, all of which indicated general vigour of system (for the patient was in the prime of life, and, a few days before, was in high health), led us to suspect that there might be such an irregularity in the distribution of the arteries of the forearm as is alluded to above. This, upon examination, we found to be the case, and all doubt in regard to the treatment was at an end.

There is, nevertheless, a state of pulse that is always extremely unfavourable to blood-letting, if not altogether prohibitory of it: I mean, where it is at once small, soft, and compressible by the slightest force. But this is sure to be accompanied by other unequivocal signs of extreme weakness, that cannot well be mistaken.

Upon the whole, I may observe, that there is hardly any condition of pulse, either in regard to strength or weakness, fulness or smallness, hardness or softness, frequency or slowness, regularity or irregularity, which, taken singly and in all cases, either absolutely calls for, or prohibits, blood-letting. Much attention, doubtless, is due to all of them. Each may serve as a guide, in regard to the quantity of blood to be drawn, the repetition of the operation, and the like; while, taken in conjunction with other circumstances, it may serve to determine the propriety of the evacuation altogether. We shall have occasion to recur to this part of the subject, when speaking of the use of blood-letting as a remedy for inflammation—by far the most important purpose for which it can be applied.

THE MORBID ANATOMY OF SCROFULOUS FORMATIONS.

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Communicated by Sir James M'Grigor, Bart. Director Gen. Army Medical Department.

[Concluded from p. 292.]

Tubercles have been described as encysted: for my own part, I have never seen but one instance of an encysted tubercle, properly so called. It may, therefore, be laid down as a fact pretty well ascertained, that they are rarely found encysted. I do not, however, by this mean to deny that a solitary tubercle, occurring in the midst of cellular tissue, may not now and then be met with, possessing a distinct enough cyst, yet this must be looked upon as sufficiently rare; and, indeed, nothing is more uncommon than to meet with a tubercle possessing an envelope of condensed cellular tissue, while no part of its centre or circumference is as yet softened or broken up.

As to the ultimate termination of tubercles, they either begin to soften in the centre or circumference, or, as has been already remarked, they sometimes appear to die whole, and can thus be turned out of their cavities in such a state. In either case, the inorganized matter thus produced acts as a local irritant, exciting inflammation, suppuration, and ulceration, in the surrounding tissues; in short, a kind of abscess is produced, and pus and tuberculous matter become mixed up together. This incongruous mixture, if discharged, generally leaves behind it a cavity, more or less complete and of variable size, and, more frequently than otherwise, lined by a membrane, if not mucous, nearly allied to it.

The nature of this lining membrane, however, depends much upon situation. Thus, tuberculous cavities in the lungs are almost always found having their inner surface covered by a membrane pretty much allied to the mucous, and in such a case it may be considered as an accidental production, and *mucous* from its continuity with the lining membrane of the bronchi.

The same holds true with tubercular deposits opening on the mucous surface of the intestines, trachea, larynx, &c. &c.; but in whatever part of the body tubercular deposits break up, they are apt to leave cavities, the interiors of which are lined by membranes more or less resembling the mucus. After all, this probably depends more upon the nature of the secretions than any thing else; for as we have already said, that, on the breaking up of tubercular deposits, a kind of abscess is produced, so we are led to calculate, and with some degree of certainty, on the nature of the investing membrane;—because we are aware that an abscess in the lungs is generally lined by a membrane more or less mucous; whereas, again, the serous remains of an apoplectic effusion are, on the contrary, enveloped by

a tissue, smooth, glistening, and in every respect resembling serous membrane generally.

When occurring in the form of masses.—In the majority of cases, tubercular masses are merely the result of many tubercles congregating together; and this appears clear from the irregular and *tuberculated** outline which these masses generally present.

Moreover, such formation can often be very distinctly demonstrated, and nothing is more common than to meet with miliary tubercles in clusters, which, as they increased in size, would no doubt assume this form, and similate the appearance in question. I have said, in the former part of this article, that we seldom meet with miliary or unripe tubercles of a size larger than a grain of wheat; and as tubercular deposits deserving the name of masses are generally above the size of a walnut, it is clear that it would take a large number of them to compose a mass equal even to one of this latter magnitude. But tubercular masses, such as I am about to describe, are not so composed; they are not made up of unripe tubercles, but are altogether formed either from the aggregation of ripe tubercles or from the scrofulous matter being effused into a part, first in a fluid state, but which ultimately becomes of the proper caseous appearance, from the absorption of its more watery parts. These masses have, therefore, the yellow cheese-like character of the ripe tubercle, and however intimately attached to the surrounding textures they may be, they yet possess the friability and inorganic appearances of the tubercle which has just commenced to break up. How long these masses may remain without beginning to soften, it is difficult to say; but I have known the testicle of a phthisical patient, which had rapidly enlarged to a certain extent, remain stationary for months; and after death, a mass of tuberculous matter, of the size of a hazelnut, was found occupying its centre, firm, and without the slightest mark of degeneration.

Tubercular deposits of a scrofulous nature, are occasionally met with in the liver, of large size; and from their not presenting very well defined borders, it is just possible that they might be looked upon as masses similar to the description under consideration: the mistake, however, can be readily avoided, by observing, that however uneven the outline may appear, yet that their surfaces are sufficiently smooth, and at all events present nothing of the *tubercular* irregularity which has been described as dependent on the aggregation of a number of tubercles. The nature of their origin is to be sought in the following observations:—

I have just said that tuberculous masses sometimes owe their origin to the scrofulous matter being deposited in a part in a fluid state, and that the liquid particles becoming absorbed, the more consistent or proper cheese-like substance remains. I formed this opinion from having observed that cavities are sometimes met with in the lungs of phthisical patients, containing a substance of the consistence of cream, or partially coagulated albumen, of a more or less yellow or greenish-yellow appearance; and I look upon this to be tuberculous matter in a semifluid state—1st. Because by experiment it was ascertained not to be pus; 2dly. Because I have never met with a similar secretion unattended with tuberculous disease; and lastly, Because I have met with cavities where the same kind of substance was evidently in a more advanced state, and where, in consequence of this, it had assumed the proper cheese-like unorganized appearance of a tuberculous mass from other causes. It is rare to meet with more than one cavity in a lung thus affected; it seldom exceeds the size of a walnut, and, if seen in an early stage, the tissue of the lung is sometimes found in immediate contact with the secretions: no cyst is found, and sometimes no bronchial tube can be traced terminating in or communicating with it: in such a case the tuberculous matter was clearly secreted in the substance of the organ; the reverse, however, particularly in the lungs, is more commonly the case, as the preparation of the portion of lung to which I have previously alluded well illustrates, and where a mucous cavity was found filled, as well as two bronchial tubes which terminated in it.

After a time, however, and when the watery parts have become absorbed, and

* *Tubercle?* A small swelling, or excrescence. —*Walker.*

the substance has thus acquired the consistence and character of a large ripe tubercle, a proper cyst is occasionally developed; and I am much mistaken if, under these circumstances, such has not been by some considered and described as an encysted tubercle. The mistake, however, is readily detected by observing that the caseous substance may be completely turned out of its bed whole, and that the inner surface of the cyst will then present a clear shining structure; and it may be be further observed, that while this was being done, the contained matter had no adhesion or connexion whatever to the parietes of the cyst. We may therefore say, that it is tuberculous matter encysted, but not an encysted tubercle, inasmuch as it differed, in these essential particulars, from the only instance of an encysted tubercle which I have ever seen, and which I shall by and by describe; and as, moreover, it has no analogy to the manner in which encysted tubercles (the encephaloid, for instance) are generally found *.

But this which I am describing, and which I look upon to be scrofulous matter encysted, and not an encysted tubercle, is probably more allied in its nature to the atheromatous tumor, but with this difference, that the pap-like matter of the latter is secreted by the cyst which contains it, whereas the contents of the former are secreted first, the cyst being subsequently formed by the irritation occasioned in the part by the presence of the unorganized scrofulous matter acting as a foreign body. Tuberculous matter, in this semifluid state, is sometimes found in a dilated bronchus, and, again, it is occasionally poured out upon an ulcerated surface. There is a preparation at Fort Pitt, corroborative of this statement. The peritoneum which covers the upper and anterior surface of the rectum, presents two small deposits of this nature, and it may be taken for granted that they are exudations from an ulcerated surface; for in the immediate vicinity is a fine specimen of ulceration of a serous surface, which is seen in consequence of my having removed carefully a similar secretion which covered it. The following very interesting case, where tuberculous matter was poured out in great quantities by the secreting surface of the peritoneum, and where, although the man might be said to have died of tuberculous disease, yet there was scarcely a *tubercle* to be met with in his body, occurred in the practice of my friend, Mr. Gaunnie, of the 80th regiment. The patient (James Gould, æt. 19) I did not see during life, but I assisted at the post-mortem examination; and I then understood that he had been only a recruit, and that he had not looked well since joining; that he had been taken into hospital four days previously, apparently labouring under common continued fever, but that symptoms of acute peritonitis shewing themselves, he died in a few hours. The whole of the small intestines were loosely matted together by recent adhesions, and on their serous surface generally, as well between their adhesions as elsewhere, small-sized projections of scrofulous matter existed. They varied from a pin-head to a hazel-nut, and were connected to the surface of the membrane by a loose kind of filamentous tissue. They were whitish coloured, of the consistence of soft cheese, and could be taken from the surface of the peritoneum, so as to leave its secreting surface apparently little affected. The same kind of inorganized matter existed in layers beneath the serous coat of the liver, on its under surface, and the same prevailed to some extent under the peritoneum lining the anterior parietes. The abdominal absorbent glands were likewise affected; indeed the most extensive tuberculous disease of the abdomen that can well be conceived existed. Curious enough, too, that although the bronchial glands were similarly affected to those of the abdomen, with the exception of the upper lobe of the left lung, these organs were quite healthy. In this latter part the disease shewed itself in two days. In one part, a dilated bronchus, of the size of a hazel-nut, was found quite filled, and close to this a cavity, of the size of a

* This is, however, scarcely a fair simile, inasmuch as the encephaloid tubercle is always organized, and for the most part encysted; and the cellular-like connexions which exist between the brain-like matter and the cyst, are therefore dependent on the ingress and egress of vessels. The simile, however, holds good when the *true scrofulous tubercle* does become encysted, as the case which I met with in the brain, and which I shall by and by describe, corroborates. The scrofulous tubercle, therefore, differs from other organized and encysted tubercles in this much, that while they receive their nutrient vessels through the medium of a cyst, it receives them direct from the structure of the organ in which it may be situated.

walnut, lined by mucous membrane, and communicating with two bronchi, was likewise seen, similarly filled. The matter, in these instances, was of the consistence of wet plaster of Paris, without any trace of organization, and, without doubt, was secreted by the membrane with which it was in contact. This is a curious case in more ways than one; for, first, there were no tubercles in the lungs or body generally; secondly, the tubercular matter on the serous aspect of the peritoneum was evidently a pure secretion of the inorganic scrofulous matter by its vessels. For if these tuberculous exudations had had their origin after the manner of tubercles generally, we should have found some in different stages. Now the very opposite of this was the case, for the smallest quantity of matter had the same characteristics as the largest, and not a single miliary tubercle was to be seen.

Scrofulous matter in this mass-like form is chiefly met with in the lungs, conjoined with tubercles; and as they are, more frequently than otherwise, dependent on the aggregation of them, the size which they sometimes acquire is truly astonishing. Beneath the peritoneal surface of the liver, in the substance of this latter organ, in the omentum majus, and beneath the peritoneum lining the anterior parietes of the abdomen, it is likewise common; but in these latter situations it is generally poured out in the form of patches, of a flattened or compressed appearance *, and varying in diameter, from the size of a sixpence to a crown-piece, or even more.

In these cases the morbid product has invariably the friable characteristics of the inorganized tubercle. The largest mass of tubercular matter which I have ever witnessed, is preserved in the collection which is at Chatham; it consists of a whole lung, which is completely transformed, as it were, into this adventitious production. A section of it exactly resembles a piece of *Stilton cheese;* very little of the vesicular structure of the lung remains. It was got from the body of a young soldier, and its origin and growth I am inclined to attribute to the following process, or that by *interstitial deposition.*

Interstitial deposition of tuberculous matter, or tuberculous infiltration — This is the third manner in which we have said that this adventitious product occurs; it is particularly described by Laennec, and has received from him the above appellation. It is a name which appears well chosen, and is peculiarly applicable to this variety of the affection as met with in the lungs. But from repeated observation I am inclined to deviate from that part of his doctrine where he says, " that whatever be the form under which the tuberculous matter is developed, it presents at first the appearance of a grey semi-transparent substance, which gradually becomes yellow, opaque, and very dense*." For when the tuberculous matter is deposited in the form of interstitial injection, or when it is met with in masses, I have never seen it have any other appearance than that presented by tubercles of some standing, and which I have described as ripe. Hence I look upon the tuberculous matter which is met with in this variety as being purely inorganized from the first, and secreted by the vessels of the mucous lining of the air-cells. A portion of lung, therefore, which is said to be affected with tuberculous infiltration, on a section being made presents the same colour as a tubercle in an advanced state does; it has lost its proper healthy characteristics, and does not crepitate on pressure. Its vesicular structure, in a portion recently and partially infiltrated, can be rendered perspicuous by gentle manipulation, and slight ablution in clear water; for by these processes part of the tuberculous matter is got rid of, and each air-cell can be satisfactorily enough ascertained, by the contents only being partially evacuated by this method, to have formed the nidus for the adventitious product.

But the most satisfactory evidence that the tuberculous matter is really contained in the air-cells, is not unfrequently to be found by examining a portion of a lung where this morbid appearance is met with generally; and for this reason, that the air-cells of the diseased part, as well as those in the neighbourhood, are commonly dilated. Hence

* This most probably depends upon the position of the parts when this inorganized product is poured out.

* Dr. Forbes' valuable translation.

those air-cells which have been partly freed of their contents by the processes above detailed, present rather a striking resemblance to the dilated air-cells in the immediate neighbourhood, because the adjoining ones being about to become affected in a similar manner, are more or less tinctured with the scrofulous matter, and have therefore a greater likeness to those which may be said *now* only to contain a little. In short, I consider that for a portion of lung to become really affected with tuberculous infiltration, it is necessary for it, in the first place, to become emphysematous, or, more properly speaking, to have its air-cells so dilated, that they may have capacity to receive the tuberculous matter. This, however, is a state which is but too commonly met with, and on this account, that the morbid appearance which we are now describing is almost never met with but in those cases where the tubercles exist to a very great extent. The consequence of this is, that those air-cells in the immediate vicinity of the tubercles are so completely compressed, and no doubt obliterated afterwards, that respiration in the part affected would be altogether impeded, if it did not happen that both the bronchial tubes and air-cells generally, which are situated in the neighbourhood of the tubercular deposits, become dilated. A nidus is thus formed, and the vessels of the mucous lining of the bronchi and air-cells partaking of the same action which exists throughout the lung generally, secrete the scrofulous matter in question. This, which is primarily poured out in a semifluid state, is of the same nature as that which I have already described, and which constitutes one of the ways by which tuberculous masses are formed. If secreted by the membrane lining the air-cells, there is reason to believe that in many cases it remains there, and gives rise to the appearance which we are describing; but I am very much mistaken if it is not sometimes altogether secreted by the mucous lining of the bronchi; and being carried along by the violent efforts at respiration which patients so circumstanced are obliged to make, it becomes truly *injected*, or *infiltrated*, into the dilated cells.

Again, it is sometimes collected in the dilated portion of a bronchus; and I have seen a part thus enlarged, and to such an extent, as to be capable of containing a walnut, and filled with the matter in question. And again, it is extremely probable that tubercular matter thus secreted may be got rid of by expectoration.

The manner in which tubercular matter is thus contained in the air-cells and bronchi presents a strong analogy to the same occurrence when met with in the vesiculæ seminales, vas deferens, and epididymis. For the matter thus secreted by the vessels of their mucous lining gradually distends these parts, until the pressure from within is so great that the natural tissue begins to disappear, and finally nothing is left but disorganized structure and tuberculous matter. But before the morbid process has proceeded this length, the vas deferens, either when it leaves the epididymis, or joins the duct of the vesiculæ seminales, may be opened, and the scrofulous matter turned out, so as to exhibit the duct to all appearance healthy, but much dilated. The *rationale* of this is, that when scrofulous matter is met with in the epididymis, it may be considered as a secretion from the mucous lining *of* the part; but if seen in the vesiculæ, it is by no means improbable that it may be merely a secretion *in* the part, having been brought there and deposited by the same powers which influence the passage of the semen*.

In the same manner it may accumulate in the air-cells of the lungs, being originally secreted by the vessels of the mucous lining, until the natural tissue altogether disappears, and nothing is left but a mass of tuberculous matter. But before advancing to this extreme it sometimes presents, on a portion of lung so affected being divided, a granulated appearance; and as the grey softening of Andral, or the purulent infiltration of Laennec, has something of the same resemblance to the eye of a junior, it may be necessary to remark that tuberculous infiltration is almost never met with but when tubercles likewise exist. But still tubercles may exist, and yet a portion of lung be so hepatized as to have something of this appearance. In such a case the history of the symptoms may assist; but an

* This, however, is merely offered as a conjecture, because there is no reason why it should not primarily be likewise secreted by the mucous lining of the convoluted tube of the vesiculæ.

accurate examination, and the inorganized nature of the tubereulous matter, are those most to be depended on. Serofulous matter poured out in this semifluid state I have only met with in the bronehi, air-eells of the lungs, lacteal vessels, epididymis, vas deferens, and on the uleerated surface of the peritoneum, as illustrated by the preparation already referred to; it soon, however, beeomes eoncrete, and presents the appearances of ripe tuberele. It is clearly inorganized from the first, and yet it is astonishing to what an extent it may exist, particularly in the epididymis, without oecasioning any inconvenience.

Tuberculous conversion.—An organ, or a part of an organ, eonverted into scrofulous matter, presents on a section being made the yellow cheesy eharaeteristics of the ripe tuberele, excepting in those eases where the ehalky diathesis prevails among the tubercles throughout the body generally; in sueh cases the portion of an organ which has undergone this change assumes then the eolour, and partakes more or less of the same appearances.

Tubercular conversion, like tubercular infiltration, may be said almost never to oecur but when tubercles in some part of the body exist to a eonsiderable extent; and like it, it appears only to affeet certain organs. The absorbent glands and testieles are its ehief seats; but the epididymis often presents appearanees very mueh resembling it. The way, however, in which this latter organ beeomes affeeted, so as to assume this state, is, I apprehend, after the manner of *infiltration*, as already deseribed, the natural tissue of the part disappearing as the tubereular matter continues to be deposited. Subsequently the adventitious production is so held together by the remaining filamentous tissue of the disorganized organ, as to exhibit in many respeets the same appearanees on a seetion being made as a part originally converted would, the only difference existing in the mode of attack.

I have already said that eellular tissue forms the general, if not the exclusive, seat of tubercles; and as the parenehyma of the absorbent glands is almost, if not altogether, devoid of this structure, this is one reason why, in the scrofulous constitution, those organs partaking of the prevailing diathesis have not tubercles deposited in them, but beeome affected in the manner just to be described.

When an absorbent gland, therefore, is about to become converted into serofulous matter, it first enlarges; at the same time it aequires a eertain degree of firmness; gradually at one or more points it assumes a white appearance; these coalesce, until the whole gland is similarly affeeted, and, finally, it degenerates into a heterogeneous substance resembling broken-down tubercles and pus.

Generally a whole absorbent gland beeomes thus affected; but in the liver or kidney the process eommences either in one or more places, sometimes in the eireumferenee, and sometimes towards the centre of the organ, and seldom affeets a space larger than a walnut.

In this morbid process we thus observe three distinct stages. The first is one of inerease, attended with some degree of firmness. This is uniform throughout the diseased part, and may be ealled the preparatory stage*.

The seeond embraees the change from a fleshy appearance to a colour whieh varies from a dirty white to a yellow. This commenees in one or more points, whieh gradually coalesee until the whole is so affeeted; and this is now, properly speaking, the stage of *tuberculous transformation.*

The third and last stage is one of decay; it includes those ehanges of softening and total disorganization which we have already described as affecting tubereles at a certain period of their existenee; it may therefore be ealled the stage of *degeneration.*

The abdominal absorbent glands are its chief seats, but the testicle is not unfrequently in a similar manner affeeted; and portions of the lungs, liver, spleen, and kidneys, are likewise sometimes so diseased. It differs from tubereulous infiltration, inasmuch that, if the tuberculous matter was merely diffused or infiltrated into the texture of the part, one would think that it might be washed out, or squeezed out, or at least got

* We are enabled to trace this morbid change in the abdominal absorbent glands most satisfaetorily; and as this deseription principally applies to the disease as met with in them, I may observe that during this stage they present, on a section being made, somewhat of a fleshy appearance.

rid of in some manner, and to such a degree as to leave some appearance of the healthy tissue remaining; but the contrary is the case. The natural structure of the part affected is irremediably gone; it has lost all normal appearances, and has acquired the tuberculous character without having actually passed into the inorganized and concrete matter of tubercle. My inquiries have never led me to suppose this state advances any further; and instead of partaking of the real character of ripe tubercle, and thus becoming a concrete and inorganized mass, it appears rather to degenerate and break up, leaving more of a sloughy base than a tuberculous excavation.

I formed this opinion from having observed, that however perfectly converted the part may appear to be, yet on a section being made, and a thin slice stretched between the fingers, and torn, it will still present some trace of filamentous tissue; and the healthy structure, although completely changed, and partaking more or less of the tuberculous state, will still present some traces of organization, which is proved by the presence of small vessels, by the filamentous tissue already alluded to, and by the resistance it offers to being lacerated,—all of which a mere mass of tuberculous matter is totally devoid of; it has none of these marks of organization, and is more or less readily broken up, from the friability of its nature. Sometimes a part which is thus undergoing the scrofulous transformation appears now and then to form a nidus for tuberculous matter in a semifluid state being deposited in it; for in one or more places small tuberculous points begin to make their appearance, which are readily recognized from the surrounding tissues by their deeper yellow colour, by their complete want of organization, and by their having exactly the character of the tubercular masses already described. That they are not the tissue of the part so changed is pretty clear from their circumscribed form, and from the capability which exists of turning them out of a kind of encysted bed which they appear to occupy.

RECOLLECTIONS OF CHOLERA:

ITS NATURE AND TREATMENT.

By W. Griffin, M.D.

[Concluded.]

Saline Injections by the Veins.

Of all the triumphs of medical art over disease, none could appear more daring or wonderful to one who has for the first time witnessed it, than the restoration of life, feeling, thought, strength, and animation, within a few minutes, in the blue, sodden, cadaverous, and insensible body of a person who has lain some hours in the collapse of cholera, by injecting into the veins a chemical substitute for the vital fluid that had drained away through the alimentary canal. The temporary effect of the remedy—the extraordinary transition, as it were, from death to life which it almost always produced, even in those cases in which it eventually failed to sustain the improvement, would in itself be sufficient to convince us that a new field for discovery had been opened out to the therapeutist, and a means of altering and controlling morbid action, as well as of restoring vital energy, placed within his reach, of which former physicians had not the most distant conception. It is true, perhaps, that with respect to cholera, the only disease in which it has yet had a trial, this new remedy has disappointed the early hopes it excited in the profession, and been followed by an immense fatality; but it remains to be seen whether those hopes should have been so sanguinely entertained under all the circumstances of its employment—whether in the unsuccessful cases, numerous as they were, the fatal event did not attach rather to the disease than the treatment; and finally, whether the recoveries which resulted could have been calculated on if no such means had been resorted to.

Transfusion of blood into the veins of persons dying of hæmorrhage was long since proposed and practised in this and other countries; but Magendie was the first who proposed, and, I believe, ventured to inject, water, or medicated fluids, into the veins as a cure for hydrophobia, many years back. M. Hermann, of Moscow, more lately, on observing the quantity of aqueous fluid ejected from the body in cholera suc-

ceeded by collapse and death, suggested the injection of warm water into the veins, to preserve the circulation and volume of the blood which remained; and this suggestion was subsequently carried into execution by Professor Delpech, of Paris, though without any success. The practice in this country, however, did not originate in any thing which had been done on the continent; it arose entirely out of the ingenious and clever papers of Dr. O'Shaughnessy, on the analysis of the blood of cholera patients as compared with that of healthy persons. As it appeared the great loss which the circulating fluid sustained, and which seemed to be the immediate cause of collapse, consisted of water, albumen, and saline matter, it occurred to the late Dr. Latta, of Leith, that death might be protracted, if not altogether prevented, and other remedies become available, by directly restoring to the circulation the materials of which it was robbed by the disease. He instantly put the remedy to the test, and his skill and ability are displayed in nothing so strongly as in the fact, that his first inexperienced application of it in the cure of cholera was more successful than any trials which were subsequently made, when there had been extensive experience of its effects. He saved three patients out of nine in his first set of cases, and five out of seven in his second; of which number, on the whole (16 cases), it is acknowledged by all practitioners in and about Edinburgh, two would not otherwise have recovered, taking the ordinary mortality in such states there as the standard. Injection by the veins seemed, notwithstanding the early encouragement given by these cases, to be losing credit towards the decline of cholera, after having been very freely tried; and it is well worth consideration, whether this failure of reputation is founded on judicious inferences.

All the published cases of injection of the veins which I can find amount to 282, among which there were 221 deaths, and only 61 recoveries. As all these cases were in collapse, sometimes very deep and protracted, before this treatment was employed, it only remains to see what portion might fairly have been expected to recover if no such remedy had been resorted to. According to the statements of Drs. Christison and Mackintosh, not more than one in twelve recovered, in Edinburgh, under any previous mode of treatment; and even this calculation of recoveries is looked upon by the latter gentleman as too high. Sixty-one recoveries in 282 cases gives, on the other hand, somewhat more than two recoveries in ten. It may be said, perhaps, that our own reports of the Limerick cholera hospitals give an amount of two and a half, and even three, recoveries out of ten. The average of all the reports from hospitals in that city, however, does not exceed, if it indeed reaches, the average recoveries by venous injections. At the same time it should be considered, that no fair comparison can be instituted between cholera cases occurring among the half-starved pauper population, crowded together in the lanes of Limerick, with those which occurred among the well-fed artizans and labourers of Edinburgh.

It can hardly be necessary to remind the reader of a fact referred to when treating of the mortality of the disease, that persons living on a poor vegetable diet, though much more liable to fall into cholera than those living on a sufficiency of animal food, recovered nearly in the proportion of two to one comparatively. In Limerick the recoveries from collapse among the wealthier classes of the population, in private practice, were, I should say, quite as low in amount as the proportion named in the Edinburgh report; and even in the hospital reports of St. Michael's parish, the one in which all the wealthy portion of the population of Limerick reside, and where the class of patients admitted consisted chiefly of well-fed servants and small shopkeepers, the average recoveries from collapse did not exceed one in ten, while in every other hospital in the city it exceeded two in ten. The legitimate conclusion from all I have stated is, that the average recoveries from collapse by injection of the veins has far exceeded the amount by any other treatment, in the same district and under the same circumstances, and has equalled the utmost proportion which has been obtained on the average of large numbers, in any district and under any circumstances.

It has been asked by some medical men who lost faith in the remedy, can any means be called curative which saves only 61 patients out of 282? But this, it must be admitted, is not a fair

mode of estimating any treatment, since, putting all other considerations aside, if one plan proves more successful invariably than another, however small the amount, it is not to be rejected merely because that amount falls immensely short of what we are anxious to attain. The fair deduction from all our present experience surely is, that injection by the veins has recovered more patients from collapse than any other treatment, on the average of all the cases in which it was tried; that while in some sections of these cases it has been confessedly unsuccessful, in others it has almost realized our most sanguine expectations from the efforts of human art; and finally, that as in those latter it has manifested its great curative influence beyond question, its failure in the former must be attributed to some mismanagement or accidental fault in the application, rather than in the principle of the remedy. To advert to some of these which may have operated to a very injurious extent, and may yet be wholly obviated by a more extended experience and greater care, I may mention the danger of having any decomposed salts in the fluid—of having minute portions of foreign matter intermingled with it—of not having the instrument air-tight—of allowing the admission of air in applying it—of injecting too much fluid, or even the proper quantity too rapidly—of using too little or too much saline matter—of repeating the operation too often, or deferring it too long, or of performing it at an injudicious period of the disease.

There are certainly two points of great difficulty in applying injection by the veins: 1st, a difficulty in ascertaining when enough of fluid has been thrown in; and 2dly, in selecting the most opportune period of the disease for the operation. With respect to the first, Dr. Lawrie's recommendations seem exceedingly judicious; he says he would cease injecting whenever the pulse was steadily improved — whenever the patient fell asleep, whether the pulse were improved or not—whenever the respiration was much hurried—and whenever acute pain was felt in the abdomen. In the first instance, because injection had done all that it can do; and in the others, because it had begun to do harm.

With respect to the period of the disease most favourable for injection, I think it very important that it should be accurately determined. Much of the fatality following the operation may have resulted from its performance at an injudicious time. Dr. Lawrie remarks, "If fluid is thrown into the circulation of a person whose system is at the moment labouring under the poison of cholera, the salt and water is drained off along with what remains of the serum of the blood, and the mucous membranes are injured by having an increased quantity of fluid forced through them. On the other hand, if we are too late in employing the remedy, throw in what quantity we choose, the pulse never returns to the wrist; it becomes powerful—too strong in the iliacs and carotids; and if we persist, delirium and fearful irritation soon close the scene." Dr. Lawrie is hence of opinion, "that injection will only do good after the violence of the disease has expended itself, the greater part of the discharges having taken place, and before permament sinking or reaction are established. If reaction has begun, he believes injections to be useless, probably injurious."

Notwithstanding the feasibility of these reasons as they apply to early injection, I think there are facts equally strong which would lead to a different conclusion from Dr. Lawrie's, and induce us to resort to it at the first moment of collapse, and perhaps before the pulse is altogether extinct at the wrist. There appears to exist a very decided relation between the occurrence of pulseless collapse and the consecutive fever; as if the partial cessation of the circulation occasioned some organic mischief, which was the cause of the subsequent reaction and its results. The facts, that any patient, however close on the verge of collapse, whose pulse has not become imperceptible, will escape the consecutive fever, and that those who are treated with injection by the veins in that state, do also escape it, if they are to recover at all, must sufficiently prove the advantage of its early employment, even at the risk of having afterwards to contend with what Dr. Lawrie so much apprehends, an *unspent disease*. The danger of cholera, so long as the pulse has never failed, or nearly failed at the wrist, even if we should have to contend with it, after injection, is out of all measure less than the danger of the after consequences of a state of

perfect collapse existing for a few hours. These opinions are strongly supported by the post-mortem appearances where death has taken place, dense fibrous clots being frequently, nay, almost invariably, found in the right side of the heart, extending into the branches of the pulmonary artery, and great venous channels in the head. "It appeared," Dr. Mackintosh states, "to all who watched the symptoms and witnessed the examinations, that these plugs were formed *during the progress of the stage of collapse*, and not after death."

When injection by the veins is resorted to as we have suggested, at the earliest period of collapse, or just previous to its becoming quite decided, it is essential, for the reasons mentioned, as soon as the patient is a little revived, to resume the active medical treatment, as if no such operation had ever been performed, and we were only anxious as before to prevent the occurrence of collapse.

I have entered, perhaps, with unnecessary minuteness into the merits of a remedy, the interest in which may appear to have gone by. The temptation to do so has, however, entirely arisen from a conviction that the injection of medicated matters by the veins, though as yet applied to the cure of cholera only, is a treatment of extraordinary power adapted to many morbid states of the system beside the collapse of cholera, and certain at some future period of time to become a well-understood and successful therapeutic agent.

SOME OBSERVATIONS

ON THE

MEDICAL STATISTICS OF ARMIES.

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Part I.

In the "Notes on the Medical History and Statistics of the British Legion of Spain," published some months ago, I endeavoured to condense the observations of many preceding years, passed in active service with the armies of Portugal and Spain, and to give only conclusions and general results bearing upon points of importance to the welfare of soldiers.

Few subjects of equal interest and weight have been so much neglected or so loosely treated as the medical statistics of armies; yet throughout the various fields of inquiry upon which men are ever ready to devote their energies, it would be difficult to select one more rich in facts, or more promising in valuable results. In its scientific and political aspect the subject is equally worthy of attention. The inquiry embraces, first, the causes affecting the health and lives of soldiers, and the means of estimating, before a period of action, the efforts in men and measures required to successfully sustain a war. For although military tactics and strategy, which decide the fate of a battle, form no part of the subject, yet the number and quality of effective men which a given force will furnish the general for action does, and the success of military combinations obviously depends upon the quantity and quality of the materials—that is to say, upon the physical means by which the highest designs of a master mind can alone be executed.

And, secondly, the precise effects of these causes, both as regards degree and duration, from whence arise the means of distinctly appreciating before the event the consequences of any given combination of circumstances, which can often be foreseen, upon an army composed of a definite number of men and officers; the necessary result of such knowledge being to guide the judgment in measures of strategy, and to enable preparation to be made for inevitable consequences. These objects are to be attained by tracing effects to their causes, and reversing the process, by following the causes down to their effects. In war, the same circumstances continually attend armies; often will they follow in the same order and succession, producing similar combinations, and invariably giving rise to similar effects. Upon the principle, therefore, that similar causes under similar circumstances will produce like effects, and that armies are constantly subjected in successive wars to parallel causes and effects, is based the conviction, that nothing but well-directed observation has been wanting to furnish conclusions at once accurate and invariable in their application, and affording information

of the highest importance to the well-being and success of armies.

Sickness not only destroys more silently, but far more widely, than the sword; and an inquiry into the history of the various wars in which men have taken delight since the beginning of the world, might lead to the startling conclusion, that diseases has often played a more decisive part in the final results than the sword, which still has claimed the honour, or borne the disgrace. Not only the close observation of facts is required; but the generalization of all data and results connected with the constitution and career of armies, from the individual to the mass, now sweeping hill and plain like a wasting torrent, in the full vigour of destructive energy, and anon pouring in lengthened stream to the hospitals, enfeebled by disease, crippled by wounds, and blighted by reverses, is essential to the solution of problems upon which so frequently hinges the fate of armies, and even of nations.

In no attribute of man's nature has he more jealously maintained his pre-eminence as head of the creation, than in his destructive propensities. Jackals and lions only feed and destroy when they are hungry, but man is always ready to hunt down his neighbour. It is strange, therefore, considering the wondrous alacrity with which men have ever ranged themselves for battle, whether, among savages, for a murder done, or, among civilized races, for a lady's eyes—a tomb—a crown—or any other thing that is, or was, or will be—how little attention has been paid to subjects bearing upon success and life; for that which has been said of literature applies to war—"*Ne pas réussir, c'est périr.*"

If proof were necessary, that we gratify our passions and propensities first, to think of consequences afterwards, and that foresight is among the rarest of human virtues, most wars are rife, and especially as regards the subject under consideration. The Spaniards offered a signal example in their first *Christian* wars; albeit they had long before shown that they yielded to no nation under heaven in the love of bloodshed, a character worthily maintained at this moment, yet it was only in the last of their exterminating wars against the Moors in Andalusia that they thought of providing even for the *cure* of their wounded near the field, when "*El celo caritativa de la Reina*" (Isabella) provided a field hospital and surgeons (that is, tents and barbers). If men liable to wounds in their own persons think not of immediate remedy, they cannot be expected to devise means for diminishing waste of life in their heirs and successors.

Since that period the science and art of surgery, and therefore all medicine, has gained largely by the exertions of those engaged in the service of armies, and this in itself forms an interesting subject, not altogether foreign to the general question. It could, however, only find place in an inquiry made on the enlarged scale alluded to above, and which has never been successfully pursued, or perhaps attempted. Some very general and imperfect statements of numbers, with here and there a disquisition or a treatise on some of the causes of sickness in armies and navies generally, or under some peculiar circumstances, have alone appeared. In other words, detached portions have been more or less ably treated, but they have been scattered seeds, producing little fruit—fragments of a great picture, which separately wanted both value and harmony. The questions, even when opened in their whole extension, have rather been rendered attractive by snatches of information, than useful by being pursued to their legitimate conclusions, each firmly based upon well-sifted and generalized facts.

It was under a conviction that the medical statistics of armies formed a field hitherto neglected, and capable of yielding a rich harvest, that I endeavoured, in the "Notes," to give some statistical results of a detached force through its period of service, rather in the hope that others more capable might hereafter take the same path, than that I could in any important degree supply the deficiencies of past wars, or fill up, unaided, a design of colossal proportions. A paper on the mortality and sickness of soldiers appeared lately in the Lancet, founded upon apparently a long and laborious study of the states of the Peninsular army, in the Adjutant-general's office, in which many returns are given, and a number of conclusions drawn. An analysis of this paper has induced me to throw my own observations and conclusions on the same subject into a connected form, and this affords me both the opportunity and the

motive for filling up, in some degree, the sketch I have already laid before the public. A general view of the results which might be obtained from the medical statistics of armies, and the means by which its important objects would most certainly be attained, shall conclude this paper, and will thus complete the outline. The analysis of the chief features of the paper alluded to on the Peninsular army, and the conclusions and returns contained in it, will serve as a running text to illustrate what I conceive might be done in such a field, and how much is yet required before we can hope for success.

Mr. Edmonds's paper embraces so many points, by the bearing and scope of his conclusions and tables, which are numerous, that it will be advisable to reduce the propositions, together with the facts and returns by which they are supported, into separate and distinct heads. My object is not to criticise the paper, but to ascertain, by careful analysis of its contents, how far a great deficiency in the medical statistics of armies has been supplied, and all the various and important indications of the subject fulfilled.

Preserving, therefore, as nearly as may be, the words of the writer, the chief propositions seem to me to be these:—

1. "The extent of the sacrifice of life and health in an army engaged in a campaign is capable of being estimated by the aid of the proper statistical documents."

2. "Also the sources of the inefficiency of an active army, hitherto imperfectly understood."

3. "The knowledge of the particulars of loss in a given time, and number of campaigns of the Peninsular army, will suffice to afford a correct idea of the destructive effect on life and health produced by a vigorously prosecuted war on an army generally victorious, and rarely suffering from disastrous retreats."

In support of these, many returns are given, and conclusions formed upon the consideration of others not published, tending to determine the following points:—

1. The total loss of life, whether from disease or battle, incurred by the Peninsular army, in a given time (41 months,) distinguishing officers from privates.

2. The average proportion of the army constantly sick.

3. The proportion of killed in different battles, and the proportion of wounded.

4. The relative mortality in the same battle, of officers of different ranks and arms.

5. Comparative mortality of officers and privates at different seasons.

6. The proportionate mortality of officers and men from battle.

7. The proportion of wounded in a year.

8. A general law connecting sickness with mortality.

On these different heads I would observe, that the extent of the sacrifice of *life* may be calculated from Mr. E.'s returns; but no means are given of estimating the sacrifice of *health* in each campaign or period; the divisions of seasons, summer and winter, not corresponding with them. Sir James Macgrigor, in a paper on the Diseases of the Peninsular Army, published in 1815, in the same manner gives total amounts of sickness and loss in hospital each year; but not having given monthly results, it is equally impossible from them to arrive at the precise data required for the due estimate of the sacrifice of *life* and *health*.

The sources of the inefficiency of an active army cannot thus be clearly demonstrated; for although the mortality which can be calculated in any given period gives an indication, it is by no means complete; the numbers sick and invalided, or disabled for service from injury to health, by disease or wounds, are essential to accurate conclusions on this subject.

Many important particulars of the loss are wanting. The total loss, in forty-one months' war, is alone distinctly brought out; but before this can be reduced to averages which may be safely applied to other armies and other wars, the whole loss must be rendered capable of separation into periods as various as the circumstances under which the army was placed, and so traced to the causes, which are neither the same in character nor degree at different periods.

With regard to the facts in proof, consisting of returns and statements, the total loss of life, from wounds and from disease, perhaps the most important of all distinctions, is stated, as "probable distribution of deaths," which is by no means satisfactory. Although it may be correct, it is impossible to rely upon it without it can be demonstrated.

The average proportion of sick in-

cludes both wounded and sick; and even were it otherwise, the furnishing a table of half-yearly periods instead of monthly, does not allow of the calculation of relative sick and wounded in the different operations and circumstances of the army, which form the only true periods, and not the seasons of summer and winter. The various tables respecting killed and wounded, although useful and interesting, if made out monthly, and divided into the different arms as well as ranks, would have been fit for much more extensive application than they are at present.

The general law connecting sickness with mortality is simply stated as one death, wherever there are two years of sickness; and as no returns or facts are brought forward to demonstrate its accuracy, it is not necessary to enter at length into the subject. It seems to me, however, an inconvenient mode of averaging, and not capable of application except to the past. The average loss per cent. per annum upon any given number of sick, or of wounded, distinguished from each other, is required. The paper offers no return upon which such an average could be formed. By this average the loss upon a given number at any time present in hospital could be estimated without difficulty, and upon at least equally certain grounds.

The chief questions involved, and the tables and statements bearing upon them, being thus shortly examined, we are at liberty to enter more at length upon all the important features of the medical statistics of armies, recurring from time to time to the paper before us.

Agreeing that the sacrifice both of life and health may be estimated, it becomes a question, what are the proper statistical documents, and whether any documents of *one* army only, can prove sufficient to establish a just average of the loss in all? If it be merely desired to ascertain the loss in a late war, without any farther object, the question resolves itself into one of mere curiosity, tending to no useful end, and not worth discussion. The true object of all such investigations should be to afford an average which may be applied in any future war. The accurate results, therefore, of one army serving through several campaigns, engaged in various operations, and consequently placed under great diversity of circumstances, would form a most important step towards the attainment of so desirable an object, upon the principle already stated, of similar causes and effects. For this purpose, the documents, to be complete, should show *the relative sacrifice of life and health in all the various periods, made into distinct epochs by the operations of the army.* Such as the results, during an active campaign, in which the troops constantly advanced and fought successfully in summer,—in winter. During a season passed in healthy, and a similar period in unhealthy cantonments. During a campaign marked by a rapid succession of battles. During a long and harassing retreat. While besieged, or besieging. During a first year's service, and a last, when the army is chiefly formed of veterans and hardened troops. Many other leading circumstances might be enumerated, which exercise great and direct influence upon the health and life of soldiers.

Any army which presented all these varieties, and of which accurate returns could be studied and classed, would go far to establish a true average for the aggregate loss in war. In every one of these circumstances the loss varies. The greater part of these vicissitudes appear in the History of the Peninsular Army. But can the returns extant be so divided or classed? The tables furnished by the paper lately published do not enable us to decide how far it may be possible. Some of these classed results may be obtained by using the materials of the first, and by far the most valuable return, which gives the monthly loss by death for 41 months.

If, instead of a half-yearly return of deaths and number living, a monthly return had been furnished of the number in hospital, from sickness, from wounds, and the deaths of each, together with the monthly muster of living, any new combination to illustrate the influence of particular operations could easily have been formed. A similar table for the officers would be desirable; and if the whole had been cast into the results in different arms, the aggregate loss, and that of the particular periods and epochs, in relation to the seasons and operations of the army, could both have been demonstrated with facility, and would have been conclusive as far as regards this feature of the inquiry. These returns, however, would not be

exempt from a very likely source of error, viz. the different circumstances under which the divisions of an army may be placed. One part may be in campaign, another blockading, and a third in quarters. Where such striking differences occur, if the whole are included under one head, such a return will inevitably lead to a false average of the effects resulting from any one of these circumstances. The sub-division into the peculiar forms of disease and injury causing the admissions and deaths of officers and men, would form the last and most abstruse series of facts necessary for the entire and full elucidation of the subject pursued to its ultimate objects.

The sources of inefficiency, and the particulars of loss, are the Alpha and Omega of the medical statistics of armies; and within their scope is included very nearly the whole subject. The sources and particulars lead to the accurate appreciation of the causes, and their relation to the effects, which form the losses of an army. The objects of the inquiry are thus presented to the mind in their most simple and concentrated form.

Although I have carefully observed the results of the various circumstances and vicissitudes of an army, through many changing periods, yet the difficulty of obtaining returns to be depended upon, from either the Portuguese or Spanish armies, on many occasions prevented my verifying by numbers the conclusions resulting from more general observation. I convinced myself, however, that error exists very commonly as to the periods and operations which are most disastrous to an army, and the most costly by the sacrifice of life and health. The following are my conclusions on this head:—

An inclement winter, ushered in by a wet autumn, passed by troops in quiet cantonments, is the most destructive period.

A long harassing retreat, or series of retreats, the second.

A campaign with a succession of battles, however hard fought, but successful in their results, is among the least destructive periods, and infinitely less costly in health and in life than either of the two former.

The heat of summer is as injurious as the rains, or the cold of the succeeding seasons, causing as much sickness and mortality. Hence in an active and successful campaign, different seasons produce nearly similar results.

In support of these opinions it is to be remarked, that the healthiest periods of the Portuguese army, varying in strength from 10,000 to 30,000 men, in the late war in Portugal, were three summer campaigns; one before the Queen's army was hemmed in by an overwhelming force, and besieged in Oporto; a second, which commenced on the 18th of August, when we cut our way through the lines and drove the enemy beyond Valongo; and a third, which commenced in spring, and closed the war by the capitulation of Don Miguel and his army, together with his companion, Don Carlos, in Evora. In all these, the last especially, there was no lack of fighting or of marching; one action only, in the first campaign, ending in a short retreat.

The Legion, in its winter cantonments in Vitoria and its neighbourhood, lost 1200 men and 45 officers by sickness in eight months, the force at head quarters averaging 7000; 25·7 per cent. per annum in serjeants, rank and file, and 18·6 for officers. The Spanish army, amounting to near 30,000 men, in the same neighbourhood, lost grievously also, although in a less degree, being better cared for by their authorities, and not so much exposed to evils from which the Legion suffered. In the next twelvemonths, beginning in May, there was but little improvement in quarters, and the winter was inclement; but the whole period was passed in movements, a succession of actions, and almost incessant skirmishing. In this period, out of an average force of 7000 men and 360 officers, upwards of 1600 rank and file, 22·8 per cent., were wounded, and 197 officers, 54·8 per cent. The mortality was only 900 serjeants, rank and file, and 40 officers, in which number is included 400 rank and file, either killed on the field or dying during the action, and 36 officers. Thus the total annual loss was 12·8 per cent. from all causes, more than one-half less than in the previous period; and 8·7 per cent. of this loss was from battle, 5·7 being the average killed; whereas the whole loss at Vitoria was from sickness. Thus, when it was argued in the House of Commons that we had no

right to draw conclusions between the proportionate mortality and losses of the Legion and those of the peninsular army, because we had not marched in victory from the east to the west of the kingdom, because we had not stormed Badajos, &c., it seemed to me that had not an erroneous opinion been entertained by the honourable gentlemen as to the *true* causes of the greatest loss in armies on active service, they would have seen that the Legion were far from shewing themselves favour by voluntarily challenging the comparison. On the contrary, they had been subjected to more disastrous causes, because the Government they served did not place them in circumstances to march, as had the peninsular army, from battle to battle across the kingdom. It is true that there were counterbalancing circumstances, upon which it might have been said the comparison did not hold. The Legion had no long and harassing retreat like that from Madrid and Burgos to the mountains of Portugal behind the Coa and Agueda; therefore it might not be fair to compare results without due allowance for a long and harassing retrograde movement, and the consequent actions fought under disadvantageous circumstances,—the most disastrous of all the periods of that army save one, the succeeding winter, which was passed in cantonments. As far as my memory serves me, however, no claim was made upon this ground; they claimed an extra allowance for mortality, because that army had been distinguished by many great battles, sieges, and glorious achievements. And in this they were wrong, or I am much mistaken, for it will be easy to prove, from the documents at present under consideration, that the periods distinguished by well-fought battles, victories, and assaults, were among the least, and not, as was inferred, the most costly to them. This is not a mere question which regards the degree of credit or reproach due to the Legion, which is of no great moment to any but those affected by it; but it forms one of the most vital of all the questions embraced by the medical statistics of armies. It is important, therefore, to test the conclusions drawn, by the returns of the peninsular army. If allusion has been here pointedly made to a period of the British army's career marked by discomfiture, retreat, and loss, it is from no desire to follow in the footsteps of those who have taken a patriotic pleasure in tarnishing the laurels of the Legion, and deepening its trials and losses into disgrace, to support a party question in politics. Mr. Edmonds seems to have been struck by the fact, "that the deaths of privates are, in several instances, as numerous during months in which there was no battle, as during months in which important battles were fought."

Taking the monthly tables of deaths furnished by Mr. E., I have classed them into different periods of campaigns, &c., and the following is an analysis of the chief features of each, and their results. These returns commence in January 1811, when the main part of the British army were occupying the lines of Torres Vedras, which they entered in October 1810. Passing over the two months before the army broke up in pursuit of Massena as merely a fraction of a period, we begin with the

1*st Period.* — A campaign opening with the pursuit of Massena, March 5, 1811, and ending with the failure before Badajos, and the doubtful but most costly battle of Albuera, where, out of 6000 British infantry engaged, only 1500 were left standing. This was a spring campaign, commencing with a rapid advance of twenty days, and a succession of combats, and one of these the hottest for its duration of any in that year, when Graham's division was nearly sacrificed by the Spaniards, and he was left to fight the battle of Barossa single-handed, losing in an hour and a half 50 officers, 60 serjeants, and 1100 soldiers. Thus it was a mixed campaign in a favourable season of the year, partaking of the characters of success and failure in nearly equal proportion. In the battle of Fuentes Onoro, where the loss of the Allies was estimated at 1200 men and officers, exclusive of prisoners, there were partial advantages and reverses on both sides, and each party claimed the victory. The siege of Badajos was relinquished after a loss of about 1000 men, and the battle of Albuera completes the period. This campaign gives a monthly average loss of 551, or 10·7 per cent. annually. As the army during this summer was not certainly more than 57,000 strong, a more true average would be 11·6 per cent. It is worthy of remark, that the two first months, signalised by advances and

success, notwithstanding a number of sharply contested affairs, including that of Barossa, presents a much lower average mortality than that of the two months preceding, passed in the lines of Torres Vedras. The excess in the average of the whole period rests entirely with May, in which was the siege of Badajos and the battle of Albuera, one a failure, the other only partially successful.

2d Period. — Summer cantonments. In June, July, and August, the troops were quartered near the Guadiana, where fever was very prevalent. Although in August, Lord Hill's division moved to Villa Viciosa, and the rest of the army marched to the blockade of Ciudad Roderigo, on the Coa, the whole of the month may fairly be included. This period gives a monthly average of 527, 11·1 per cent. per annum. August, part of which was occupied in marching, presents the smallest loss.

3d Period.—The next and last four months of the year form a mixed period, partly occupied by the blockade of Ciudad Roderigo; little doing, however, except towards the end of September, when the main army was attacked, and twice made short retreats. Lord Hill's division made some movements, and had two or three successful affairs with the enemy, both in October and December. Thus it stretched through two seasons, autumn and winter, the army generally stationary. The loss by the sword in this period was so trifling as scarcely to be worthy of notice. In the chief affair of Elboden, and the double change of positions, the loss of the main army of the Allies, was only estimated at 300. The period partook more of the character of cantonments than of the active operations of a campaign; and the latter part of the time, during the fall of rains, the troops were cantoned and stationary on the Coa and Agueda. Colonel Napier, in his eloquent History of the Peninsular War, tells us that there were 20,000 sick at this time, whereas by the returns from which we are quoting it would seem there were only 13,405 (?). The monthly average loss is 644½, making the annual average 12·5 per cent.

4th Period—Stretches through four months of winter and spring, beginning on the 1st January, 1812, and ending in April, a vigorous and hotly contested campaign, in which siege was laid to Ciudad Roderigo and Badajos, both carried, and the latter by storm, with great slaughter. The campaign was carried on through very inclement weather, the sick and wounded moved to a great distance, more than forty leagues, through heavy rains, which lasted nearly the whole time of the siege. Whether we regard the labour of the soldier, the inclemency of the weather, and the distant line of hospitals, or the immense loss at Badajos, it is difficult to conceive a more destructive or disadvantageous period for an army. The monthly loss is 863¼; annual loss per cent. 16·8. The month of May was one of rest, and gives a loss of 595.

5th Period—Is a campaign of six months, stretching through summer and autumn, beginning in June and ending in November, during which the battle of Salamanca was fought, and the army finally retreated from Madrid and Burgos to behind the Coa and Agueda. Up to the commencement of the siege of the castle of Burgos, in August, the campaign was successful, varied by many affairs, and the well-fought battle of Salamanca. Failure and retreat began with the siege at Burgos; five unsuccessful assaults took place before the siege was raised on the 21st of October, when a retreat of 200 miles commenced, and after long marches, seriously harassed by the enemy, and more than once obliged to turn at bay, the previous cantonments behind the Agueda were gained on the 27th of November. The loss by the sword was not excessive in this campaign; it is difficult, however, to estimate it accurately, for killed and missing are very generally classed together. Now the loss under that head, arising from the double retreat alone, is estimated at 9000! The average loss per month during the campaign was 872, according to the return published by Mr. Edmonds; making the annual average loss per cent. 17·01. The loss in the *two* last months is equal to that of the preceding *four*, in which, besides the fierce combat of Salamanca, there were several less important and successful affairs, and much marching and hard work.

6th Period.—The last campaign was succeeded by five months of winter passed in cantonments, the army tolerably provided and undisturbed. The average monthly loss was 1220, or 19·8 per cent. per annum.

7th Period. — A campaign of six months, which commenced in May 1813, when the army advanced from its winter quarters, and, after a series of hard-fought actions, went again into quarters in the Pyrenees. The battles of Vitoria and Pampeluna were fought, and San Sebastian besieged and carried by assault. A campaign as vigorously pushed as it was hotly contested. The monthly average gives a loss of 822, 16.03 per cent. per annum.

8th Period.—Three months in cantonments, beginning in November and ending in January. The monthly loss is $743\frac{1}{3}$, or 12·5 per cent. per annum. Continual partial engagements, and some movements, took place during this time.

9th Period.—The last of the forty-one months, a spring campaign, beginning in February and ending in May; entirely successful, the troops advancing, and two important battles, Orthes and Thoulouse, were fought. The average monthly loss Mr. Edmonds makes $1240\frac{1}{4}$! 24·1 per cent. per annum.

Here, then, are thirty-nine months divided into nine distinct periods, each having their peculiar characters bearing directly upon the loss. Let us see what inferences they bear. The first most prominent result is startling—namely, that the shortest and most successful of the campaigns in which the battles of Thoulouse and Orthes alone were fought, when the troops were all veterans and hardened, should produce a much larger loss than any other period in the whole forty-one months; far beyond that in which Badajos fell, its breaches choked, and its ramparts covered with our dead; outstripping the loss of the most unsuccessful campaign marked by doubtful battles, assaults repelled, and retreat; greater than the campaign which witnessed the battles of Vitoria and Pampeluna, and the storming of San Sebastian. In fine, leaving all others behind, its average loss is 24 per cent., while the worst of all the previous periods is 19·8. 2513 deaths are given to March alone; the battle of Thoulouse, which only threw some 1500 wounded into hospital, took place in April; and this loss is double that occasioned by the onslaught at Badajos.

Sir James M'Grigor, too, informs us, that in hospital during the whole half year, from January to May, 2900 died in hospital—that is, only 9·4 per cent. on the year. The loss, by these returns, is 18·7 upon the six months; therefore 15 per cent. on the whole strength of the army, must have fallen on the field, or upwards of 4000! This seems so incredible, and is altogether so disproportioned to the number of wounded admitted, which only amounted to 4500 in the six months, and the sick to 20,000, whereas in all previous half-years they had been upwards of 30,000, that I am led to believe some error may have crept in. I shall, at all events, leave this out as doubtful, and consider the other eight periods.

It will be observed that the period of greatest loss is the sixth. Comprising five months of perfect quiet in winter cantonments, after the retreat from Spain to the north of Portugal, it is 19·8 per cent. per annum.

That the next is the fifth; a summer campaign of six months, ending in a long retreat. The loss is 17·1 per cent.

That the fourth and seventh come next in order, and present very nearly parallel results. The fourth was a successful winter campaign of four months, in which Ciudad Roderigo and Badajos fell; the seventh a successful summer campaign, equally distinguished by hard fighting — the battles of Vitoria and Pampeluna were fought, and San Sebastian was carried. The loss in the fourth is 16·8, in the seventh 16·3. Here we see how little seasons have to do with the losses of an active campaign: a winter and a summer produce the same average, although there was most inclement weather in the one, and trying marches in the other.

Two periods, again, strike the eye with closely parallel results. The third, a period of four months, of mixed character, with some movements, suecesses, and reverses, but principally a period of autumn and winter cantonment; and the eighth, consisting of three months, passed in winter quarters, interspersed with actions, and the attention of the whole army kept on the stretch and in activity by a bold and indefatigable enemy. They are both periods of mixed character, and both present an annual loss of 12·5 per cent.

The first and second periods also present like results. The first was a summer campaign of mixed character, marked by success and failure in nearly equal proportions. Two or three costly and doubtful battles succeeded a heat-

ing pursuit, and a siege, continued with considerable loss, and finally abandoned. The second, a period of three summer months passed in cantonments. The average annual loss on the first is 11·6; of the second, 11·1 per cent. In reference to the conclusions stated a few pages back, we find, then, that these returns prove—

1. That the most disastrous of eight periods is the sixth; passed, undisturbed by an enemy, in winter cantonments.

2. That the next most disastrous is the fifth; a campaign concluded by a long retreat.

3. That in each of the two campaigns most distinguished by fierce and numerous battles, and the storming of the strongest fortresses in the kingdom, the loss is less than in the two just described; and, as if to leave no room for doubt, we find the average loss in each corresponds to a fraction.

4. The next in order of loss are mixed periods of fighting, movement, and quiet. The least destructive to life in these periods are the first and second: a partially successful campaign, with much fighting, and a short summer cantonment; and the least fatal of the periods of winter quarters was that in which there was the most fighting, and consequently when the troops were most actively engaged.

Does not this review of facts demonstrate clearly what are the sources of inefficiency and loss in army on service? The question naturally suggests itself—how is it that the campaigns distinguished by the greatest activity, by the hardest fought and the greatest number of successful actions, are less destructive than periods in which they are neither harassed by marches nor slain by foes, more even than those marked by retreats; and that the winters passed in cantonments seem less destructive in proportion to the frequency of their engagements with hostile columns?

And the answer, as I have said, is clear. The chief source of loss in large armies is sickness, and neither shot nor sword—these destroy a smaller share only; and the chief source of sickness, when combined with much necessary exposure and privation, is *inaction*. The excitement of active operations, of marches, battles, and sieges, surrounded by a halo of success, acts like a spell upon the soldier's frame. Cold and rain he alike despises; the more insidious attacks of disease can make no impression; he bears, in truth, a charmed life, and naught but shot or shell, or the keen edge of the sabre, can lay him low. Remove the spell—carry him out of the circle of the thousand ever-varying excitements in which he has lived and moved—leave him to the idleness and misery of his cantonments, which, at best, are never very luxurious, often not weather-proof—and he shivers under the cold, grumbles over his rations, and absorbs every breath of pestilence or disease that floats around him! The bowstring is no longer tense, and the rain as it falls saturates and destroys. The finest soldiers droop; they have lost the high stimulus which braced every fibre of their frame, nerving it with iron powers, and they sink gradually, without effort or resistance, victims to colds, fevers, agues, and dysenteries. In a single month of such a period, more men blench and fall under the stroke of disease, than bite the ground in the hottest battle, or during the same period of the most active operations. This I have both seen and felt. I have slept in the open bivouac, drenched with rain, and such rain as I verily believe only falls in Portugal; I have been frozen during the night, awaking each morning with hair stiffened to my face, and each succeeding day been, not thawed, but broiled under a hot sun during long marches. In the north of Spain, again, I have slept in the snow, but never at such periods have I taken cold or suffered sickness in any shape, although quite capable of suffering from wet feet or a London fog now. So it is with the men. Sufficient care has never been taken of them at the close of a campaign, and in the interval of idleness and lack of excitement before active operations recommence. Beds, blankets, hospitals, medicines, warm clothing, &c. have often been imperfectly supplied at this critical moment, even in the best regulated services, and in badly ordered armies: such things seem not to enter into the philosophy of the governing powers. They would rather make unheard of exertions to recruit new men and incur vast additional expense, than take care of those they have. I do not think this is humane; I am sure it is not wise.

The conclusions to be drawn are strictly practical, and of great importance.

1. Active operations, when an army

is collected, and the privations and trials of service have commenced, unless disastrous, destroying the *morale* of an army by serious or repeated defeats, and ending in a period of inaction, whether in winter or summer, cause a lesser degree of loss than cantonments, unless under such favourable arrangements as can rarely, if ever, be made in an enemy's country.

2. It is of less importance to the health and efficiency of an army, in a physical point of view, to suffer from any deficiencies or irregularities in food, clothing, or quarters, while actively engaged, than when in cantonments. In the former position, the excited mind enables the body to resist injurious influences; in the latter, both body and mind are unstrung, and offer scarcely any resistance on the approach of disease. Marching and fighting, which seem to have been hitherto considered as the most serious causes of sickness and loss, afford a stimulus, which, however it may contribute to wear out the frame rapidly, not the less certainly counteracts the immediate effects of fatigue, hunger, or cold; and more than counterbalances any loss resulting from battle.

3. Never is it of such great importance to the well being of an army to be well provided with clothes, regular pay, good rations, &c. as when it goes into cantonments after an active campaign. Every cause of discontent, or discomfort, becomes a pregnant source of disease at such a period, and no exertion should be spared to remove these causes on the one hand, and find occupation and amusement for the soldier on the other.

Park Place, St. James's,
May 1838.

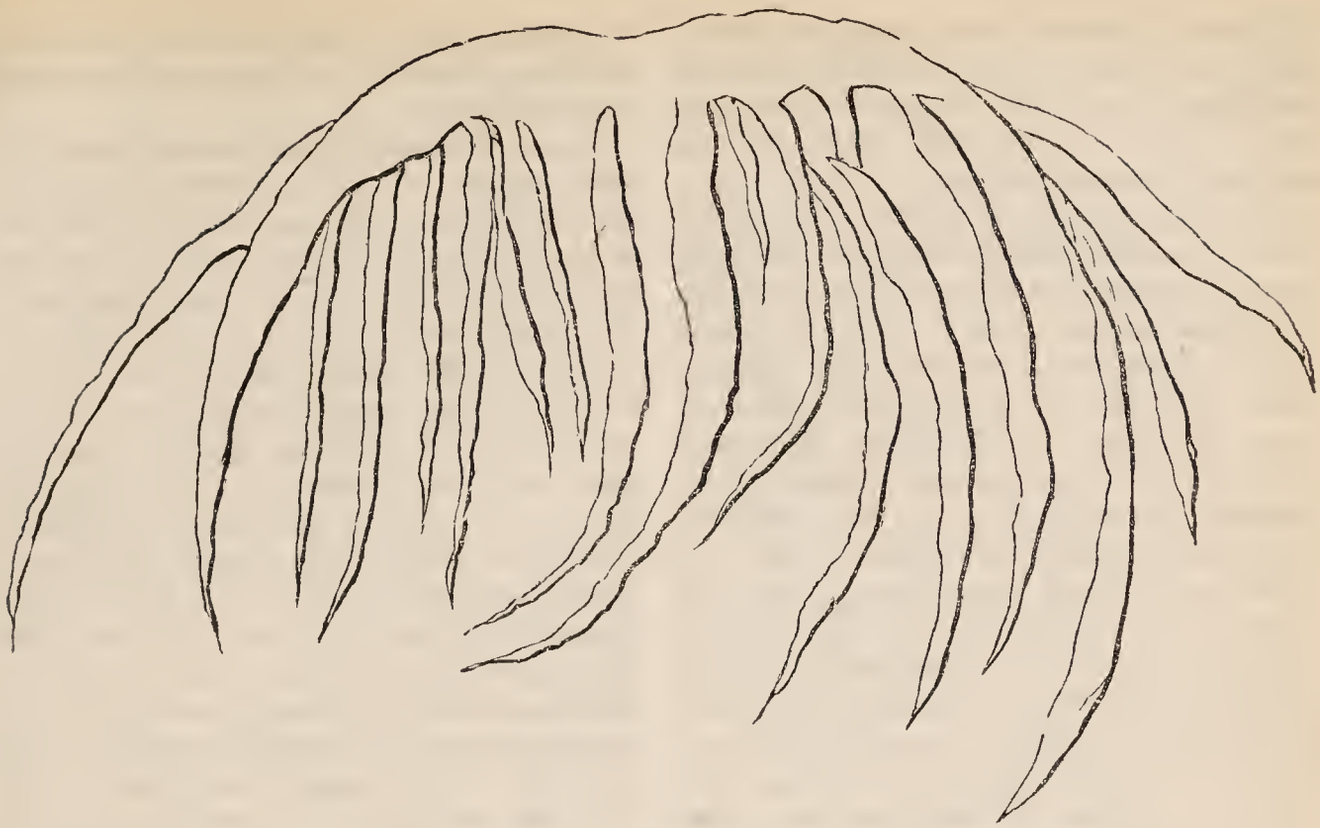
AN ACCOUNT
OF
TWO CASES OF BRONCHIAL POLYPUS;
With Remarks.

By John North, F.L.S.
And Lecturer on Midwifery at the Westminster Hospital School of Medicine*.

The term "bronchial polypus" has been quite arbitrarily applied to certain albuminous concretions formed in the air-tubes. As it has, however, been adopted by the few English writers who have described this not very common form of bronchial disease, I retain it; that the cases I am about to relate may not be distinguished merely by a new name from others on record, to which, in every respect, they are exactly similar. It may be proper to state, that various designations have been given to this form of disease, by different foreign writers; as angina polyposa, catarrhe suffoquant, asthma rarum, croup chronique des bronches, &c. &c.

Case I.—Mr. B., æt. 21, when about four years of age, had been attacked with severe inflammation of the lungs, from which he completely recovered, after an illness of several weeks. No other trace of this attack remained than a very slight and occasional cough, which did not even excite the attention of very anxious parents. He grew up stout and strong; he was active like other healthy boys, and indulged in the ordinary and violent sports of youth, without shortness of breath, or any inconvenience. He continued in good health until the age of 17, when his cough became somewhat more troublesome, but not sufficiently so to prevent him from pursuing an active occupation. He now, also, occasionally expectorated a small quantity of white curdy matter. No increase of these symptoms occurred for a year, when he was quite suddenly attacked with great difficulty of breathing and violent fits of coughing; and for several successive mornings he expectorated a large quantity of substance looking "like a fibrous root with many branches," as his friends, not inappropriately, described these moulds of the bronchial tubes. One of these moulds is on the table, marked 121. He had now some pain in the left side, and could not sleep comfortably when he lay on it. He had no febrile symptoms. In the course of a few days he completely recovered, and he remained perfectly well for two years, having neither cough, dyspnœa, nor expectoration. At the end of this time he was again attacked with slight cough and difficulty of breathing, and, in the course of three or four days, he expectorated many of these "bronchial polypi." As in the former instance, there was no obvious cause for the attack. He had not been exposed to cold, nor had he undergone any unusual exertion. Some of these "polypi" were slightly tinged with blood, in consequence, no doubt, of their firm adhesion to the bronchial

* Read at the College of Physicians, May 7th.



Sketch of one of the "Bronchial Polypi" shewn at the College of Physicians.

membrane. The dyspnœa that accompanied their expectoration was very severe, and one concretion was coughed up with so much difficulty as to threaten suffocation. His breathing had now a whistling sound, and the left side of the chest felt very dull on percussion. For several days his countenance resembled that of a patient labouring under asthma. All these symptoms very quickly ceased, and he has remained perfectly well for more than two years.

Case II.—A delicate-looking boy, æt. 17, had always been subject to catarrhal affections, from slight exposure to changes of weather; but he did not present any symptoms of serious pulmonary disease, nor were the catarrhal attacks ever severe enough to confine him to the house. He was rarely free from a slight cough, but it had not been accompanied by any expectoration. Without any assignable cause, his cough rather suddenly increased, and he began to spit up a quantity of white stringy substance. His breathing now became difficult from slight exertion, and a constant mucous rattling was heard when he breathed.

On the left side, just above the clavicle, the chest sounded very dull on percussion. He was free from pain, and had no fever; pulse remained natural. He continued in this state, with little alteration, for several weeks. The cough then became more frequent, and he suffered very severely from dyspnœa. On the third day from this aggravation of the symptoms, he expectorated with great difficulty and distress, a well-formed "bronchial polypus," exactly similar in appearance to that in the first case. This preparation is also on the table. For many days he continued to throw up, with violent paroxysms of coughing, similar concretions. Sometimes three or four were expectorated in as many hours; but none were so firm, or so complete a cast of the bronchial tubes, as that in the specimen prepared. He quickly rallied from the distress he suffered in expectorating these substances. The cough and expectoration, and râle in breathing, ceased in about ten days, and he was apparently restored to perfect health. The stethoscope indicated a healthy state of the lungs and breathing. He remained well for several weeks, when he was again suddenly attacked in a similar manner, and several "polypi" were again expectorated by violent coughing. This attack entirely subsided in a few days, and for more than a year and a half he has remained quite well. He has grown fat and strong, and is not in any way inconvenienced by variable weather, which he has necessarily been exposed to in travelling.

In the relation of these cases I have purposely avoided a dry daily detail of

slight and unimportant changes; but I hope the brief account I have given is sufficient to render their rise and progress intelligible to those who may not have met with similar cases.

The appearance of these "bronchial polypi" may be briefly described. In form, they are complete casts of many of the bronchial tubes. They are of an opaque white colour, and have more the appearance, I think, of coagulated albumen than of fibrin. The larger branches are much firmer than the smaller ones. Several of the branches, too, are hollow; for I was enabled to inflate them with a blow-pipe, and into some of them a bristle is introduced. A portion that I kept in water for a few days entirely lost the arborescent appearance exhibited in the preparations, and was reduced to a thick mucous-looking substance. Some information may be derived of the nature of these cases of "bronchial polypi" by referring to other cases which my researches upon the subject have furnished me with. In the cases related by John Hunter*, Raickem†, Warren‡, Acharius§, Dixon‖, Cheyne¶, and Iliff**, exactly the same ramiform concretions were thrown from the bronchia by coughing. All these cases recovered. In most of them, the patients had laboured under bronchial inflammation; but it is important to observe, that in nearly all, a considerable time elapsed, *after* the subsidence of the bronchial affection, before the "polypous" concretions were formed and expelled. It appears, then, from the cases related by others, as well as those which I have mentioned, neither unreasonable nor unwarrantably speculative to infer, that there existed, as supposed by Andral††, "une sorte de vice de sécrétion, qui *survivait* au travail inflammatoire," which gave rise to the formation of these albuminous concretions. It must, at all events, be admitted, that these polypi are not the result of common inflammation of the bronchial membrane, either active or chronic; for if they were, they must be of much more frequent occurrence in various diseases that are accompanied by bronchitis.

Some difference of opinion exists as to the nature of these concretions. It would be scarcely necessary to advert to the fanciful belief entertained by former writers, as Tulpius *, Bartholine †, &c., that these substances are the blood-vessels of the lungs which are expectorated, if the same absurd notions were not supported by respectable and much more modern authority ‡. Tulpius, who first advanced this opinion, was himself at a loss to account for the possibility of the pulmonary blood-vessels being thus separated, and thrown off, without the immediate destruction of the patient; and he, and his master, Paaw, professor of anatomy at Amsterdam, regarded the fact *ceu miraculum inauditum*; and Planque was equally puzzled, as he states in the *Histoire de l'Académie de Chirurgie*. The opinion now generally entertained is, that these concretions are owing to an inspissated state of the bronchial mucus, which adheres to the bronchial ramifications; the thinner parts of the mucus being carried off by the constant current of air, and thus a consistent and viscid mould would be formed, of the shape and size of the air-tubes §. This was the opinion of Morgagni and Dr. Warren. Dr. Baillie ‖, indeed, differs from them, but not upon very satisfactory grounds; for he does not refer to the necessary supposition that in all these cases there must be some morbid change in the mucous secretion of the bronchial membrane. His only objection is, that the *natural* mucus of the bronchia would not present, when dry, the appearance of these concretions. It has been supposed that these "bronchial polypi" are similar to the adventitious membrane which is formed in croup. But this appears improbable; for croup is very generally, if not always, a disease of early childhood: and nearly all the cases in which these "polypi" have been expectorated, have been in adults. It may be worth remarking, too, that in an inaugural dis-

* On Inflammation, p. 574; with engraving.
† Bulletin de la Faculté de Médecine, t. 14, p. 38; "Sur un croup chronique des bronches."
‡ Med. Trans. of the College of Physicians, vol. 1, p. 407.
§ Med. and Phys. Journal, vol. 8, p. 201.
‖ Case of Angina Polyposa: Duncan's Med. Comment., vol. 9, p. 254.
¶ Ed. Med. and Surg. Journal, vol. 4, p. 441.
** London Med. Repository, vol. 18. p. 20.
†† Clinique Médicale, 2nde partie, p. 8.

* Observationes Medicæ, lib. ii., cap. 3.
† Epist. Med. Cent. iii. Hist. 98.
‡ Planque: Bibliothéque Choisie de Méd., t. 9, 1766.
§ Bretonneau, "Sur la Dipthérite," offers many instructive remarks on the various modifications to which the mucous secretion of the air-passages is liable, from various causes.
‖ Works, by Wardrop, vol. ii., p. 88.

sertation, published by a German physician of the name of Schmidt, there are some curious experiments, to prove that artificial inflammation excited in the windpipes of animals only produced the adventitious membrane in those very young, whilst every attempt to create it in the old proved abortive. Mr. Porter gives an account of these experiments in his work on the Surgical Pathology of the Larynx. It appears that, first, some change takes place in the mucous secretion of the bronchia, whether from a morbid action of the parts, or from peculiarity of constitution, I do not venture to decide *. The mucous secretion gradually becomes firmer, and forms a mould of the part in which it is contained. Thus, both the patients whose cases I have related first expectorated a creamy-looking mucus; then, at times, a stringy and firmer looking substance; and lastly, well-formed and firm "polypous" concretions. In a case which occurred in the practice of Mr. Read, a part of the expectoration is soft, like viscid mucus. Other parts are firmer, but still scarcely forming consistent bronchial polypi. I presume that until the secretion has become firm, so as to block up many of the bronchial tubes, but little distress is occasioned. The process of respiration is not much obstructed. But the violent cough, and equally violent dyspnœa, come on when the substance contained in the air-tubes becomes firm, and adheres to the bronchial membrane, and forms a serious mechanical obstacle to the act of breathing. Like the membrane of croup, these concretions, which have received the fanciful name of "bronchial polypi," are very quickly regenerated, so that many may be thrown up in rapid succession, and at short intervals. Dr. Paris informs me, that a patient of his expectorated, in a short time, enough of these concretions to cover a plate.

Although we find but few cases of bronchial "polypus" on record, it is probable that the number would be much increased if we were more in the habit of minutely examining the state of the bronchial tubes after death; and that, as Andral suggests, many cases of asthma, which are supposed to depend entirely upon some ill-defined disturbance of the nervous system, would be found to result from bronchial obstruction.

Dr. Cheyne* directs our attention to another kind of "polypous" concretion of the bronchial tubes, which occurs only in connexion with hæmoptysis. The distinction between the two is important with respect to the prognosis. This last variety consists merely of the coagulum of the blood, moulded into shape by the bronchial tubes, into which the blood has been poured. The formation of the coagulum checks the attack of hæmoptysis; but the relief to the patient is but temporary. Cough and difficulty of breathing speedily ensue from the presence of the coagulum in the air-tubes; the coagulum is expelled; the hæmorrhage returns, and not unfrequently destroys the patient at once, or he dies of pulmonary consumption. Cases of this kind are related by Mr. Moyle†, Dr. Samber‡, Le Bœuf§, &c. On the contrary, when these concretions are voided in chronic affections of the bronchi, the great probability is that the patient will recover, however numerous they may be. It has happened in more than one case, that the unusual appearance of these polypi has led to the suspicion of pulmonary disease, and that hence an erroneous opinion has been given.

But although the formation of these concretions does not alone indicate organic disease in the lungs, the patient is often exposed to some degree of danger from another source. The "polypous" substances may adhere so firmly to the bronchial membrane, that however violent may be the paroxysms of coughing, they are not expectorated, and suffocation is the consequence. Two instances of the kind are related by Andral‖, in both of which the patients died from suffocation. Upon dissection a large branch of the bronchia was found completely blocked up by "une masse de mucus concret, demi-solide, qui fermait comme un bouchon ce conduit membraneux, et se prolongeait en s'amincissant dans son intérieur." This description corresponds exactly with the

* We are equally unable to determine with any degree of certainty the altered action that leads to the membranous effusion in tubular diarrhœa, or on the mucous surface of the uterus in dysmenorrhœa.

* Edin. Med. and Surg. Journal, vol. vi.
† Lond. Med. Journal, vol. vi.; p. 252. 1785.
‡ Phil. Trans. No. 398, p. 262.
§ Mém. de l'Acad. Royale de Chirurgie, t. xiv. p. 449.
‖ Clinique Médicale, part. 2, p. 41, *et seq.*

appearance of the preparations on the table.

I abstain from entering further into the treatment of such cases than to observe, that the inference to be derived from the rise and progress of the majority of the cases on record, would appear to shew that free bleeding is not necessarily indicated. To assist the efforts of nature in throwing off the concretions, small doses of tartar emetic, or ipecacuanha and squills, have been found useful. M. Raickem speaks very highly of the good effects of repeated doses of sulphate of potash. In chronic cases, where the disposition to their formation long continues, I should think it would be proper to endeavour to remove the morbid action, by rubbing in mercurial ointment upon the throat, so as slightly to affect the gums. To alleviate the distressing dyspnœa, the violent cough, and the threatening suffocation that occur, when nature is making violent but ineffectual efforts to remove the too firmly adhering concretions from the air-tubes, warm inhalations of æther and water, and the internal administration of a diffusible stimulus, as Hoffman's anodyne, would be probably more effectual and safer than blood-letting, which some have advised; for all the power of the patient is required to get rid of the mechanical obstacle to the act of respiration.

CASE OF

EPILEPSY ALLEGED TO HAVE BEEN CURED BY MESMERISM.

[We have thought it right to answer the appeal of M. du Potet by inserting the case which follows; at the same time we must take leave to say that the statements it contains go so far beyond verisimilitude, that unless supported by competent and disinterested witnesses, the general impression, we suspect, will be, that the Baron has been deceived.—Ed. Gaz.]

A Monsieur le Rédacteur de la Gazette Médicale.

Monsieur,

Vous avez eu déjà la bonté d'insérer dans votre estimable journal plusieurs articles dans lesquels je défendais la découverte de Mesmer.

Aujourd'hui j'espère que vous ne me refuserez pas la même faveur pour une fait de guérison produit par le magnétisme. Ce sera, monsieur le rédacteur, une nouvelle preuve de votre impartialité.—J'ai l'honneur d'être

Votre très-humble serviteur,

Le Baron du Potet Sennevoy.

Londres, 11 Mai, 1838.

Lucy Clarke, eighteen years of age, has been affected with epilepsy for several years, and the fits had latterly been very frequent. A seton in the nape of the neck had suspended them for some time, but they subsequently returned, and all remedies proved ineffectual.

This was one of the first patients in England to whom I applied a regular magnetic treatment. She was brought to the North London (University College) Hospital, by Mr. George Denton, a young medical student, who had himself already magnetised her with effect.

I continued the experiments publicly, in presence of a great number of persons, and the following phenomena were observed.

On employing longitudinal "passes" with more or less rapidity, and standing in front of her, the eyelids closed at the expiration of a few minutes. In vain did the patient rub them; an influence stronger than her will kept them closed. Very shortly, the movements which betrayed the resistance she opposed to the magnetic influence became more languid and less frequent, and profound sleep supervened. First, her head, then her whole body, obeyed the law of gravity, and tended towards the ground. It was useless to endeavour to keep her either standing or sitting, or to support her in an arm-chair; it seemed indispensable that she should lie at full length. In that position, strongly shaking her, pulling her hair, pricking her with pins, caused no sensation of pain; sharp cries in her ears did not awaken the sense of hearing, the jaws did not yield to the tractions employed to unlock them. When the eye-lids were raised up, the pupils were not visible. It was only by magnetism that she was again brought out of this singular state.

Magnetised three times a week, the same effects were manifested until the eighth time. Some changes then took place; I could from this time make

her sit up, and succeeded in making her speak. Insensibility to pain, however, continued throughout the whole treatment, but this was not the case with the sense of hearing, which existed with respect to sounds universally, (and not only in connexion with myself) after I had touched her ears with the intention of destroying the kind of paralysis which magnetism had occasioned. The somnambulism thus developed exhibited no very remarkable phenomena. With respect to lucidity her perceptions were limited, which may, perhaps, be attributed to too many questions having been put to her, and to premature experiments. She several times, however, distinguished the colour of objects applied to the epigastrium, and told the hour indicated by a watch, the hands of which had been purposely deranged. But this slight manifestation of lucidity was presented to so many incredulous persons, and the suspicions excited were so numerous, and so insulting, that I checked, as far as lay in my power, all such trials, as success did but augment the incredulity of the spectators.

There are epochs when truths seem to come prematurely. It will one day, no doubt, be matter of surprise that a natural faculty should have excited such passions, and that persons should have been found believing that they had ascertained the limits of possibility. Under such circumstances, however, I thought it would be advisable, afterwards, to aim merely at producing physical phenomena, such as might be more easily comprehended, and would admit of explanation, and soon fresh and very surprising ones occurred, which I may on some future occasion lay before the public. At present, I shall continue my narrative more especially with a reference to her complaint, of which, in her somnambulic state, she described the symptoms, and gave the history of their invasion; and she also foretold that her perfect recovery would take place on the 30th November, asserting that she should have no more fits, and should suffer only from general debility, attended by pains in the region of the heart; that she should remain in that state for some months, and afterwards entirely regain her health. An accident, however, occurred. which for a time counteracted her predictions. A jar full of sand fell upon her head, and the blow was so violent that she lay for a long time insensible. In this state she was magnetised by Mr. Pereira, a medical student, but delirium came on, which lasted three days. She was brought to me while in that condition, and I again magnetized her. I succeeded in producing a slight degree of lucidity, and she prescribed a copious bleeding in the arm. This was performed during her sleep, without her being sensible to the prick of the lancet; she only gave notice when a sufficient quantity of blood was taken. The delirium ceased at the time she had announced it would, but when again thrown into sleep, she foretold its return unless she should be again bled. This was done notwithstanding her weak state, and it answered perfectly. This second bleeding was also performed during her somnambulism, without her making the least movement as though she felt the lancet. On another occasion she suggested the removal of the seton from her neck, affirming that it would be for the future quite useless. An excrescence which had grown up at the border of one of the apertures, was removed at her own request. The excision was made by Mr. Perkins. She did not appear to feel the operation, and asked, some minutes afterwards, if they were going to cut off that inconvenient piece of flesh. When awakened, as was the case in all her magnetic sleeps, she retained no recollection of anything said or done while in that state.

I have of late magnetised her once a week, being unwilling to give up the treatment suddenly. This precaution is, I think, generally advisable. As I believe that her cure is complete, I have published this new fact for the interest of science, and I hope it will be the means of inducing further investigations.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Works of John Hunter, F.R.S.: with Notes. Edited by J. F. PALMER. 4 vols. 8vo. Illustrated by a volume of Plates, 4to. London: Longman and Co.

THIS work, which we propose to notice somewhat more fully than we have

hitherto been able to do, is admitted to be of the highest order. The philosophic spirit which pervades every page, and emanates from each casual observation or incidental fact, reminds us of no similar production in the circle of medical literature—recals to our memory no author of the same note—and suggests no compeer who enjoys the same unbounded extent of intellectual horizon. Laennec, the genius of a later day, is excellent in his researches into a particular set of diseases, but he does not attempt to carry out his principles of investigation into large and general theorems of science. Few physiologists have advanced, armed like Hunter, and striddeu across the fields of science; few, like him, have sought to ascend from partial to general laws, and have not tarried over what was local and transitory, instead of attaining to what was permanent and universal. Cuvier is the only name that meets our view in this survey. The host of preparations along the walls of the College of Surgeons is marvellous, not as the specimens of a dexterous dissector, but as the evidences of profound thought, and the fabrications of a peculiar and very extraordinary mind.

With this feeling of just veneration, we hailed the first announcement of Hunter's works being about to appear in a collected form, and we expected with eagerness the day when they should issue from the press.

The first volume was published as far back as the year 1835, and conveyed to us much that was novel. The Surgical Lectures alone were sufficient to fix us long in our chair, and our pains were amply compensated by the perusal of the very words in which Hunter had instructed his class. They breathe the same spirit as the most masterly of his works, the Treatise on Gunshot Wounds, and they betray the same faults, namely, those of a mind too eager in the quest of its quarry. In these Lectures especially, Hunter sometimes struggles to express thoughts which he could not form into words, and sometimes seems to form thoughts which no one could express. This erratic playfulness of a prolific intellect is reduced within the bounds of sound sense, by notes at the foot of the page; and the text of the author is rectified whenever it is illogical and inconclusive, and brought up to the knowledge of the day whenever it is imperfect or obsolete. By this assistance, the mind of the reader is not allowed to fall into vague speculation, and the inexperienced are prevented from receiving as true those positions which subsequent science has controverted or disproved.

Of the many books which we have had occasion to review since the first publication of this journal, we have opened none that build upon a principle of life as their foundation of thought. The metaphysician and the logician may state their arguments of confutation, and shew that the doctrine of "life," "principle of life," and "vital action," are but the deductions of an *à priori* reasoning; but no argument, however convincing, can overthrow the common instinct of our nature; and no one dare be so audacious as to assert that the mechanism, and the power that moves the mechanism, are the one and the same. We are not to regard the body as a machine, and the physician as a mechanist. Life is a versatile power, manifested we know not why, and abolished we know not how; varying in the intensity of its manifestations in every possible form of combination, and every possible degree, and type, and mode of development, throughout youth, health, age, sickness, renovation, and decay. But to man, independent of this vital principle, which, like the principle of magnetism or gravitation, is a causation cognisable only in its effects, is superadded the high moral principle, the will, the soul, the spirit, by which we communicate with man—and man with God. This view of life brings a different train of thoughts to bear upon the question. In this light the physician is a moralist as much as the divine; and he feels that he exercises over the diseases of his fellow beings those means only which have been appointed to his hands by a higher power.

These ennobling thoughts have taken their rise from these Lectures, which, with every fault and every excellence (the very attributes which prove their truth and authenticity), have been presented to us by Mr. Palmer, who has displayed sincerity and care in their collation. In Mr. Palmer's preface, it is curious to remark with what chance the words of a distinguished man have been preserved, and how certain it is

that words worthy of preservation are not eventually lost; for Hunter seems to have been careless or procrastinating concerning these lectures, the fate of which depended on the diligence of nine gentlemen, and have been given to the public by a tenth, Mr. Palmer himself, forty-nine years after they were delivered!

The rescuing of these lectures from that oblivion which they must needs have fallen into in private hands, alone constitutes the editor of Hunter's works a benefactor to the student and the scholar. To ourselves, devoted as we have been to the writings of this medical philosopher, they bear the highest value; and they may have strengthened the conviction which we have formed during the experience of several years, that Hunter's works (his writings and preparations) are not yet sufficiently known, studied, and understood. In the practice of our profession, we are chiefly routineers; the velocity of modern society hinders us from meditation. We require to be brought back to first principles, and our depraved appetites withdrawn from the garbage of ephemeral systems, popular treatises, and specific remedies. Mr. Palmer, therefore, deserves great praise for having executed a task which forms a prominent feature in the medical literature of this country. Who would not possess and read those lessons which have inspired and elevated the talents of the most distinguished surgeons of our day? Among our professional stores they are invaluable; and though they may be too ponderous to be appended to the packs of students under the forced marches of their present curriculum, and the daily drill for their final examinations, yet, in private and alone, they offer to every cultivated mind that profound stillness of thought and exuberance of productive energy, which we have been accustomed to look for only among the writers of early Greece.

There is an instructive Life of the author, enriched from every point of information, at the commencement of this volume. But it has the fault, common to biography, (from the life of Sir Humphry Davy to that of Sir Walter Scott,) of being too circumstantial. "We spend our years," says the Psalmist, "as a tale that is told;"—a tale soon told, and soon forgotten. The memoir of a life ought to be so brief, that it may be read off at a sitting, and yet so ample, that the image, like the figure of an excellent portrait, may stand out bold, clear, and distinct, on a dark aerial canvass. Every lesser mark and discrimination in the detail of character should be made to fall into the shade, and serve to fill up the embellishments of the background. A consummate artist never blemishes his brush with too sharp a minuteness of outline; and it is this graphic brevity of style that imparts the deep pathos to the rapid history of the kings of Judah.

These critical remarks, however, have been thrown out with no intention to asperse the narrative of Mr. Drewry Ottley, which really is very clever.

A chronological history of Hunter's writings, an account of the editions of his works, and his evidence in the case of Sir T. Boughton, complete the volume. As for ourselves, we thank Mr. Palmer for having edited so ably a work in strict accordance to the best principles of medicine; and we envy him the title of having his name connected with John Hunter in a manner which, in our younger days, we ourselves aspired to.

The two treatises contained in the second volume are, the first on the Natural History and Diseases of the Teeth; and the second on the Venereal Disease.

The treatise on the teeth is edited by Mr. Bell, a gentleman accomplished in his art. Mr. Bell has studied his subject with the greatest minuteness and care; and in appropriate notes at the foot of the page, corrects the author with the air of a gentleman, and the accuracy of a man of science. The matter contained in these short notes forms an ample scholium to the text; and, without aiming at the slightest display of learning, they at the same time exhibit a ready knowledge on every point, and an extensive information both of comparative anatomy and pathology. With his natural fervour of mind, Hunter outruns himself on several important subjects;—he denies and admits the vitality of the teeth, advocates their transplantation, advises the premature extraction of the deciduous set, and, from the shape of the cuspidate, is very nigh inferring that man might be a predaceous animal. It is amusing to see with what address the editor pursues the great leviathan of surgery through-

out these pardonable eccentricities of thought, and with what ease and skill he reduces the whole treatise into a correct, a useful, and even an elegant form. The wanderings of Hunter, however, are never those of stupidity and weakness; for after each departure from the line of truth, he returns with fresh subjects of analogy, and enlarged views of disease.

The many low publications of the day, which, like fungi about the stem of a great tree, grow under the shadow of its branches, derive what excellence they may have from works like this; but, unlike this work, evolve no new truths, and adorn no part of science. Hunter's Treatise on the Teeth, like his other productions, involves in its consideration the leading principles of disease, and remains, with some corrections and elucidations from the pen of its able editor, an interesting production in itself, and a master-piece on the subject of which it treats. We cannot but regard this treatise as a very highly finished portion of Mr. Palmer's edition.

There is one point of view in which neither the author nor the editor of this Treatise seems to have regarded the teeth. The decay of the teeth, provided that it be not the result of immediate accident, is the index of a proportionate diminished vitality throughout the animal. The vitality of the teeth (a question no longer debatable) may be directly destroyed by violence, chemical agency, and the thoughtless exposure of them to unusual degrees of heat or cold. But their structure alone is able to resist the shock of many external agents, and their vitality, like the vitality of the whole body, is capable of resisting, for a length of time, many of the direct causes conducing to their death. The tendency of every vital function is to antagonize dissolution, and the teeth are equally endowed with this same vital existence. But the causes of the death of the teeth are to be sought for in the energy or weakness with which the assimilative process is carried on; for the closest correspondence is maintained between the functions of the stomach and the mouth. The teeth form an apparatus attached to the line of the mucous surface of the alimentary canal; and it is proverbial, that in infants the digestive power of the stomach advances with the rising and growth of the teeth; and that, though the stomach of the infant may receive, yet it will not make use of fibrous food until the jaws are completely furnished with the first set. Many ailments in the nursery are to be imputed to the ignorance or disregard of this physiological fact, and the remedies for those ailments will be supplied by the simple recognition of the fact itself. In subsequent life, when the permanent teeth are formed and set, the same correspondence is maintained between the stomach and the mouth; and the permanent teeth remain or fall out in exact proportion to the greater or less vitality of the stomach. Whatever course of life or kind of diet weakens the energy of digestion, renders the stomach less fitted for receiving hard fibrous food; and, accordingly, the teeth are less needed when the food requiring mastication is not so requisite. Cases of indigestion may be relieved or cured by attending to this indication of the teeth; and in obstinate dyspepsia, we never omit to examine the mouth and manducatory apparatus. With enfeebled digestion, there is enfeebled animal power, and a less vigorous development of organic life: in this point of view, the teeth are indexes of the quantity of vital force. Old age, instinctively warned by the dropping teeth, returns to the aliment of a child; and, as our years multiply towards their close, the solid gives place to the pultaceous, the succulent, and the liquid nourishment. According to our apprehension, decayed teeth indicate, not merely that the food is imperfectly masticated, but that the stomach is less prepared for food requiring mastication.

In the second part, that on the Venereal Disease, Mr. Babington, the editor, has had a difficult subject to treat. We should have been reluctant to attempt the elucidation of a subject in which the author himself does not shine; for if the whole of Hunter's Essay on the Venereal Poison were broken up, most of his generalizations would be found to belong to his other works, and his descriptions of the disease itself to consist of solitary insulated facts, which still require the master-hand to string them into a whole. A concise history of syphilis is still wanting. Detached essays, and occasional observations, are abundant enough; even the most elaborate treatise on this subject that we have met with has not offered all that we would desire. The ground is still open; and

a man of science, endued with a turn for clinical observation as well as with a philosophic spirit of viewing life as a whole, has before him the opportunity of carving out for himself a name, from materials which no one has hitherto handled with success.

With these difficulties to contend against, the intelligent editor of this treatise has nevertheless done much. The notes, in illustration of the text, contain a summary of our present knowledge on the subject: the manner in which these notes are constructed is at once clever and perspicuous; and the modes of treatment prescribed, spring from a right apprehension of the disease. We would recommend to the reader the note on the primary venereal sore: the note itself is an essay, in every word of which we fully concur. It omits to notice, however, the superficial sore, without any hardened base, either preceding or following the ulceration. This ulcer is superficial, flat, oval, with a red margin: there is no hardness. We have disregarded these ulcers ourselves, healed them by local applications, and seen the train of secondary symptoms set in at the end of eight or twelve weeks. Likewise, the diffused excoriation of the glans and prepuce is far from innocent. All ulcers which arise within four or eight days after illicit commerce, are suspicious; and the practised surgeon will become more tenacious of watching them all, the older he grows. The hardened base even without ulceration, is, however, the true form.

Our space will not permit us to make further references to these very useful practical notes. We must refer the reader desirous of information to the work itself, and reserve our own observations on those interesting points for a larger space than is afforded us by a weekly journal. We will conclude by remarking, as the result of our own experience, that more evil has been perpetrated by the non-mercurial or demi-mercurial treatment, than by all the chances of injuring the health by too large a dose of the remedy, under the hands of the strictest mercurialist. Mercury, indeed, is not infallible—for what in this world is so?—but it approaches as near to infallibility as any thing earthly can.

"The stream of time," says Dr. Johnson, "which is continually washing away the fabrics of meaner poets, passes without injury the adamant of Shakspeare." The same may be said with equal propriety of Hunter's works, especially of the two now under notice; which have stood secure and firm, while many a popular treatise of the same day has long since floated down the stream and been forgotten.

Medical Portrait Gallery, to be continued monthly; each Part containing Three Portraits, Biographical Memoirs of the most celebrated Physicians, Surgeons, &c. who have contributed to the advancement of Medical Science. By Thomas Joseph Pettigrew, F.R.S. &c. Fisher and Son. Parts 1, 2, and 3.

A collection of well-executed portraits of our professional brethren would certainly constitute a very interesting work, and we therefore sincerely wish success to the one before us; at the same time there are various circumstances which render the execution extremely difficult as regards the biographical department. At any time, to write the history of those yet living is rather a delicate undertaking; but when this is to be accompanied by engravings, which have almost uniformly to be executed from original paintings in the possession of the party the picture of whose mind as well as person is to be given, impartiality would amount to a positive virtue, and one perhaps too great for frail humanity to attain. In this respect, certainly, Mr. Pettigrew has not been able to rid himself of our common frailty, for his biographies of the living are written in the most eulogistic strain, and such as requires, and will no doubt receive, a good deal of pruning, should the work pass through a second edition after the living subjects of these memoirs are no more. Among the deceased worthies we have Æsculapius, Alpinus, Ruysch, Haller, Linacre, and Akenside; among the living are Sir Henry Halford, Sir Anthony Carlisle, and Sir Charles Clarke. Of the former, almost all are exceedingly good engravings; of the latter, the first and last are striking likenesses, but Sir Anthony Carlisle totally unlike any appearance that gentleman has presented during the last twenty years. What he may have been before that time we cannot say; but if he resembled the por-

trait he must have been decently dressed, and how far this might alter his appearance we are unable to say.

We must add, in fairness to the publishers, that three shillings a number is a very moderate price, considering the style both of the engravings and the letter-press.

Velpeau's Anatomy of Regions. Translated form the French, by HENRY HANCOCK, Lecturer on Practical and Surgical Anatomy at the Westminster Hospital School of Medicine, &c. London, 1838.

WE have abundance of works which treat of general anatomy, but one in which the view should be confined to the different regions was still wanting—or, at least, there was ample room for such a volume, if well executed. The one which lies before us is made by translating a portion only of Velpeau's work, omitting that which treats of general anatomy. By this means the part most wanted is brought within a reasonable compass, and the translation is executed in such a manner as to do full justice to the original. We have no doubt, when it comes to be known, that "Velpeau, by Hancock," will be a popular work with students of anatomy.

MEDICAL GAZETTE.

Saturday, May 12, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE NEW FRENCH PHARMACOPŒIA.

IN comparing the French Codex with the London Pharmacopœia, the chief object of inquiry with most persons would naturally be the points in which they differ; but, perhaps, as a guide to practice, it would be more instructive to ascertain the points in which they agree. When an English book becomes popular in France or Germany, we conclude, with reason, that its merits must be great, to resist the lowering process of translation, and, in spite of the difference of instinctive or acquired feelings, to satisfy the *literati* of Paris and Berlin; and, in like manner, it is no slight proof of the merits of a remedy, when we find the physicians of distant capitals, however differing in theory, all employing it in practice. Thus, Dover's powder, perhaps the best of all the preparations of opium, next to the tincture, is the most generally diffused. In the French Codex it contains nitre and liquorice root, in addition to the ingredients ordered in the London Pharmacopœia; and the proportion of opium is 1-11th instead of 1-10th*. The work from which the formula for this useful powder was originally taken, might be cited as a topic of consolation to those timid authors who are afraid that the valuable discoveries contained in their works will be lost to posterity, unless those works are stoutly bepraised by reviews, magazines, and other laudatory engines. Let them be comforted; their books may be neglected, but their discoveries, if any, will be preserved.

If there is any gold *in stercore Ennii*, it is sure to be picked out. Few books are more deservedly forgotten than "The Ancient Physician's Legacy to his Country," though the result, as the author affirms, of forty years' practice; while his powder *volitat vivu' per ora virûm.*

On comparing the long lists of tinctures in the French Codex and ours, we find in both, those of aloes, assafœtida, balsam of tolu, cinnamon, cantharides, cascarilla, castor, catechu, cinchona, conium, colchicum, digitalis, ginger, guaiacum, black hellebore, hyoscyamus, jalap, myrrh, opium, squill,

* Dr. A. T. Thomson says, that the proportion in the Swedish Pharmacopœia is only 1-60th; but in the same table he states the proportion of the London and Edinburgh Pharmacopœias at 1-9th instead of 1-10th. (*Dispensatory*, Ninth Edit., p. 1033.)

and valerian. In others there is a near resemblance. Thus, instead of our compound tincture of gentian, they have a simple, and also an ammoniacal one; a tincture of iodine in place of the London Tr. Iodin. Comp.; a simple tincture of rhubarb instead of the London compound tincture; and a simple, instead of a compound, tincture of senna.

The *Teinture Balsamique*, alias *Baume du Commandeur de Permes*, alias *Tinctura Balsamica*, resembles the London *Tr. Benzoini Comp.*; the former, however, has olibanum instead of storax, and contains, in addition to the other ingredients, angelica root and hypericum flowers. There are two tinctures of camphor, the one containing ℥j. to ℥vij., and the other ℥j. to ℥ xl. of spirit. In the London Pharm. the proportion is 1 to 8.

The *Teinture d'Opium Ammoniacale, Elixir Parégorique*, or *Tinctura Ammoniacalis cum Opio*, differs considerably from our paregoric elixir, or *Tr. Camphoræ Comp.*; as it contains no camphor, a large quantity of ammonia, a much larger proportion of opium, and some saffron.

The tinctures of wormwood, aconite, ambergris, asarum, elecampane, belladonna, contrajerva, euphorbium, cloves, gum ammoniacum, gamboge, lactuca virosa, mastich, musk, nux vomica, pyrethrum, quassia, rhus toxicodendron, stramonium, amber, turpentine, and vanilla, have nothing corresponding to them in the London Pharmacopœia. The *Teinture de Raifort Composée* (*teinture antiscorbutique*, or *tinctura cum cochleariâ composita*) resembles the *Spir. Armoraciæ Comp.*; the ammoniacal vegetables being reinforced with sal-ammoniac. The *Tinctura Aromatica*, and the *Tinctura aromatica dicta vulneraria*, bear but a slight resemblance to our *Tr. Cardamomi Comp.* and *Tr. Lavendulæ Comp.* The *Teinture aromatique sulphurique*, or *Elixir vitriolique de Mynsicht*, has no longer any counterpart in the London Pharmacopœia. The *Teinture acétique d'Opium* is made with an ounce of opium, six ounces of strong vinegar, and four ounces of alcohol; so that it contains the soluble principles of one-tenth of its weight of opium. The framers of the Codex say, "*Un gros* [72 grains] *correspond sensiblement à 7 grains d'opium brut.*"

On the other hand, the London Pharmacopœia contains about 17 tinctures, which have no counterparts among those of the Parisian Codex. They are the Tr. Ammoniæ Comp., Tr. Aurant., Tr. Calumbæ, Capsici, Cardam., Cardam. C., Cinchonæ Comp., Cinnamomi Comp., Colchici Comp., Cubebæ, Gallæ, Guaiaci Comp., Kino, Lavendulæ Comp., Lupuli, Serpentariæ, Valerianæ Comp.; though even among among these, several of the compound tinctures may be considered as represented by the corresponding simple ones in the French work.

This comparison of the tinctures of the two codices is certainly favourable to the theory that the points in which men agree are more important than those in which they differ. At present, the two ends of the same island must needs make up their drugs differently; perhaps some happier age may see a European Pharmacopœia!

The Codex also contains a pretty long list of ethereal tinctures, of which those of assafœtida, balsam of Tolu, belladonna, cantharides, castor, conium, digitalis, and valerian, are directed to be kept by every druggist.

In our last article on this subject, we mentioned several new remedies contained in the French Codex, but not in ours. To these we may add urea, piperine, and the new or revived preparations—oxide of gold, oxide of gold prepared by means of tin, (or Cassius's

purple), chloride of gold, and chloride of gold and soda.

Urea, as we learn from Magendie *, has been administered as a diuretic. It is given in distilled water with sugar, and the dose to begin with should not be more than 25 or 30 grains, though several drachms have sometimes been taken.

Piperine has been used in ague, instead of quinine; and Magendie suggests that it might be substituted for cubebs in the treatment of gonorrhœa.

The salts of gold are used in syphilis, in much smaller doses than corrosive sublimate. According to M. Chrestien, the chloride of gold, given to the extent of only one-tenth of a grain a-day, occasioned in one case a violent fever.

One of the most striking features of the Parisian Codex is the number of niceties, or tit-bits, which it contains: thus, the syrups, which with us are only 14 in number, are carried by our Gallic brethren to the enormous number of 97! Not more than two or three of ours can be considered as active remedies, while among theirs we find syrups of acetate of morphia, of hydrocyanic acid, of belladonna, of digitalis, of hyoscyamus, of opium, of sulphate of morphia, of sulphate of quinine, of cinchona, and of cinchona with wine; besides a score fit to make their appearance on the table of the epicure, such as the syrups of lemon-peel, orange-peel, bitter orange-peel, and others made with the juices of lemons, oranges, barberries, cherries, pomegranates, currants, raspberries, mulberries, apples, and one of raspberry vinegar.

Who is there that does not recognize in these luscious remedies the amenity for which our neighbours have always been celebrated, and their constant care to gild the pill of life, and make even medicine desirable? Whether the attempt to sweeten bitters can always succeed, is another question. It may be doubted, for instance, if the *syrupus cum summitatibus absinthii* would make wormwood go down much more pleasantly, than the device which, Lucretius tells us, was practised with children in his days. The edges of the cup containing the wormwood were plastered with honey, and the young patient was thus tricked into taking his physic.

> Sed veluti pueris absinthia tetra medentes
> Cùm dare conantur, priùs oras pocula circum
> Contingunt mellis dulci flavoque liquore,
> Ut puerorum ætas improvida ludificetur
> Labrorum tenus, interea perpotet amarum
> Absinthî laticem, &c. Lib. i. 935.

The former edition of the Codex contained a syrup of hydrocyanic acid made with nine parts of sugar to one of the medicinal acid; on which Magendie observes, that such a preparation can be administered by drops only, and that if as much as we give of other syrups were mixed with a potion, a deadly beverage would be the result. (Formulaire, 8e. édit. p. 183). It was in consequence of this syrup being administered instead of the weaker one intended by the physician, that several epileptic patients were destroyed at Paris a few years ago, as was narrated at the time in the Medical Gazette. The present formula is the same as the one recommended by Magendie, namely, a drachm of the medicinal acid to a pound of syrup.

The *tablettes* and *pastilles*, or lozenges, are another large attempt to make medicine agreeable, and prevent its being thrown to the dogs. They are twenty-eight in number, nine of which bear the star indicating that they must be kept by every druggist. Among these latter are the lozenges of bicarbonate of soda, ipecacuanha, kermes mineral, magnesia, calomel, and sulphur.

The Edinburgh Pharmacopœia contains lozenges, but they are only six in number. Perhaps the most solid objection to the introduction of these phar-

* Formulaire des Nouveaux Médicaments, 8e. édit. p. 337.

maceutical delicacies into our islands is, that we are already a very physic-taking people; and if remedies were made too captivating, there is no knowing how far our morbid appetite for drugs might be sharpened. The alderman, not content with taking his dinner pills before the repast, might serve up the *syrupus rhei* or *ipecacuanhæ* after it; and lozenges of soda or magnesia might become as necessary an accompaniment to champagne, as curry to rice.

The Codex contains six kinds of chocolate, but none of them honoured with the compulsory star.

Besides a considerable number of medicated wines and vinegars*, there is an antiscorbutic beer, made by macerating scurvy grass, horse-radish, and fir buds, in new beer.

We might here mention the formulæ for Seltzer, Seidlitz, Spa, and other mineral waters, a capital tooth-powder, and the various *pommades*, some elegant, and others active, but it is time to conclude this article. We strongly recommend our readers, especially our younger ones, to consult the original work; it will exceedingly enlarge their stores of pharmaceutical knowledge, and be equivalent to a whole series of lectures on the art of prescribing.

DR. GRIFFIN'S PAPERS ON CHOLERA.

We this week insert the concluding portion of Dr. Griffin's papers on Cholera; and although it be not customary for us to allude to the papers which we publish, yet we beg earnestly to direct attention to the series of essays in question, constituting, as in our humble judgment it does, the most complete and satisfactory analysis extant of all that has been ascertained regarding this formidable malady.

* The *vinaigre des quatre voleurs* is otherwise called *vinaigre anti-septique;* but in our edition this last word is curiously misprinted *anti-sceptique*—a vinegar of which, according to some sour critics, there is an especial need in France. This vinegar of the four thieves is distinguished from English aromatic vinegar, for which a receipt is also given.

COLLEGE OF PHYSICIANS.

May 7, 1838.

The following paper in addition to that by Mr. North, inserted at page 330, was read, viz.:—

On the Preparation of certain Articles of the Materia Medica; with a Detail of the Processes employed. By Mr. Battley.

My attention having been directed during the earlier part of my life to the practice, and during a long period of time to the preparation, of medicine, I beg your permission to submit to you the following observations, in the hope that they may not be without effect in the treatment of disease.

The materia medica is presented to the physician by the hand of the pharmaceutical chemist, both in its natural and its artificial state.

The natural takes precedence of the artificial, being the medicine which a merciful Providence gives us to heal our sickness; and to obtain which manipulation need only be employed, as I conceive, to separate the remedial agents from their various admixtures.

The artificial state is the work of chemistry, as usually now conducted, proceeding by effecting the separation of the base of the medicine from the combinations in which it is naturally held.

Far from adopting this view of the subject, it has been my endeavour, generally speaking, to maintain the combination which nature has formed, and, with few exceptions, when separation becomes necessary, to effect the recombination of the separated matter.

At the outset of modern chemistry, the base of a thing so essential to life as atmospheric air was seized upon with avidity; but in its application as a remedy for disease, vital air has not been found to be even a substitute, much less an improvement, on pure atmospheric air, as it is given us in its just proportions by the Divine Artificer.

In the decomposition of the various articles of the materia medica, art has succeeded to a certain extent with some of the remedies, of which morphine and qui-

nine may be referred to by way of illustration; but it remains to be proved whether or no these bases of opium and bark, even when re-combined with acids, be equally efficacious as when administered in their native combination, purified and concentrated only, as in the case of bark; or more freely divided, as in the case of opium.

Without impugning, therefore, the science of chemistry in multiplying the number of potent poisons by these separations, some of which cannot stand their ground from the impossibility of adjusting them to a standard sufficiently uniform, or if adjusted, of controlling their action on the living body with a due regard to its safety, I have arrived at the conclusion that the desideratum in pharmaceutical chemistry is—

To leave unchanged the medicinal quality of the substance acted upon, separating and removing therefrom, as far as may be possible, every matter not possessing remedial virtue; and, I may further premise, that I am thoroughly impressed with the belief that the active medicinal virtues are, in their natural state, generally, if not always, held in solution by acids. To maintain this combination undisturbed should be the chief aim and end of the pharmaceutical chemist.

The principal agent applied, by me, in effecting these important separations is distilled water, generally cold; and I proceed to submit to the attention of the President and Royal College, the details of the processes by which some of the various forms of medicine, which I have now the honour to submit to their inspection, have been procured.

Bark.—Fine sub-pulverized yellow bark, having been macerated in cold water, yielded a transparent liquid of an amber colour, very fragrant, and extremely bitter, sp. gr. 1009; a free acid being indicated by litmus paper. By the condensation of this fluid, at a low temperature (130), to the consistence of syrup, a dark and transparent mass resulted, possessing, in the highest degree, all the characters of the cold infusion.

The analysis of this fluid mass gave the following results; viz.—

Tannin, quinine, resin, bitter extractive, colouring matter, wax, lime, held in solution by the vegetable acid peculiar to bark, and yielding aroma in a high degree, the acid also existing in the infusion in a free state; together with muriate of potash and sulphate of potash.

This first maceration appears to me to have the effect of separating and securing nearly the whole of the medicinal properties of the bark, since, by the second maceration, lime in union with some of the acid, a little colouring matter and the other properties of the bark, in very minute degree, were the only results, and on boiling the residuum in water, little was obtained besides starch.

It is sufficiently evident, then, that unless the starch contain medicinal virtue, the first maceration leaves little besides a portion of the vegetable acid, to which that property can be attributed; and with regard to this acid, sufficient is procured to hold in solution the quinine, resin, extractive, colouring matter and wax, together with the tannin; and the importance attaching to this process may be considered as fully established when I add, that the acid is a ready solvent of quinine, even without the assistance of heat, the usual solvent being dilute sulphuric acid at the boiling point; and that the bitter procured by the first is far more intense than that produced by the latter, one being the compound prepared in the great laboratory of nature—the other, artificial.

I have, for some time past, received repeated assurances from high medical authority, of the superiority of the medicine prepared by cold infusion, and subsequent condensation; but I have only recently ascertained the high solvent power of this acid.

Sennæ.—To double its weight of cold distilled water, imparted nearly half its own weight. The analysis of this liquor was as follows, viz.:—

Gum, resin, extractive matter, lime, alumina, and magnesia, held in solution by a vegetable acid, and yielding aroma in a high degree; the acid also existing in a free state in the cold infusion; together with sulphate of potash and muriate of potash.

It has appeared to me, as the result of much observation, that the object to be desired in the preparation of senna, for medicinal purposes, is, to retain the whole of these properties in their native combination, and for this purpose the great tendency to separation and precipitation of that, which I apprehend to be the most effective property of the drug, viz. the resin, is particularly to be guarded against; for if that effect or consequence be allowed, the medicine ceases to be of value.

I find that these constituents are secured with the least disturbance, by this method of cold infusion; and that the medicine so procured may be made to preserve its efficacy for a great length of time.

Rhubarb requires to be treated in a similar manner to senna.

Jalap contains resin, extractive matter, starch, gum, alumina, lime, held in solution with its peculiar acid, the acid also

existing copiously in a free state; together with muriate of potash and sulphate of potash.

To extract the medicinal properties of this root, a high temperature is required, and may be safely used, beginning by cold infusion.

	Oz.
One pound troy yielded extract	6
To which was added, by boiling in Papin's digester	4
	Oz. 10

In the extract, the predominance of free acid is highly remarkable; nearly 2 oz. of alkali being required to neutralize the free acid from one pound of jalap.

Conium contains—

Free muriatic acid.
Resinous matter, combining *a highly volatile and very subtle principle.*
Essential oil.
Gum in great abundance.
Colouring matter.
Muriate of potash.
Sulphate of potash.

The active medicinal properties of this plant appear to reside chiefly in the subtle principle above mentioned.

In the Medical Gazette for July 1831, I stated, that in the ordinary manner of preparing the extract of conium from the expressed juice of the plant, this principle volatilizes, and escapes at the temperature of 120°; but I am now of an opinion that that is an extreme limit, and that volatilization commences at a still lower temperature. The great care and caution necessary, therefore, in the preparation of the medicine, must be obvious; and the admitted uncertainty of its effect under the hand of the physician, is explained by the simple fact, that a temperature much exceeding 120° is ordinarily employed in its preparation, and hence it too often proves to be powerless.

To secure the resinous matter without decomposing or disturbing the volatile principle, I have adopted the method of heating the juice to a temperature just sufficient to produce the separation of this matter (commencing at about 100°), which, being thus separated, is removed from time to time, as the separation proceeds. The liquor remaining, and in which muriatic acid is abundant, is evaporated to the consistence of thick syrup, the resinous matter is then admixed therewith, and thus the extract is formed; after which, it may be interesting to remark, that several weeks are required to effect the desired re-union of the resinous matter in the manner essential to the efficacy of the medicine, and which re-union I believe to be effected chiefly by the agency of the muriatic acid. However this may be, extract of conium, thus prepared, is a medicine not only of considerable power, but equally to be relied upon with any other medicine duly prepared. It is a medicine of great power, and is uniform in its operation.

Opium.—This secretion from the poppy head requires more attention than it is in my power at present to devote to the analytic explanation which its great importance, and the complexity of its details, require; but I cannot close this paper without observing that in separating the two antagonist principles which it contains, the stimulant and the sedative, I have, I believe, secured all the principles of the sedative power, in combination with the peculiar acid of the secretion by which they are held in solution, free from any admixture of the stimulant principles.

Remarks.

Bark yields all its medicinal properties to cold water, a moderate elevation of temperature only being used in its condensation.

Senna and *Rhubarb* yield their medicinal properties without the aid of heat.

Jalap, on the contrary, requires a very high temperature for the complete extraction of its medicinal virtues.

Conium.—The separations required for the purpose of my preparation of this medicine are effected at a low degree of heat; and I have stated, as the fact is, that the medicine, as thus prepared, is of great and uniform power, but it will, notwithstanding, be the object of my endeavour still further to improve this medicine.

The properties of the abovementioned, and other vegetable acids, will, in particular, be the object of my future attention; and I shall hope to be permitted to submit the results of those and other investigations to the consideration of the members of the Royal College.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

May 8, 1838.

On the Pulse. By James Hope, Esq., M.D., F.R.S., Assistant Physician to St. George's Hospital.

The object of the author was to shew that the pulse, although held by numerous authors, ancient and modern, to several of whom he referred, to be a most fallacious symptom, is, notwithstanding, a sign of considerable value and diagnostic precision, more especially as throwing light on the nature of organic diseases of the

heart. To the ignorance of most former writers on the subject of the action of the heart, Dr. Hope referred their doubts regarding or their denials of the utility of attention to the modifications of the pulse in diagnosis. After some prefatory observations, in which the author pointed out what he considered defective in the views of some of the more eminent recent pathological writers in France and England, he proceeded to shew—1. That the several more important organic diseases of the heart produce corresponding pulses. 2. That cardiac disease is capable of simulating all the varieties of pulse presented by other diseases in general; and that, consequently, in judging of other diseases, allowance must be made for any coexisting disease of the heart.

To prove the former position, the author stated that by means of various discoveries he had made relative to the action of the valves of the heart, and the consequences of organic change in those parts in disease, and of artificial suspension of the valvular play in physiological experiments, he was enabled to connect certain characters of pulse with particular valvular defects. His discovery of mitral regurgitation in 1825, and of the murmur indicating that disease, enabled him to ascertain that the corresponding pulse was, when the regurgitation was considerable, eminently weak, small, intermittent, irregular, and unequal; also, that in contraction of the mitral valve the pulse is small, feeble, and irregular—an observation made by the author in 1834 in the first instance, and since confirmed. The existence of aortal regurgitation, rendered probable by the facts just stated, was substantiated by the author in 1831; and in various physiological experiments in 1833-4, and 5, was further elucidated; and by those investigations the author ascertained that the pulse, indicating aortic regurgitation, is a pre-eminently jerking pulse, such as occurs after hæmorrhage in anæmia, &c.

The influence of semilunar valvular diseases on the pulse was further elucidated by the author's discoveries as to the cause of the second sound of the heart, namely, that it depended on tension after sudden closure of those valves during the diastole; and by means of the new views arising out of those discoveries, the author was enabled to devise a method of distinguishing the particular valve affected in organic diseases. He found that the diagnosis depended on examining at different points of the thorax, namely, over the pulmonary arteries, and on the left and to the right and over the aorta, in each case about two inches from the valves, when examining the semilunar valves; but that the interior or auricular valves should be examined near to the apex of the heart, a little to the right or left, according to the side of the heart to which the valves to be examined should belong.

The author further referred to his discoveries respecting the pitch or key of the murmurs accompanying the valvular action: these he found to vary principally with the depth of the source from which the sound proceeded. Murmurs attending the play of the pulmonary valves are more or less hissing or whizzing; and murmurs in the ascending aorta have the same character. Murmurs seated in the aortic orifice resemble the letter *R* in a whisper. Murmurs from regurgitation are lower toned, and more open. Those of the mitral orifice are hollow-toned, more so than those of the tricuspid, because more deeply seated. By attention to the marks and rules pointed out by him, Dr. Hope conceives that it will be comparatively easy for observers to satisfy themselves of the position that the more important diseases of the heart produce corresponding pulses.

The author then entered upon the second part of his paper; and, after enumerating 16 leading varieties of the pulse, proceeded to explain the characters belonging to each, shewing that though all producible by certain constitutional causes, which he points out under each head, they are each occasioned likewise by morbid affections of the heart. He then proceeds to shew in detail how the following varieties of pulse may be explained, as effects of cardiac disease:—

1. Large or full pulse.
2. Small pulse.
3. Strong pulse.
4. Weak pulse.
5. Hard pulse.
6. Soft pulse.
7. Prolonged pulse.
8. Quick or jerking pulse.
9. Slow or infrequent pulse.
10. Frequent jerking pulse.
11. Intermittent pulse.
12. Not intermitting pulse.
13. Irregular pulse.
14. Regular pulse.
15. Unequal pulse.
16. Equal pulse.

And some subordinate varieties, such as the bounding, the wiry, &c. &c.

The author then concludes his paper with some observations on the importance of being able to distinguish between abnormal pulses depending on constitutional causes, and those that depend on cardiac causes alone.

UNIVERSITY OF ERLANGEN.

MEDICAL DEGREES.

To the Editor of the Medical Gazette.

SIR,

THERE have appeared from time to time, in your journal, numerous allusions to

Degrees in Medicine, granted by the University of Erlangen, and these allusions have always been of a nature calculated to lower the possessors of these degrees in the estimation of the public. My object in addressing you is, to put you and the medical public in possession of the terms on which such degrees are granted, and to show that much misunderstanding prevails on this subject.

The respectability of any distinction, whether in medicine or otherwise, is, I take it, in an exact ratio with the amount of acquirement which it pre-supposes in the holder of it. And the objections to degrees obtained without personal application at the University granting them, proceed upon the supposition that such a University confers its highest honours without proper evidence that the candidate's acquirements entitle him to them.

Degrees conferred by diploma are, in themselves, far more honourable testimony to the merits of the recipient, than those obtained after an examination, just as an acquittal without calling on a prisoner for his defence, is more satisfactory than one wrung by starvation from a non-unanimous jury. It is, then, only upon the ground that such diplomas are conferred without the merit of the candidate being properly tested, that any objection can lie against them. Now, the only manner in which we can ascertain if proper care is taken, is by comparing the conditions required by the University of Erlangen with those exacted by the Universities in our own country, which license the great majority of our practitioners in medicine. These are Edinburgh and Glasgow; as St. Andrew's has not yet recovered from the really reckless manner in which its degrees used to be granted, and Aberdeen has not made a single Doctor of Medicine for some years, its conditions being much too high. Oxford, Cambridge, and Dublin, are, of course, completely out of the question, and their graduates look down as contemptuously on those of the Scottish Colleges, as these latter affect to do on those who have gone abroad for their honours.

I am not now going to speak of the impossibility of obtaining any degree in this country (except from St. Andrew's), whatever may have been the education of the candidate, without a residence of at least a year at the University granting it. This is ground which has often been travelled, and would lead to no satisfactory result. My intention is simply to lay before you the steps I took, and which I found it necessary to take, in order to obtain a degree from Erlangen. I speak of my own case alone; as I am not acquainted with any other gentleman who has received a similar degree, and can therefore only give my testimony as to what was required of me. I presume, however, that the same proofs of competency (or at least others equivalent to them) must be shewn by all candidates.

The common impression is, I know, that a degree can be obtained from Erlangen by any one who can produce a certificate from two or three medical men, and is willing to pay the fees. It is stated, too, that it is only necessary to transmit the certificate, together with the money, in order to receive the diploma by return of post. Let us see what truth there is in this.

First, I transmitted certificates of my qualification and attainments, signed by some of the most eminent men in the profession; secondly, a statement of the diplomas I already held, (and here I may remark that the candidate must be a member of one of the Surgical Colleges); thirdly, a history of my life, and an account of my professional education—written in Latin; fourthly, two Theses, one written in Latin, and the other in French, each occupying near a quire of foolscap. One Thesis, however, only is required. It must be sworn to be the unassisted composition of the writer, and the candidate must pledge his honour that the statements in his *curriculum vitæ* are strictly true; fifthly, I transmitted to the faculty copies of a published work of mine; and lastly, I referred them to articles which I had at various times contributed to the medical periodicals—your own journal amongst the number. The transmission of a work on some branch of medicine, of which the candidate is the author, renders it unnecessary to write a Thesis.

At the expiration of about seven weeks—and not by return of post—I received an announcement from the Dean that the faculty were satisfied with the Theses, &c. and that I should receive the diploma. I then paid the fees, and, in about two months, I received the diploma. Surely, sir, the time that elapsed, shews that no indecorous eagerness to clutch the fees exists, and warrants the presumption that the application of the candidate is not acceded to without due deliberation. The conditions, too, that the applicant shall be a member of a College of Surgeons, and the requiring one or more Theses, prove that the degree is not conferred, as used to be the case in the Scotch Universities, on any one applying, without reference to his previous education.

I would ask you, sir, how many Edinburgh graduates would, or could, write theses in Latin and French. The best answer to this is the fact, that since that

University has allowed the language of the theses to be English, not a single Latin one has been presented.

In respect to professional attainments, too, I do not think some of the Erlangen graduates need shrink from a comparison with those of Edinburgh or Glasgow. Many of these, it is well known, have proved so deficient, when under examination at Apothecaries' Hall, as to compel the Examiners to refuse them their certificate. Most of the Erlangen graduates, on the contrary, are already licentiates of the Worshipful Society. Many, too, have afterwards passed the London College of Physicians (their title of *Doctor* being there recognized), and have thus acquired a legal title to practice, whilst their competitors (notwithstanding their permission to practice "*ubique gentium*") incur a penalty of 5*l.* per month, for not possessing the license of the London College.

Few will, I think, be found to contend that a gentleman who has been examined at the College and Hall, and who is capable of writing the theses required by the Erlangen faculty, even should he not have passed the London College of Physicians, is not as well qualified to practise medicine as a raw Edinburgh graduate, who has been refused his license as an apothecary, or as a Glasgow doctor, who has undergone "one general examination in Anatomy, Physiology, Chemistry, Botany, Materia Medica and Pharmacy, and Practice of Physic," and whose classical attainments have been tested by "being made to translate some Latin author," *i. e.* precisely such an examination as is passed at Apothecaries' Hall. I think, too, that it will now be admitted, that the possession of an Erlangen diploma is a proof of something more than of having paid the sum of 20*l.* for the title of Doctor. Trusting to your impartiality for the insertion of this letter,—I am, sir,

Your very obedient servant,

M. D., Erlangæ.

May 8, 1838.

MR. JOHN GALE JONES.

[The following sketch is from the pen of a highly respected physician, who, as he states, was a fellow student of the somewhat eccentric individual whose history he briefly records;—a history, we may remark, not without a useful moral to the younger members of our profession.—Ed. Gazette.]

The writer of the following sketch of Mr. John Gale Jones, is not acquainted with the place of his birth, nor with his family connexions; but he remembers him as a fellow-student, attending the lectures of Dr. Baillie and Mr. Cruikshank, at the Theatre of Anatomy, in Great Windmill Street.

In 1794, Mr. Jones made his first appearance as an author, by publishing "Observations on the Tussis Convulsiva, or Hooping Cough; shewing that the practice of giving emetics is hurtful;" for which, it is believed, he received a prize medal, given by the Medical Society founded by Dr. George Fordyce and Mr. John Hunter, under the name of the Lyceum Medicum Londinense.

Mr. Jones was in the habit of attending the meetings of this Society, and spoke upon the subjects brought forward for discussion, with great eloquence, great lucidness of observation, and aptness of reply; and he was at this time in a position to acquire a very large share of medical knowledge, and the brightest prospects were opened to him of rising to a high rank among the practitioners of medicine.

Unfortunately, Gale Jones quitted the science of medicine to involve himself in the turmoils of politics. Jacobinism, as it was then called, the prototype of modern Radicalism, presented charms to his ardent mind, which enticed him from his medical studies and duties. He was now the foremost in attending political meetings; he addressed the populace from the hustings; he travelled as a propagandist of the political faith he had imbibed; and his pen was ever ready to defend the opinions he had embraced; but, alas! these exertions tended not to forward his interests; they only led to prosecutions and imprisonment. Arguments, it is true, were abundantly published, to prove that his prosecutions were undeserved, and his punishment unjust; but however convincing these arguments were to his friends, they produced no effect upon his opponents.

To discuss the qualifications of parliamentary candidates, to shew the necessity of reforms in church and state, to eulogize changes in the system of government, whether utopian or practicable, may be a very animating and attractive occupation, but is incompatible with the duties which a medical man owes to his patients. Mr. Jones, doubtless, acquired great applause for his exertions as a very gifted partizan in politics; but he failed to obtain, what by his education he was designed for, and by his talents deserved—high medical reputation, and its consequences, a good medical practice.

Whether in the early part of his life Mr. Jones attempted to settle himself as a practitioner is not known: in the latter part he carried on the business of a surgeon-apothecary, in a small shop

in Cromer Street, Gray's Inn Lane, but his practice must have been very limited, and unproductive: here he suffered privations many and severe. Among his other afflictions he lost, after a long and painful illness, a daughter whom he affectionately loved, and much regretted. His own health and strength, also, began to give way; and though not quite deprived of his eye-sight, yet his vision was greatly impaired. A few friends, who had known him in more fortunate days, contributed occasional assistance to him in his distresses; and, lastly, a subscription was attempted, among his political friends, to afford him more permanent relief, but it was unproductive, and of little service.

Mr. Jones retained to the last the conversation and manners of a gentleman. About a month before his death, the *Lyceum Medicum Londinense* was recalled to his mind, and the recollection was accompanied with a circumstance which tended to alleviate some of the sorrows of his departing days. The *sopiti ignes*, the sleeping fires, were awakened by this occurrence: he talked with pleasure of his old associates, and his early medical occupations; he exhibited the fondness of an author for his first literary attempt, the "Observations on the Tussis Convulsiva," remarking that it was become a *scarce* pamphlet; he evidently felt delight on thinking, that his early endeavours had been turned to the cultivation of medical science; and perhaps he breathed a silent sigh at the remembrance of the time when he quitted the calm and peaceful pursuit of medical investigations, for the vexatious disturbances of political controversy.

His life terminated on the 4th of March, 1838, at the age of 68.

TRITUS.

VACCINE INSTITUTION.

COPY OF THE LAST REPORT FROM THE NATIONAL VACCINE ESTABLISHMENT TO HER MAJESTY'S PRINCIPAL SECRETARY OF STATE FOR THE HOME DEPARTMENT.

(Ordered, by the House of Commons, to be printed, 11 April, 1838.)

To the Right Honourable Lord John Russell, Secretary of State for the Home Department.

MY LORD,

THE amount of deaths by small-pox this year, within the bills of mortality, has been less than in any former one since vaccination was first promulgated; but we are sorry to say that the disease has prevailed with its usual mortality in several parts of the country.

The apathy under which some thoughtless people wait until danger from small-pox be at their very door; the unreasonable distrust of others of the protective power of vaccination (notwithstanding the satisfactory experience of more than 40 years over millions of mankind), and the artful schemes of unworthy inoculators, who avail themselves of such ill-grounded fears —all these circumstances contribute to countenance and to keep up the practice of inoculation; by which a perpetual source of contagation is supplied, to the great danger of those who have not already had the disease, or have not been vaccinated properly.

A notion has been entertained by not a few, that the vaccine matter has lost its influence by time; but this is not supported by the analogy of any other poison. The virus of small-pox itself has lost nothing of its force in the course of 200 years; and we are enabled to state a strong fact, with perfect confidence, in proof of the efficacy of the vaccine matter at present, viz., that of more than 70,000 vaccinated in descent with successive portions of the matter originally collected by Dr. Jenner, 38 years ago, vaccination has manifested its peculiar influence in all; though of this number some hundreds have been subjected to the severest trials by exposure to small-pox in its most fatal form.

No, it has not worn out its protecting property. The rarity of an example of disfigurement by small-pox now to be found in the theatres, in churches, or any large assemblage of the people, affords some proof of this; but unfortunately, it was propounded by its original discoverer too broadly, in recommendation of the novelty of vaccination, that any person might perform the operation successfully, when, as we confess, and desire to have it made known, that, to do justice to the merit of vaccination, it ought to be performed by well-instructed and skilful surgeons, who are able to discover whether there be any temporary ill prevailing in the habit of the patient to be submitted to it, in the form of a slight cutaneous eruption (for this will often render the body unsuseeptible, for a time, of effectual vaccination); whether the prevalence of any epidemic disease may interfere with the success of the process; for it has been remarked by several experienced vaccinators, that the influenza of last year did make it necessary to repeat vaccination more than once or twice before it took its proper effect.

Merely to have been vaccinated has satisfied multitudes of those who apply to our establishment for this protection against small-pox; and we cannot help lamenting

that it is so difficult to prevail upon parents to bring back their children, at a proper distance of time, to ascertain whether the operation has been successful, or requiring to be repeated to ensure its efficiency.

This Board has been unremittingly occupied, and has supplied 20,000 charges of lymph more this year than it has sent out in the course of any former one; and not only have our Army and Navy, and the Colonies, been supplied, but most of the capitals of Europe have availed themselves occasionally of our resources, in which they express their entire confidence.

(Signed) We have, &c.

HENRY HALFORD,
President of the Royal College of Physicians.

ANTHONY CARLISLE,
President of the Royal College of Surgeons.

EDWARD THOMAS MONRO, M.D.
Senior Censor of Royal College of Physicians.

CLEMENT HUE, M.D. Registrar.

TEMPERATURE OF THE HUMAN BODY

IN DIFFERENT DEGREES OF EXTERNAL HEAT.

DURING the voyage of "La Bonite," for the purpose of making various scientific investigations, the attention of the naturalists was especially directed by the Academy to determine the temperature of man and animals in different parts of the globe. Experiments were therefore made on ten men, every day, from April 1836, while the Bonite was at Rio Janeiro, till her arrival in France, in November 1837, by introducing a thermometer into the rectum, and observing carefully the temperature of the surrounding atmosphere. All the men were occupied in a constant employment on board the ship, with regular diet, &c., so that no interfering agency could be present. The results obtained are, that the temperature of the human body falls very slowly during the passage from hot into cold regions; and that, on the contrary, it rises more rapidly in passing from cold countries to the torrid zone. The degree of change varied in different individuals. The average temperature of the men, observed at Cape Horn, at 59° south latitude, with an external atmospheric temperature of 0° centigrade, presents only a difference of one degree from the average observed in the same men in the Ganges, near Calcutta, with an external temperature of 40° centigrade. Thus a variation of forty degrees in external temperature gives rise to a difference of only one degree in the temperature of the human body.—*Rapport sur les résultats scientifiques du Voyage de la Bonite autour du monde, par M. de Blainville.*

Somewhat similar investigations have been made on another plan, by MM. Becquerel and Breschet, by the application of a thermo-electric apparatus for ascertaining the temperatures of different parts at the same time, or under varying circumstances. Their plan is, to introduce one needle into the biceps muscle of the arm, and to place the other in a temperature which constantly remains the same, as the mouth, or an apparatus heated with hot water. Each needle is composed of two pieces, one of copper, the other of steel, soldered together at one end. They communicate together at their steel extremities, by a fine steel wire, and at their copper ends they communicate with the wire of a delicate thermo-electric multiplier. It is evident that when the two needles are placed in parts having the same temperature, no deviation of the magnetic needle will take place; but that, on the contrary, the difference of temperature of the parts in which they are placed will be indicated by the degree to which the magnetic needle will diverge in one direction or the other.

The two needles were introduced into the biceps of the right arm, in two young men, and the temperature was found to be the same in both. One of them then placed his arm into water at 10°, 8°, 6°, and at last at zero centigrade, the external temperature being 16°. The experiment lasted an hour, and the magnetic needle deviated only 2° in favour of the muscle which was not immersed, indicating that its temperature was 1-5th of a degree greater than that placed in cold water. The same arm being then put into water at 42°, for fifteen minutes, the temperature of the muscle increased 1-5th of a degree. Numerous experiments were made by plunging the whole body into hot baths; but it was found, that after remaining for twenty minutes in water at 49°, which was as much as could be borne without injury, the temperature of the biceps was in no case elevated more than 1-5th or 2-5ths of a degree, and was sometimes quite unaffected; though the pulse was increased in frequency, the skin reddened, and the head felt congested. — *Nouvelles Observations sur la Mesure de la Température des Tissus Organiques au moyen des effets Thermo-électriques, par MM. Becquerel et Breschet. Comptes rendus des Séances de l'Institut*, No. 15, April 9, 1838.

AN ELECTRICAL LADY.

The following singular case is detailed by Dr. Hosford, in the "American Journal of the Medical Sciences," for Jan. 1838 :—

A lady, of great respectability, during the evening of the 25th of January, 1837, the time when the aurora occurred, became suddenly and unconsciously charged with electricity; and she gave the first exhibition of this power in passing her hand over the face of her brother, when, to the astonishment of both, vivid electrical sparks passed to it from the end of each finger.

The fact was immediately mentioned, but the company were so sceptical that each in succession required, for conviction, both to see and feel the spark. On entering the room soon afterward, the combined testimony of the company was insufficient to convince me of the fact, until a spark, three-fourths of an inch long, passed from the lady's knuckle to my nose, causing an involuntary recoil. This power continued with augmented force from the 25th of January to the last of February, when it began to decline, and became extinct by the middle of May.

The quantity of electricity manifested during some days was much more than on others, and different hours were often marked by a like variableness; but it is believed, that under favourable circumstances, from the 25th of January to the 1st of the following April, there was no time when the lady was incapable of yielding electrical sparks.

The most prominent circumstances which appeared to add to her electrical power, were an atmosphere of about 80° Fah., moderate exercise, tranquillity of mind, and social enjoyment; these, severally or combined, added to her productive power, while the reverse diminished it precisely in the same ratio. Of these, a high temperature evidently had the greatest effect, while the excitement diminished as the mercury sunk, and disappeared before it reached zero. The lady thinks fear alone would produce the same effect by its check on the vital action.

We had no evidence that the barometrical condition of the atmosphere exerted any influence, and the result was precisely the same whether it were humid or arid.

It is not strange that the lady suffered a severe mental perturbation from the visitation of a power so unexpected and undesired, in addition to the vexation arising from her involuntarily giving sparks to every conducting body that came within the sphere of her electrical influence; for whatever of the iron stove or its appurtenances, or the metallic utensils of her work-box, such as needles, scissors, knife, pencil, &c. &c., she had occasion to lay her hands upon, first received a spark, producing a consequent twinge at the point of contact.

The imperfection of her insulator is to be regretted, as it was only the common Turkey carpet of her parlour, and it could sustain an electrical intensity only equal to giving sparks one and a half inch long; these were, however, amply sufficient to satisfy the most sceptical observer of the existence in or about her system of an active power that furnished an uninterrupted flow of the electrical fluid, of the amount of which, perhaps, the reader may obtain a very definite idea by reflecting upon the following experiments. When her finger was brought within one-sixteenth of an inch of a metallic body, a spark that was heard, seen, and felt, passed every second. When she was seated with her feet on the stove-hearth (of iron) engaged with her books, with no motion but that of breathing and the turning of leaves, then three or more sparks per minute would pass to the stove, notwithstanding the insulation of her shoes and silk hosiery. Indeed, her easy chair was no protection from these inconveniences, for this subtle agent would often find its way through the stuffing and covering of its arms to its steel frame work. In a few moments she could charge other persons insulated like herself, thus enabling the first individual to pass it on to a second, and the second to a third.

When most favourably circumstanced, four sparks per minute, of one inch and a half, would pass from the end of her finger to a brass ball on the stove; these were quite brilliant, distinctly seen and heard in any part of a large room, and sharply felt when they passed to another person. In order further to test the strength of this measure, it was passed to the balls by four persons forming a line; this, however, evidently diminished its intensity, yet the spark was bright.

The foregoing experiments, and others of a similar kind, were indefinitely repeated, we safely say hundreds of times, and to those who witnessed the exhibitions they were perfectly satisfactory; as much so as if they had been produced by an electrical machine, and the electricity accumulated in a battery.

The lady had no internal evidence of this faculty, a faculty *sui generis;* it was manifest to her only in the phenomena of its leaving her by sparks, and its dissipation was imperceptible, while walking her room or seated in a common chair, even after the intensity had previously arrived at the point of affording one and a half inch sparks.

Neither the lady's hair nor silk, so far as was noticed, was ever in a state of divergence; but without doubt this was owing to her dress being thick and heavy, and to her hair having been laid smooth at her toilet, and firmly fixed before she appeared upon her insulator.

As this case advanced, and supposing the electricity to have resulted from the friction

of her silk, I directed (after a few days) an entire change of my patient's apparel, believing that the substitution of one of cotton, flannel, &c. would relieve her from her electrical inconveniences, and at the same time a sister, then staying with her, by my request, assumed her dress, or a precisely similar one; but in both instances the experiment was an entire failure, for it neither abated the intensity of the electrical excitement in the former instance, nor produced it in the latter.

My next conjecture was, that the electricity resulted from the friction of her flannels on the surface; but this suggestion was soon destroyed when at my next visit I found my patient, although in a free perspiration, still highly charged with the electrical excitement. And now if it is difficult to believe that this is a product of the animal system, it is hoped that the sceptics will tell us from whence it came.

In addition to the ordinary appurtenances of a parlour, it may be proper to add, that the lady's apartment contained a beautiful cabinet of shells, minerals, and foreign curiosities.

This lady is the wife of a very respectable gentleman of this place; she is aged about thirty, of a delicate constitution, nervous temperament, sedentary habits, usually engaged with her books or needle-work, and generally enjoying a fine flow of spirits.

She has, however, never been in sound health, but has seldom been confined to her bed by sickness even for a day.

During the past two years she has suffered several attacks of acute rheumatism, of only a few days' continuance; but during the autumn, and the part of winter preceding her electrical development, she suffered much from unseated neuralgia in the various parts of her system, and was particularly affected in the cutis vera, in isolated patches; the sensation produced being precisely like that caused by the application of water heated to the point a little short of producing vesication; in no instance, however, did it produce an apparent hyperæmia, but about the last of December a retrocession took place of this peculiar irritation, to the mucous membranes of the fauces, œsophagus, and stomach, there producing a very apparent hyperæmia, and attended, during the exacerbations, with burning sensations that were torturing indeed; and it was for the relief of these symptoms that medical means were used, but it was found no easy matter to overcome this train of morbid action.

It was nearly immaterial what medicines were used; no permanent relief was obtained, and no advantage resulted from the use of the alkalies, or their varied combinations. In a few instances a dose of the acetate of morphine was given to secure a night's rest, but she seldom made use of an anodyne.

The effervescing soda draught being very acceptable was freely given—from which, in addition to a rigid system of dietetics, the influence of the opening spring, and the *vis medicatrix naturæ*, relief came of her electrical vexations, of most of her neuralgia, and other corporeal infirmities, and to this time a much better state of health has been enjoyed than for many years.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, May 10, 1838.

Frederick George Rose.—Thomas Saumarez Lacy, Guernsey.—William York. Leeds.—Francis Whitwell.—William Henry Duff.—John Oakeshott, Chatham.—Thomas Harvey Hill, Herefordshire.—John Smith, Derby.—John Fenton, Staffordshire.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *May* 15, 1838.

Abscess	1	Heart, diseased	2
Age and Debility	32	Hooping Cough	8
Apoplexy	4	Inflammation	10
Asthma	5	Bowels & Stomach	1
Childbirth	1	Brain	3
Consumption	36	Lungs and Pleura	4
Convulsions	15	Influenza	1
Croup	3	Jaundice	3
Dentition or Teething	1	Paralysis	1
Diarrhœa	2	Small-pox	8
Dropsy	3	Spasms	1
Dropsy in the Brain	6	Stone & Gravel	1
Epilepsy	1	Unknown Causes	41
Fever	17		
Fever, Scarlet	3	Casualties	6
Fever, Typhus	5		

Decrease of Burials, as compared with the preceding week . . . 80

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

May.	THERMOMETER.	BAROMETER.
Thursday . 10	from 32 to 56	30·21 to 30·28
Friday. . . 11	22 58	30·31 30·21
Saturday . 12	18 5 68	30·10 29·96
Sunday . . 13	37 58	29·69 29·63
Monday . . 14	24 51	29·63 29 64
Tuesday . 15	17 56	29 64 29 68
Wednesday 16	15 57	29.75 29·76

Winds N.W. and N.E.

Generally clear, except the 13th and 14th, when rain fell.

The extraordinary severity of the frost on several of the mornings during the past week has, it is to be feared, entirely destroyed the bud and fruit of the more tender trees. The extent of the injury cannot immediately be ascertained.

Rain fallen, ·05 of an inch.

CHARLES HENRY ADAMS.

ERRATA.—In our last number, for "W. P. Barlow," read "W. F. Barlow;" p. 285, col. 1, line next to last, for "and there are," read "and then inquire."

WILSON & SON, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MAY 26, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XX.

Diseases of the Parenchyma of the Lungs (continued)—Treatment of Acute Pneumonia—Treatment of Typhoid Pneumonia, &c.—Chronic Pneumonia—Anatomical History—Pathological History, Signs, and Treatment—Œdema of the Lung—Pulmonary Hæmorrhage, or Apoplexy; Causes—Anatomical History—Signs and Treatment—Pulmonary Emphysema, or Dilatation of the Air-cells.—Anatomical History; Tense and Flaccid Emphysema—Causes, and Pathological History—Signs of Tense Emphysema—Signs of Flaccid Emphysema—Interlobular Emphysema—Treatment of Emphysema.

I shall say but little on the treatment of pneumonia, for you will find this subject pretty fully given in the article in the Cyclopædia. It will be enough to give you a word or two on the principles which may guide us in the application of remedies, as far as I have found by experience that those principles hold good.

Seeing that distension of the great pulmonary plexus of blood-vessels is the first condition of pneumonia, from whatever cause it may proceed, we may hope, in the early stages of the disease, to relieve this distension, as well as to prevent the process of reaction which renders it inflammatory, by free blood-letting; and in some instances, where the local signs announce the presence of the disease in its first stage, and the general symptoms prove it to be of a distinctly sthenic character, the free loss of blood by one venesection will cut short and completely cure it. But when the vessels have been so long distended, and become so much the seat of irritation that the mere removal of pressure from the sanguiferous system at large will not enable them to recover their usual size, a single blood-letting will not be sufficient; it must be repeated as often as the strength will bear it, and other remedies must be used, which in another way countervail the inflammatory irritation. Of these the most important are tartarized antimony and mercury.

The tartarized antimony is, according to my experience, the most powerful remedy in the sthenic forms of inflammation, especially in their earliest stage. Its mode of action is not well understood. It certainly does not subdue inflammation merely by its nauseating, emetic, purgative, or diaphoretic effects; for although these modes of operation are occasionally induced by it, yet it is often quite as successful, without being attended by any of them. It appears to be directly antiphlogistic, and we can only conjecture that it is so by a specific action on the inflamed vessels; which, after all, is just as intelligible as a specific action on the vessels of the intestinal canal, the liver, or the kidneys, which we ascribe to purgatives, mercurials, and diuretics. I cannot suppose that its operation is merely that of a strong counter-irritant, inflaming and causing pustulation of the gastric mucous membrane, as the Broussaians maintain; for although there have been two or three examples in which such effects have resulted from its use, these are extraordinary cases; and among very many in-

stances in which I have seen it used to a great extent, I have never met with any in which, after due discretion in its administration, any symptoms of permanent gastric irritation have continued during its use. In the wards of Laennec, I have seen patients taking as much as thirty or forty grains of tartar emetic daily; not only without causing sickness, redness of the tongue, pain, or diarrhœa, but sometimes even without destroying the appetite for food, or the power to digest it. Such a quantity as this, however, is unnecessary; and I believe that all the good effects of this remedy may be obtained from twelve grains, or less, in the twenty-four hours. I commonly give from one to two grains in an ounce and a half of some nicely-flavoured liquid, every second, third, or fourth hour, according to the severity of the case. The first dose generally causes vomiting, but this generally ceases with the second or third; or if it should not, a drop of hydrocyanic acid, given after or with the draught, will often stop it. So also purging may be restrained by the addition of an opiate. But for these details I must refer you to the works of Laennec and Stokes, and to the article in the Cyclopædia before named.

Mercury is better adapted to the less sthenic forms of pneumonia, and to its second stage, after blood-letting has been used as far as it can be with advantage. The treatment of the second stage, hepatization, which is sufficiently announced by its physical signs, must be conducted on a different principle from that of the first. There is then solid matter already effused, and no blood-letting, or other means of depressing the circulation, can remove this. Their utility is therefore much more limited, and is chiefly confined to the removal of any increase of irritation or congestion, or to reduce the bulk of the blood to the capacity of the abridged state of the respiratory organs. The chief remedies are those which modify the action of the extreme vessels, and promote the absorption and elimination of the effused matter. Mercury is the most powerful of these, and calomel is the most convenient form; and its operation may be materially aided, and troublesome symptoms allayed, by combining it with opium and using an alkaline expectorant mixture, varied in composition according to circumstances. It is at this period, too, that blisters and other counter-irritants become useful; and if the disease has not reached the stage of hepatization, they seem to assist in restoring the healthy condition of the pulmonary vessels.

There is only one recommendation more which I think it necessary to make, beyond the usual rule of spare diet in pneumonia; and that is, of as much abstinence from liquids as is consistent with the comfort of the patient. I am convinced that no temporary cause tends more to increase the dyspnœa than distending the stomach, and consequently the blood-vessels, by large draughts of liquid, whether of tea, barley-water, or any other neutral beverage; and the relief given to the congested vessels by blood-letting is often for a time frustrated by the copious libations in which patients indulge soon after it. A total abstinence from liquids is scarcely practicable or desirable; but great moderation in their use, by sipping now and then, rather than drinking, is both practicable and beneficial. You may suppose that this advice is of a piece with that which I gave for the treatment of a catarrhal cold; but it is not. I cannot claim to myself originality in this *; for I heard Professor Hamilton, of Edinburgh, sixteen or seventeen years ago, strongly recommend abstinence from liquids in pneumonia; and so much was he convinced of the propriety of this plan, that he even fancied that pediluvia sometimes do harm, by supplying liquid through the means of cutaneous absorption.

The typhoid form of pneumonia requires a considerably modified treatment. Blood-letting not only is very ill borne, but it appears to have little influence on the disease. There is, in fact, no vascular tension or tonicity; and you would empty the great blood-vessels and stop the heart's action before you could relieve the congestion of the lungs, or enable their vessels to contract. The depressing influence of typhoid or adynamic diseases renders any loss of blood hazardous; and local depletion is the utmost that can be attempted. Considerable advantage may, under these circumstances, be sometimes obtained from dry cupping on the chest, which, for the time, tends more effectually than even blood-letting, to draw the fluids from the

* I understand that my originality in recommending the "dry treatment," in catarrhal colds, is disputed by M. Piorry, who states that he has successfully treated bronchitis by abstinence from fluids for six years past, and that he has described and recommended this mode of treatment in his lectures, and (I believe) in the Bulletin de Clinique. I have at present no means of referring to the precise date of M. Piorry's publication; but that I preceded him in the practice will be pretty evident when I state that the article *Coryza*, in the Cyclopædia of Practical Medicine, in which the "dry method" is recommended for a recent cold, was written in 1831, and published in May 1832; and I had been in the habit of using it since 1826, and soon after that time recommended it to many medical friends in London and in Paris; among others, I may name Sir James Clark, Sir Benjamin Brodie, and Sir David Davies. I certainly never learnt the practice from any one, but others may have used it without my knowledge; and my friend, Mr. Keate, has lately informed me that his uncle did so many years ago.

congested organs, whilst it does not waste the blood from the system. Blisters and sinapisms may also give relief in slight cases; but they have little power where, as is commonly the case, the whole posterior parts of both lungs are congested with blood, perhaps in a morbid state. The principal remedy in this form of pneumonia (if pneumonia it can be properly called,) is mercury, which may be combined with opium, saline medicines, and even with stimulants, in certain cases; but these are points of practice too delicate to be detailed here.

The complication of pneumonia with bronchitis is generally a fit subject for blood-letting, followed by the antimonial treatment. It generally terminates by free expectoration; and expectorant mixtures and blisters are of more use than in simple pneumonia. In pleuro-pneumonia, local as well as general blood-letting should be practised freely; and after the more acute stage has subsided under the influence of these and of antimony, if the buzzing bronchophony and dulness on percussion still continue, the patient should be blistered and put under a mild course of mercury, to promote the removal of the fluid and interstitial lymph, which might lead to partial obliteration of the tissue of the lung.

We have hitherto said nothing of *chronic pneumonia;* we must not, however, pass it over; for although Laennec almost doubts its existence, at least as a separate affection, I by no means partake of this doubt. As we have seen, with regard to pleurisy, so in pneumonia, the inflammatory action may not entirely terminate with the effusion of lymph, although it do not lead to the third or suppurative stage. When the acute inflammation is extensive, and the effusion of lymph is not removed by absorption, the disease generally proves fatal before there is time for further change; but in circumscribed peripneumonies, or in small parts of more extensive hepatization, a chronic inflammation sometimes goes on, and produces that kind of tough induration which is the general result of chronic inflammation in a parenchymatous structure. Thus, in the lungs of those who have suffered from long and repeated attacks of inflammation of the chest, even where there are no tubercles, we not unfrequently meet with portions of the tissue which are dense, almost destitute of air and of liquid, tough, and sometimes almost cartilaginous. Their colour varies from a dark dingy-red colour to different lighter shades of reddish brown and buff, sometimes rendered grey by a mixture of the black pulmonary matter. Their aspect also is varied like that of acute hepatization, by the tissues that are most affected, it being sometimes granular or oölitic (as Laennec has described it,) from the especial thickening of the individual vesicles; in other cases streaked or veined, from the hypertrophy of the interlobular septa and cellular tissue under the pleura and around the large vessels; in others more uniform, and of a darker colour, from the pulmonary plexus of the vessels being the chief seat of the alteration, and the colouring matter of the blood entering largely into the deposition. In this last variety the cellular tissue between the lobules and under the pleura is somewhat thickened to the amount of several lines, and is of a light drab or grey colour, like that of miliary granulations, and like them has almost the density of cartilage. Now these changes, which thus occur as the sequel of acute pneumonia, are also frequently met with complicated with those states of the lung which are called tubercular; and a considerable portion of the consolidation that is met with in phthisical lungs, often presents precisely the same anatomical characters as these chronic hepatizations which supervene on acute pneumonia imperfectly subdued. I have also more than once met with them in the lungs of those who have long suffered from extensive organic disease of the heart, where the circulation through the lungs was perpetually impeded by the structural lesions of that organ; and it seems to me that the same mechanical congestion from this cause, which sometimes leads to an effusion of blood in the tissue, constituting pulmonary apoplexy, in other cases, if long enough continued, terminates in an effusion of lymph, and an obliteration and consolidation of the pulmonary tissue. This condition of the lung is sometimes coupled with irregular dilatation of the air-cells; and on examination, after death, the organ presents a knobby surface, and feels nodulated where the consolidation occupies parts or lobules. I think that there is reason to ascribe also to a minor extent of chronic inflammation of the parenchyma, that increased density and rigidity of the lung, without entire consolidation, which is often found in the lungs of those who have long suffered from dyspnœa.

The signs of chronic peripneumony are those of consolidation and obstruction of the vesicular tissue, which continue after the urgent symptoms of the acute disease have subsided. The dyspnœa has become less oppressive, although it is still felt on exertion; the fever has been reduced, although there is still some quickness of pulse, and a return of heat of skin towards night; there is still some cough; and although there may be improvement in

strength and appearance, it is not progressive; the patient remaining with his organs and functions abridged; there is still dulness on percussion, with bronchial respiration and vocal resonance in the seat of the late inflammation; and if, in this condition, he neglect the means which may most conduce to the better restoration of his health, the indurated portions of the lung may either prove centres of fresh attacks of acute inflammation, or they may themselves spread, ulcerate, and commence the career of a phthisical disease, which sooner or later will destroy life. I have met with several cases of consumption that have appeared to originate in this manner, independently of any tuberculous disease, or diathesis; the individual having been in excellent health, and quite free from all chest complaints before the attack of acute inflammation, which, afterwards degenerating into this chronic form, laid the foundation of a consumption which ultimately proved fatal. It is, however, much less untractable and slower in its progress than the true tuberculous consumption; for the disease is more local than constitutional, and if circumstances do not occasion its extension, and injure the constitution by a constant and increasing inroad on the functions of respiration and circulation, it may be in the power of nature to effect its removal. I shall resume this subject in connexion with that of Phthisis, and shall merely remark here with regard to the treatment of chronic pneumonia, that in the first instance a mild course of mercury, with external counter-irritation, and afterwards the use of hydriodate of potash, with sarsaparilla, or some similar alterative, with mild sea air, regular gentle exercise, and a well-regulated diet, have been, in my experience, the measures most deserving of recommendation.

I must now take a rapid glance of the other diseases, which are seated chiefly in the parenchyma of the lung, and are not essentially inflammatory.

Œdema of the lung is an effusion of serum into the parenchyma, probably both between the cells and minute tubes, and within them. Its anatomical characters are, that the lung is paler and heavier than usual, pits on pressure, and crepitates little under the finger. It is seldom idiopathic, but, like œdema of other parts, results from some loss of balance in the circulation, an obstruction to the return of blood, or occasionally an excess of exhalation. Thus it may arise from the obstructions occasioned by organic diseases of the heart, lungs, or liver; or from the increased exhalation supervening on exanthematous diseases, particularly scarlatina and rubeola; or on those diseases of the kidneys which interfere with their excretory function, and are accompanied by dropsical effusions in various parts.

Being thus a sequel of other disease rather than a distinct pathological condition in itself, it must vary greatly according to the cause which produces it. When extensive, it occasions dyspnœa, cough, and thin mucous or serous expectoration. The physical signs are a crepitating or *subcrepitant* rhonchus with the breathing, less fine and even than that of pneumonia, and giving proof of the presence of more liquid by the mucous rhonchus in some of the larger tubes. The natural vesicular murmur is rendered indistinct, especially at the lower and back parts of the chest, where also the sound on percussion is somewhat impaired. Now you see these signs are very like those of the first stage of pneumonia; and although I have said there is some difference in the character of the crepitation, yet this is scarcely marked enough to constitute it a distinction; and a better source of diagnosis is in the general symptoms, the fever, rusty expectoration, and progressive increase of pneumonia being absent, and there being present œdema of other parts, or other signs of disease that may be supposed to produce it. In fact, I should say, from my own observation, that the lungs are less liable to œdema than the external cellular tissue, and that you scarcely ever have œdema of the lungs without anasarca, or at least partial dropsical effusions in the limbs.

I can say no more of the treatment of œdema of the lungs than I did of that of hydrothorax; it must depend on the nature of the cause. The œdema which follows scarlatina generally soon yields to hydragogue purgatives and digitalis.

Pulmonary hæmorrhage, or *apoplexy of the lungs*, is an effusion of blood into the parenchyma and vesicular structure of the lung. The name apoplexy is by no means appropriate; for this word by etymology implies only the chief symptom which accompanies the analogous lesion of the brain, its striking-down or stunning effect, which by no means belongs to the lesion as it affects the lungs. The effusion of blood in the tissue of the lungs may arise from three classes of causes:—1. From circumstances obstructing the passage of blood through the lungs, such as diseases of the heart, especially those impeding the passage of the blood through the left side of the heart, with hypertrophy of the right ventricle; tubercles or other deposits in the lungs, compressing the blood-vessels; violent acts of exertion. 2. From a diseased state of the pulmonary vessels, or parenchyma generally, so that any temporary

congestion from exertion, or the continued exercise of the voice, occasions a rupture: in phthisis there is often both this fragile state of the vessels, and an obstructing pressure on them, and the same combination of circumstances produces pulmonary hæmorrhage in some diseases of the heart and arteries. 3. A dissolved or scorbutic state of the blood is another cause to which I should also refer some cases of pulmonary hæmorrhage; for I have several times, in fevers of a petechial kind, met with lungs presenting circumscribed portions of their tissue quite infiltrated with black blood, with effusions of the same kind under the pulmonary pleura, there being no signs of inflammation, and the adjoining tissue being free from disease.

The effect of these extravasations of blood is to produce patches more or less extensive of a very dark red or brownish red colour in the pulmonary tissue, their margins being often quite abrupt, as you see in these drawings. These patches are generally pretty solid, from the coagulation of the blood, and sometimes exhibit somewhat of a granular texture, like that of some kinds of hepatization; but the colours are all much darker. The hæmorrhagic masses may vary in size, from that of a hazel-nut to the whole extent of a lobe of the lung. In the larger masses it is common to observe in the centre grumous or fluid blood, and there is obviously considerable destruction of the tissue. It is not uncommon to find purulent and gangrenous matter in these hæmorrhagic portions; and this is not surprising, seeing that the effusion of blood not only breaks up and destroys the tissue in parts, but also, by its pressure, sometimes obstructs the vessels, and thus destroys the life of the parts which they supply. So, also, you may have hæmorrhagic engorgement associated with pneumonia in any of its stages; for the local obstruction and irritation of a clot of effused blood may be sufficient to determine inflammation of the adjoining tissue; and the distension of the vessels in the first stage of pneumonia may also, in those predisposed to it, be sufficient to occasion pulmonary hæmorrhage. The hæmorrhagic or inflammatory engorgements are likewise sometimes associated together with those congestions which are produced by obstructions to the passage of blood through the heart; so that in the lungs of patients that have lingered for some time with organic disease of the heart, we not uncommonly find apoplectic or hæmorrhagic spots, inflammatory congestion, and hepatization, occurring together.

The symptoms of pulmonary apoplexy or hæmorrhage are none of them constant. There may be dyspnœa, a feeling of tightness or dull pain in the chest, sometimes referred to a particular spot; cough; and what is most common and characteristic, hæmoptysis. The blood expectorated may be only little, and merely tinging the sputa, or it may be coughed up as pure blood; or it may be brought up in large quantities, often containing coagula, by an action like vomiting. The latter kind is generally associated with tuberculous disease, where a vessel of considerable size has been ruptured, and communicates with a bronchial tube. In the case of the hæmorrhagic masses in the parenchyma of the lung, associated with disease of the heart, there is sometimes little or no hæmoptysis, the effused blood coagulating in the tissue before it reaches the larger bronchi. The blood expectorated is generally, but not always, frothy and florid, and mixed with mucous sputa; and it is thus distinguished from that vomited from the stomach, which is more constantly dark coloured and mixed with liquid or other contents of the stomach. I have, however, often seen quite dark coagula coughed up; and when the hæmorrhage subsides, the blood in the sputa acquires a brown tinge. So in examination after death we find the hæmorrhagic spots of some standing, of a deep brownish red colour. Sometimes the expectoration of blood is followed by a relief to the dyspnœa and tightness of the chest; in other instances it is accompanied by feelings of faintness, and where the quantity of blood is large, its effusion may cause extreme danger, and even death by syncope, or suffocation. Or if the immediate effects of loss of blood be borne, there may follow the jarring pulse, palpitation, anhelation, tinnitus aurium, and other symptoms of the reaction or irritation which ensues on excessive inanition of any kind.

The physical signs of the effusion of blood in the tissue of the lungs are such as you would expect from the filling up of some portions of the tissue, and the pressure on the parts adjacent. Thus there may be some part of the chest in which may be found dulness on percussion, with absence of vesicular respiration; and if the consolidation be extensive, there may be bronchial respiration, and vocal resonance. Around this part, which may vary in extent, there may be an irregular crepitation, with a sound of bubbling in larger tubes, arising from the pressure of the coagulated blood on the adjoining texture, and the presence of some in a liquid state in the outer tubes and vesicles. When the disease is extensive, and especially if complicated with tubercles in the lungs, the expansion of the chest will be imperfect on one side. When the foregoing signs present themselves in addition

to hæmoptysis, they pretty clearly trace the hæmorrhage to the lungs; but if hæmoptysis be absent, they can only afford a strong suspicion, in addition to the presence of the causes likely to produce it, that there is hæmorrhage or apoplexy in the tissue of the lung. For the dulness on percussion, the bronchial respiration and crepitation, may be equally produced by partial pneumonia; and if there be at the same time bronchitis, the glairy sputa may get a rusty tinge, like those of pneumonia, from the blood effused, without enough admixture of blood itself to constitute hæmoptysis. These constitute difficulties in diagnosis, but the practice required is not equally perplexing.

Whenever some or all of the preceding signs are joined with fulness and strength of the pulse, it will be expedient to bleed and purge, according as the strength may bear, and further to reduce the arterial action by the use of digitalis, a spare diet, and a limitation to the use of cold liquids, and that in very sparing quantity. When hæmoptysis is present, an internal styptic must be also administered; and the most efficacious of these is the superacetate of lead, which has really appeared to me in several instances to restrain the hæmorrhage. It may be given in two or three grain doses, combined with the aqueous extract of opium, repeated every third hour or oftener, according to the urgency of the hæmorrhage. There is one point to be especially attended to when the hæmoptysis has been arrested, and there are still dulness on percussion and crepitation in some part of the chest; the infiltrated portion of tissue, or that adjacent to it, may become inflamed, and even destroyed by suppuration, and it is of great importance to check, by seasonable depletions, the tendency that is thus shown to a lesion, which under these circumstances may become destructive to the tissue of the lung. A free bronchial secretion, provided it be chiefly mucous and only tinged with blood, is not unfavourable; for it tends to relieve the pulmonary engorgement, and I have known signs of hæmorrhagic consolidation of the lung after hæmoptysis gradually disappear during the expectoration of a pretty copious glairy mucus, bloody at first, afterwards becoming brown, and finally opaque and muco-purulent.

I have thus given you a brief account of the diseases of the parenchyma of the lung which especially affect its vascular function, inflammation, œdema, and hæmorrhage; it now remains for us to consider some changes in the vesicular structure, which alter the proportions of its parts, and impair its aptitude for the performance of its proper office, namely, *vesicular* and *interlobular emphysema*. These lesions, although they have some resemblance to each other in their name and causes, differ essentially in their anatomical characters.

Vesicular emphysema is a dilatation of the air-cells of the lungs. It may be partial or general; and the partial kind may be confined to separate vesicles, or it may affect all the vesicles of a lobule; general dilatation may affect one lung, or the whole, or a considerable part of both lungs. The anatomical characters of these lesions present some variety, but they have this in common, that when examined either through the pleura in the recent state, or after being inflated, dried, and sliced, the air-cells are seen much larger than those of healthy lungs. When the dilatation is general, the pleural surface of the lungs may be as smooth as usual, only more convex; but, when it is partial, you see either the dilated vesicles or the emphysematous lobules, forming irregular prominences. Individual vesicles are sometimes seen under the pleura, and especially at the margin of the lobes, dilated to the size of a pea, a hazelnut, and in some cases to a much larger size.

I have noticed another remarkable difference between cases of dilated air-cells, whether general or partial. Some are accompanied with a diminished pliability of the texture of the lung; it has acquired a degree of rigidity; it does not collapse when the chest is opened, and gives a greater resistance to the fingers than a healthy lung does: this kind is generally accompanied by a great deal of the black pulmonary matter. There is here, notwithstanding the dilatation of the air-cells and lightness of the lung, a hypertrophy and toughness of some of its textures; and portions near the root are sometimes found approaching to the dry tough consolidation of chronic pneumonia, but still containing a good deal of air.

The condition of the lung in the other cases is just the opposite of this: the texture is more flaccid and yielding than usual; and when the margin of a lobe is pressed between the finger and thumb, it feels almost like a soft single membrane, it is so thin. It often does not collapse on opening the chest, and this appears to be from a loss of elasticity, for it pits on pressure, like an œdematous lung, and does not recover its shape again, and like it, too, it is commonly much paler than usual. Andral has noticed this condition of the lung under the name of *atrophy*, and observes that it occurs most commonly in old people. I have seen it both general and partial; the partial kind of flaccid dilatation being of common occurrence in the anterior lobules and margins of the lungs in tuberculous phthisis. The fringe of dilated

cells, like a row of beads, which is sometimes seen at the margin of the anterior lobes, is also simple dilatation without the least rigidity. On examining the dilated cells, they are generally seen to be smooth, and lined with membrane within, but I have several times found them communicating with one another; so that, on inflating one, the air entered those adjoining. They have been sometimes found to contain serum, pus, and even tuberculous matter. These several lesions are frequently associated with diseased conditions of the bronchial tubes, redness and thickening of the mucous membrane, hypertrophy of the longitudinal fibres, dilatation, and partial contraction.

We shall better understand the pathology and signs of these affections, if we inquire a little into the manner in which they may arise from the lesions which are known to precede and accompany them. You remember the account which I gave of the production of dilatation of the bronchi, and you will find the same views applicable to the dilatation of the air-cells. Laennec explained the origin of this lesion in this manner:—In cases of chronic catarrh, particularly of the dry kind, the small bronchial ramifications became so obstructed by the swelling of their membrane, or by the secretion of a viscid mucus, that the air can be forced through them into the vesicles only by an effort. Now, as in ordinary respiration, the inspiration, a muscular effort, is more forcible than expiration, the former may prove sufficient to overcome the obstacle to the introduction of air into the vesicles, while the expiration is inadequate to effect its expulsion. Successive portions of air, expanding by the increased temperature, are thus introduced and incarcerated in the cells, which are thereby kept in a state of continual dilatation. This may be one mode in which the air-cells become dilated, but I think that we shall find other causes still more efficient and common in operation. When there is partial or complete obstruction in any of the bronchial tubes or cells, the inspired air cannot press with the usual force beyond the obstructions; but it will press with more than usual force into the adjoining tubes and cells, to which its access is quite free, and these latter may thus become distended, and in time permanently dilated. The obstructions to which I allude may be caused by the viscid secretions in the tubes, thickening of the textures, tubercular deposits, and the like; and it is with diseases in which these occur, that dilatation of the air-cells is most commonly associated. Tuberculous or other solid deposits obstructing small parts of the vesicular texture chiefly, are especially calculated to cause the dilatation of the cells immediately adjoining; for, rendering inexpansible the parts which they occupy, they cause the force of full inspirations to fall on the pervious tissue; and we accordingly find these deposits almost constantly accompanied by pulmonary emphysema, which makes prominent the lobules or parts of lobules on the surface of the lung after death. Another cause of dilatation of the air-cells, and this of a pretty extensive kind, is rigidity, or want of extensibility of the longitudinal fibres of the bronchi. This change I have already noticed as an effect of chronic bronchitis, and you can readily perceive that if the tubes do not lengthen with the expansion of the chest, the air will press unduly on the peripheral cells, and occasion their dilatation. Thus we find those at the margins of the lower lobes most dilated, for these are most under the influence of the forces expanding the chest. It is not uncommon to find the marginal vesicles dilated in the lungs of old people with ossified cartilages; and I think we must ascribe this also to the comparatively immobile state of the ribs and central portions of the lungs, and the increased action of the diaphragm, and consequent undue pressure of the air into the texture immediately contiguous to this part of the respiratory apparatus. These are the chief mechanical causes by which the vesicular texture of the lung becomes distended; but there are others of a more vital nature, which may also be concerned in the production and perpetuation of this lesion.

M. Andral conceives that the air-cells sometimes become enlarged by a wasting away and breaking down of some of their walls, so that several are reduced to a few of larger size. This may be, doubtless, a cause of the unusually light and flaccid condition of the lungs often found in old people after death; but such a state simply would not constitute the disease which, during life, is so cognizable as vesicular emphysema. I have before said that an unusual flaccidity often attends dilatation of the air-cells, and this probably proceeds sometimes from atrophy, and sometimes from a loss of the proper elasticity of the textures. The analysis which I have given you of the structure and properties of the air-tubes and cells will suggest the existence of another cause of dilatation, in a defective tone of the circular fibres, as well as in a loss of elasticity in the longitudinal. I formerly mentioned to you the fact observed by Mr. Swan, that in animals which died after the eighth pair of nerves had been divided in the neck, the lungs were found uncommonly distended with air. Again, it was a remark of Laennec, that the lungs of persons who had been asphyxiated by the air of sewers, were

always very voluminous, yet filled with air, and he proposes a question whether this may be an idiopathic dilatation of the air-cells. In fact, if the contraction of the circular fibres be an essential part of natural expiration, it is plain that such influences as those just mentioned, which weaken or destroy their power, must render expiration imperfect. Dr. W. Stokes, of Dublin, has taken this view of the subject, and supposes that paralysis of the bronchial muscles is the chief cause of dilatation of the air-cells. Following the same view which Dr. Abercromby had advanced with regard to the intestinal muscles, he considers inflammation of the tunics investing the tubes, such as that of bronchitis, to be the cause of this paralysis. I cannot, however, consider the defective action of the bronchial fibres as more than an aiding cause in the production of pulmonary emphysema: it may do more in determining dilatation of the tubes; but the contractile power of the vesicular tissue is not clearly proved, and I think that the mechanical causes to which we have been adverting must be viewed as the chief agents in distending this tissue, which is naturally so yielding and ready to adapt itself to a change of pressure.

Reverting to the anatomical differences of the *tense* and the *flaccid* vesicular emphysema, we may well trace in one the effect of an over active and irregular nutrition of the textures, the common result of repeated or prolonged inflammation: and in the other, the absence of any such process, if not the presence of one of an opposite character, causing a wasting of the same parts. Now you must perceive that these opposite conditions will lead to very different effects in the signs and course of the lesion. In the one case, the lungs become comparatively fixed in a distended state; and as they resist the power of the expiratory forces to expel the air from them, unusual exertion is required in inspiration to introduce enough air to serve the purpose of respiration. They are, therefore, perpetually exposed to a distending force; and as the dilation proceeds, and the increasing rigidity or obstruction with it, the lungs acquire a permanent volume beyond what is usual, even in full inspiration, and they distend the walls of the chest, and press on and even displace the adjoining organs and vessels. Hence may be expected to arise continual oppression of the functions of both respiration and circulation, and cachectic and dropsical disease in the system as the sequel. In flaccid vesicular emphysema, on the other hand, there is little or no increase of volume of the whole lung, and no pressure on the other contents of the chest. Having arisen from the access of air to the vesicles being more free than the vesicles themselves were capacious, there is not that tendency to accumulation which accompanies the emphysema arising from inflammatory thickening or injured motory power in the tubes. In fact, in flaccid emphysema the tubes are often also dilated, and afford pretty free egress as well as ingress to the air. But the air thus admitted to few dilated cells cannot aerate the blood as it would in many small cells; it is out of proportion to the vessels and blood in the part. Hence, however this kind of emphysema may be caused, it will not improve the function of the lung, although it supplies more air to it.

I think that you will now understand what we have to describe of the symptoms and signs of vesicular emphysema. Inasmuch it is permanent, it will cause a constant shortness of breath, or dyspnœa; and the least additional obstruction, such as that of a cold, bodily exertion, or flatulent distension of the stomach, may increase this symptom to an oppressive degree; so as to resemble an attack of asthma. The permanency of the oppression to the function of respiration, in severe cases, induces a cachectic state of body, which is manifested by pallidity and some emaciation, and a depraved condition of all the excretions. In the tense form of emphysema, with increased volume of the lungs, there are added the symptoms of obstructed circulation, as well as imperfect oxygenation of the blood, lividity, and even blueness of the face and lips, dropsical effusions, palpitation, and other signs of hypertrophy of the heart. Dr. Stokes has remarked, that these symptoms are always worse in cases where the lower lobes of the lungs are chiefly affected, which he explains by the greater injury to respiration, which would be caused by the enlarged lung preventing the free play of the diaphragm. The expectoration accompanying emphysema is very various. It is most commonly mixed, a dirty serous fluid, containing portions of tough pearly mucus, or of the opaque sputa of chronic bronchitis. In the attacks of acute bronchitis which frequently occur, it becomes glairy, and often very copious towards their termination, like in pituitous catarrh.

Of the physical signs of pulmonary emphysema, one of the most remarkable is the loud hollow sound on percussion, which is even greater than that of a healthy chest. This is common to both the tense and flaccid kinds of emphysema; but in the former, when extensive, there is a distinctly raised pitch in the sound of percussion, such as in a less degree you can produce by striking the chest when you hold in a very full breath. As in this case, in fact, the walls of the chest are rendered more tense by the increased volume of the lung, and the vibrations which they make are

therefore quicker, although from the elasticity of the contained material, they are still quite free. The increased volume of the lung is manifest in advanced cases, in the shape of the chest, which is unusually convex or rounded. The sides, the front, the back, and even the supra-clavicular spaces, some, or all, present this rounded projection; and according to Dr. Stokes, when the lower lobes are affected, the heart, the liver, and the spleen, may be displaced by the emphysematous lung, which then yields its clear resonance on percussion over an extended region. In this tense kind of vesicular dilatation, the sound of respiration is very imperfect and wheezing, and forms a remarkable contrast to the efforts used to introduce and expel the air. This is perceptible to the eye as well; for it can be seen that, with all the exertions in expiration, the chest is very little diminished, and retains its large convex shape, whilst every intercostal, and every supplementary muscle, can be seen at work endeavouring vainly to depress the ribs. Is there any wonder that this force, continually exerted on the thoracic vessels through the stuffed lungs, should obstruct the circulation, and cause lividity, cyanosis, dropsical effusion, and ultimately disease of the heart? Inspiration is easier, but even this requires exertion; for breath has to be taken, as it were, on the top of breath, and even needs the supplementary effort of the cervical and superior dorsal muscles. There are sometimes heard some odd sounds, which are not those of common wheezing or impeded breathing. Now and then there is a sudden clicking or crackling, as if from the sudden passage of air into, or out of, a set of tubes and cells which were before closed. Sometimes there is a sound of friction, like that of a finger rubbed on a table, which may, perhaps, be produced by the rubbing of projecting lobules or cells against the costal pleura.

The signs of the flaccid form of emphysema may be much modified by the other disease, such as tuberculous deposit, that commonly produces it, but they are essentially distinct from those of tense emphysema; and neither this nor their anatomical difference has been, so far as I know, noticed by authors. There is the clear sound on percussion, but no raising of the pitch of the sound. In old people, where this form of emphysema is uncomplicated with solid deposit, the sound on percussion is clearer and deeper than in any other case. Neither the shape nor the motion of the chest is materially affected by this kind of emphysema; and the sounds of respiration, instead of being obscure and wheezing, are remarkably loud and even puerile, although they have sometimes a little of the whiffing or bronchial character, from the accompanying dilatation of the tubes. You will understand the reason of all this when you reflect that, in these cases, air passes freely into, and out of, the dilated cells, and that their enlarged size, together with the increased energy which the feeling of dyspnœa gives to the acts of respiration, is a sufficient reason why the sound of respiration should be louder than usual.

I have little to say about interlobular emphysema. It is essentially distinct from the preceding affection, although it may be combined with it. It is an effusion of air into the cellular membrane of the interstitial tissues of the lung, and therefore especially between the lobules and under the pleura. You see by these drawings that it is easily distinguished anatomically, by the air being in the line of the interlobular septa, and contained in angular cells of various shapes and sizes, and not round ones, like those of the lung. Sometimes air is effused under the pulmonary pleura, detaching it from the lung in the form of large bubbles. This affection is commonly produced by violent efforts, or by wounds of the lung; but sometimes from rupture of the air-cells by excessive or sudden dilatation. It may, if extreme, produce sudden, and even fatal, oppression to the breathing; but in slighter cases it is of no consequence, being removed spontaneously. The only sign supposed to mark this affection is a sound of rubbing with the motions of respiration, which the projecting emphysematous septa make against the walls of the chest. This is sometimes in successive jerks, so as to resemble the steps of a person mounting and descending a ladder.

I have no time to say much on the treatment of emphysema. The prevention of it is the most attainable object; and with this view, the removal of those inflammations which lead to an obstructed state of expiration, and the dispersion of the obstructions themselves, are the chief indications. The use of counter-irritants, alkaline attenuants, and other remedies recommended for chronic bronchitis and dry catarrh, is the most likely to fulfil them. Where there is already evidence of loss of contractility of the pulmonary tissue, mildly stimulating expectorants, and especially inhalations, may be of some avail; and frictions of a strongly exciting kind to the exterior of the chest, or blisters, are certainly of some benefit. Dr. Stokes mentions strychnia as likely to restore, in some measure, the lost contractility of the circular fibres. The symptoms which arise in inveterate cases of pulmonary emphysema must be treated on general principles, remembering that although we cannot remove the cause, we may somewhat

prevent its increase, and diminish its aggravations from temporary circumstances. The flaccid form of emphysema is scarcely a matter for treatment: we cannot increase the number of the pulmonary cells, but we may in some degree so regulate the body as to diminish the want of breath, and make the small number suffice. With this view, a tranquillizing plan of medicine and regimen, avoiding all circumstances which may tend to excite the circulation or respiration, at the same time promoting the due activity of the secretions and tone of the system, by gentle exercise and alterative tonics, may serve to keep up a tolerable balance of imperfect health, and prolong existence on a lower scale.

SOME OBSERVATIONS ON THE MEDICAL STATISTICS OF ARMIES.

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(*For the Medical Gazette.*)

Part II.

By the careful classification of facts, and their division into natural, and not arbitrary periods of years or seasons, without regard to the operations of an army, the particulars of the loss sustained necessarily lead to a just appreciation of the causes, whether they be many or few. To distinctly ascertain the cause is the most certain step towards the application of means calculated for the diminution of the loss or injury resulting. After such an analysis, therefore, of the leading features of each defined period, we come to the consideration of the aggregate loss in a war with advantage, bearing in mind the proportions before examined between certain prominent causes and their effects. It must be observed, however, that a total loss proceeding from heterogeneous causes in a long space of time, furnishes information less applicable to future occasions than that of particular periods, where all the circumstances can be distinctly stated, and their influence determined.

We find, by the returns of the Adjutant-General's Office, now published, for a period of 41 months, that the total loss upon a force averaging 2716 officers and 61,511 serjeants rank and file, is 32,829 privates and 940 officers; the annual rates per cent. being 16·1 per cent. for privates, &c., and 10·1 for officers. This is made up by sickness, by wounds, by killed, and perhaps by missing; for the number killed must generally be a guess, and does not include the disabled, who are just as much lost to the army and their country as the dead. By the returns in question, it is unfortunately impossible to separate these classes; while the missing are made up of deserters, or of stragglers and prisoners, both of which last may be recovered, and are not ultimately lost to the country, often only for a time to the army; the killed are thus erroneously increased, and, by the omission of the invalided, the actual loss is not ascertained. These difficulties can only be obviated by differently constructed returns.

The annual loss per cent. of privates in the Peninsular army, Mr. Edmonds states at 4·2 from battle, and 11·9 from all other causes; and in officers, from battle, 6·6, and from other causes, 3·7; total loss in privates, 16·1; in officers, 10·3; average number wounded per cent. per annum, in privates, &c., 15. No returns that I am aware of are before the public, by which a similar estimate can be accurately made, nor is the deficiency entirely supplied by those now produced; the above being the results to which the author has come apparently from the study of more documents than he has published.

In comparing these results with those furnished by the returns of the Legion, I find that the number of serjeants, drummers, rank and file, killed in the last period of service, extending from the 5th of May, 1836, to the 20th May, 1837, was 381, and there died of wounds received in that period, 212; which, from the proximity of the hospitals, included all who were not killed on the spot. The number of officers killed was 20, and 16 died of wounds. The total number of serjeants, &c. wounded, was 1600; of officers, 197. From sickness there died 6 officers in the same period, and 323 serjeants, &c. The total number of sick admitted, was 5198: the average force being somewhat under 7000, serjeants, drummers, rank and file; and 360 officers. It follows, that by killed there was an annual loss per cent., of serjeants, &c. 5·4, in officers, 5·5; from death by wounds, of serjeants, &c. 3·02, in officers, 4·04; from all other causes, a loss in officers of 1·6, and in privates, 4·6. Or the results stand clearly thus:—

Loss by Battle.				Loss by other Causes.	
		Per Cent.	Total.		Per Cent.
Officers	Killed	5·5	9·54	Officers	1·6
	Dead of wounds	4.04			
Serjeants, drummers, rank and file	Killed	5·4	8·42	Serjeants, drummers, rank and file	4·6
	Dead of wounds	3·02			

Total loss in serjeants, &c. 13 per cent.; in officers, 11·14.

Average wounded per cent. per annum:—Serjeants, drummers, rank and file, 22·8; officers, 54·8.

The Legion has been charged, among other crimes, with a trifling number of casualties, as a proof that its fighting had been insignificant. It was even suggested by a gallant officer, in the House of Commons, that a *fair* ground of comparison, by which to test the loss of the Legion with that of the British army, would be that experienced by the division of picked troops sent, under General Clinton, to Portugal, whose duties extended to pipe-claying their belts and appearing on parade; who were subject to no kind of labour, danger, or privation; who were never required to look at an enemy, and allowed to want for nothing supplied to soldiers in barracks in England. What had this fine division, forming part of the flower of the British army, changed, but a foggy climate for one delicious? Surely it is impossible to be sufficiently grateful for this specimen of the *fairness* with which the Legion is tried and judged.—*Ex pede Herculem!* Even then, take away our loss by battle, and sickness from wounds, after one year's seasoning only, and the comparison so generously got up for the Legion will bring no shame upon it*.

This view of comparative numbers will shew that which I should not have taken the trouble to prove, had not other objects led me naturally to the subject—viz. that the second year of the Legion's career, when compared with the second and most distinguished part of the Peninsular army's, so far from deserving the reproach, presents a result from which it need not shrink. Its average loss by sickness was less considerably, while the loss by killed and wounded was more, than that which is given as the annual average of an army which never had its merits or its glory disputed.

The proportion of those who die from wounds, Mr. E. estimates at a third of the number killed; a mode of computation which seems to me calculated to produce a very uncertain average: both because nothing is less certain than the number *killed*, and because the proportions vary much. While it sometimes amounts to a third, I have observed it at others equal to one half; whereas an average formed upon the number of wounded is less liable to error, and ranges pretty uniformly from 14 to 16 per cent. of the number treated in hospital. Beginning with the first day, the ultimate loss to the effective force, including disabling wounds, &c. is rarely under 20 per cent. of the same number, and will range as high as 25 from the severe and permanent nature of the injuries.

The relative mortality of officers and men in battle, and the proportions of both killed to the force engaged, are legitimate objects of inquiry, leading to practical and important results. The relative mortality of officers of different grades is perhaps more curious than useful. In most actions, and in all those in which I have been present, a greater proportion of officers than men have been wounded and killed; and of the total number of wounded, a smaller proportion die among the officers.

At the assault of Oporto, on the 29th of September, 1832, the most exterminating affair I have been in, a subaltern and myself were the only two not in the list of killed or wounded of the English battalion; and had I felt any ambition to figure among the latter, I too could have shewn a leaden signature. One in five of the officers were killed; one in three of the men were wounded, and about half that proportion, or rather less, killed. Both the English and French battalions were annihilated.

* I am unacquainted with any instance on record where troops on service have suffered less from sickness.

The rest of the army, however, did not suffer in that proportion, and so it generally is; one division often bears the chief or entire brunt of an attack, and the averages are, of course, drawn either upon the whole of the troops in the field, or on paper, including sick and absent. The muster rolls of armies the day previous to an engagement, and the returns the day after, would be of great value and interest.

The returns now published only give loss by death. The loss by injury to health or limb, not sufficient to produce death, we are informed by Sir James M'Grigor, amounted to 4586 in the last thirty months of the period, 3 per cent. per annum. No means have been afforded of distinguishing the disabled by wounds from those disabled by sickness, interesting in many points of view, particularly as relates to pensions and indemnities which become due from the employment of a given force in a given time.

The average proportion of an army constantly sick is among the most important features of this subject. Mr. Edmonds calculates that of the Peninsular army to be $22\frac{1}{2}$ per cent., while the sickness from wounds did not exceed during the same period $1\frac{1}{2}$ per cent.; the proportion in a year being 15 per cent. This is, indeed, a large average of sick, but it will continue to be a true one in all armies probably, until infinitely greater exertions are made for the comfort and health of troops at the close of each campaign than has ever yet, as far as I can judge, been attempted. The statistical labours, some of the results of which Mr. Edmonds has laid before the public, I hope may direct attention to a subject the importance of which seems scarcely to have been understood. Had the same degree of labour been given in a proper direction to the results of all the modern wars—the 65 years out of 157 in which England had been engaged, up to 1815, and for which she borrowed eight hundred millions—no doubt need exist as to the exact expense in men as well as money which war entails; and that expense would probably have been much curtailed by the information thus gained.

Having analysed the leading features of Mr. Edmonds' paper, with the view to shew at once the information it conveyed, and the points of importance either untouched or imperfectly followed out, the objects which fall within the scope of the medical statistics of armies are brought before us in their full proportions. It has been shewn generally what data are required for the formation of sound conclusions on the various important questions, and for the establishment of accurate averages, easy of application to all armies—conclusions and averages which, it must be evident, are calculated to influence the operations and success of a war prosecuted on a large scale.

To complete the task, however, a few observations are still required on the nature of the returns, and the specific objects to be answered by each.

For any future war, to furnish a complete body of facts, returns would be required to shew the total force employed from the beginning to the end, and the periods at which each portion or reinforcement arrived at the seat of war, divided into different arms and ranks. Monthly musters, from which averages could always be formed both of the effective force in all its divisions—of the sick, the wounded, and the dead, distinguishing those who die from sickness and from wounds; the state of sick, wounded, healthy in the field, and absent on command, at the commencement of a period; the exact force brought into the field in each battle, and the wounded and killed resulting. The classing those who die of wounds within ten days with those killed on the field seems to me not only unnecessary, but to mix up two returns which ought to be distinctly separate, and to come from different authorities, and more likely to create confusion and error than to answer any good purpose. Thus to arrive at the total loss in life and health during a war, and the particulars which give the causes of inefficiency and loss to an army, there must be—

1. A return of total force employed, specifying numbers in different arms and ranks sent to the seat of war, with the dates of their arrival.

*2. Monthly musters of effective force, in the field and absent on command—

* When an army is divided into two or three columns, undertaking at the same time operations of very different characters, similar returns should be produced of each for the period that such division and difference lasted.

of men in hospital, sick and wounded—the number dead—and invalided from sickness or from wounds.

3. Returns of force—different arms and ranks distinguished brought to the field in each action—loss from each by killed, missing, and wounded.

From these could be constructed the *general returns of loss*, classed as follow:—

By death	In action.	Different arms and ranks distinguished.
	By wounds.	
	By disease.	
By missing	Deserters.	
	Prisoners.	
By permanent disability	From disease.	
	From wounds.	
Average temporary loss to the force in the field.	By sickness.	Different arms and ranks distinguished.
	By wounds.	
	By absence.	

From these, the loss could be estimated with certainty and ease, during any period which might be selected for a specific object. If to each return was added a short abstract, describing with accuracy the chief collateral circumstances bearing upon the results recorded in each, there is scarcely any new combination which might not be made from such documents. As, for instance, to No. 1 should be appended some general data of the average age and previous state of health and mortality of the force. To No. 2, information on the periods formed by the varying operations, on the general circumstances of each—such as the supply of rations, of clothing, the state of the hospitals, quarters, the nature of the service, the surrounding country, the weather, and their apparent or obvious influence on the mind of the soldier, the state of the medical staff as to numbers and efficiency, some general heads on the chief causes of disability, and the leading forms of sickness. These should be given in a form at once short, concentrated, and clear. To No. 3, a short description of the nature and extent of the ground; of the proportion of the force actually engaged; of the arms most employed on one side or the other; whether attacking or defending; the state of the weather; the results of the action and the *animus* of the soldier before and after; on the probable amount, when the number could not be counted, as in a retreat, of killed, as distinguished from missing.

So much for the general statistics which should give, at a *coup d'œil*, the results of the whole war and army, as far as regards all the various sources of inefficiency and loss. The total, absolute, and comparative loss of the whole and of each epoch of the war in different ranks and arms of each battle, year, &c., could at once be collated with the military and political results. The relative influence of different causes would not only be easily ascertained, but would stand out distinct. By such means we might hope to form a sytem upon which war could be prosecuted with the least possible expenditure of health and life compatible with the political or military objects to be attained, and consequently of money, for men's lives are expensive losses to a state when employed in an army. Averages once fixed, no extravagant or uncalled-for waste of life or health in the prosecution of a war, could occur, without its being detected as unnecessary, and attention being exacted to remove the causes. A certain loss is inevitable in all operations, but this loss may be confined to that which is strictly inevitable, or extended to the most lavish and heedless waste: this can never be checked or prevented until past experience is made to furnish correct averages, and for this experience has never been called upon.

A step further in the returns would lead us to the specification of the total and proportionate number of different classes of injuries and diseases, with their consequent duration and mortality, from whence would ensue accurate estimates of the precise ultimate loss to the effective force of an army in any given number of wounded or sick, first by death, secondly by disability; and further, the monthly rate at which those destined to recover would return to the ranks. While the previous or general

statistical returns would furnish Governments and Generals with data on which to estimate the contingencies from wounds and sickness, the extent of the stores, hospitals, and Medical Department required, these latter would point out the quantity and character of the stores; and the number of wounded and sick once ascertained at any period, the average ultimate loss, and the periods at which the rest might be counted on as ready for the ranks, would be not only easily calculated, but with certainty, if the averages were formed upon a sufficient number of accurate returns on a large scale.

Returns appended to each of these, shewing the annual consumption of medicines and medical stores, &c., divided into classes, including all the most requisite and important, would certainly not be impossible; and the result would be the furnishing precise data by which the annual supplies of this description for an army might be calculated with accuracy.

Some of these returns now specified, I have furnished in the "Notes" which appeared on my return to England; not, as I have stated, in the vain hope that they would by themselves supply the averages so desirable, but as examples of the character of the observations, and the nature of the returns required, to effect the objects now fully enumerated, and which have long appeared to me of the highest importance and utility. I gave but a fragment of the whole for many reasons, and principally, that a force of from seven to ten thousand men is too small to form conclusive averages upon, yet the same number and volume of returns would be required to render a complete account on the scale now described, as for an army of an 100,000. Many already drawn up for my own satisfaction, therefore, were withheld, that the volume might not be increased, and appear disproportioned in size to the strength or importance of the force. Bearing in mind, too, that the Legion, from its formation, has had not only to war with the Carlists, but with the prejudices and politics of no small number of their own countrymen, I had no right to expect that greater weight would be given to its medical statistics, than to its career in the field, even if somewhat more of impartiality were conceded. If the Chapelchurris raised the black banner, refusing to give that quarter they were ever ready to sue for, and receive on the field, the Chapelchurri party in England, with laudable zeal, seem to have adopted the same ensign; only they have exceeded their friends, for they have given even less justice than the partizans whose cause they espouse. Fortunately they differ in one essential point, and the good intentions of our friends in England break no bones!

The views given in this paper on the true objects of the Medical Statistics of Armies, are the matured conclusions of many years' service with armies in the field. Had I commenced with those views, all the data collected would have borne upon these distinct points, and the results might then more easily have been reduced to demonstration by figures. Such was not the case, and returns, or even the observation of facts, will rarely prove conclusive, unless made with a view to the particular object hereafter to be illustrated by them. Therefore, having felt this from my own labours in the first campaigns, was I anxious, in "the Medical Statistics of the Legion," to call attention to the points which seemed of importance, and to require illustration. With the same view have I analyzed the documents relating to the Peninsular war, lately submitted to the public. I referred to them eagerly, in the hope of affording the data concerning the career of armies, which observation had convinced me were desirable. If while in some instances that hope was gratified, in many others it has been disappointed, the cause most probably lies in the construction of the documents to which the author has had access, which may not have been formed to give full and precise, or in some cases, any information on the points of inquiry here suggested.

CONTAGION OF YELLOW FEVER.

To the Editor of the Medical Gazette.

SIR,

THE limits of a letter prevented me saying all I could have said on the origin of yellow fever at Demerara when I wrote last in answer to Dr. Fraser on that subject, and therefore I now resume it, believing it to be as deeply an interesting one as can engage the attention of the medical inquirer, and feeling moreover that as I have taken it upon

me to reprobate the opinions of another respectable physician, it behoves me still further to shew either that I have enjoyed superior opportunities of making myself better acquainted with the disease in question, or that his reasonings have been based upon false and untenable grounds.

The inhabitability (if I may use such a word) of Demerara is a problem to the newly arrived European, which he cannot easily resolve, for mud—the deepest alluvial mud—and surface water in deep canals, meet his view wherever he casts his eye; while he walks on the outermost rim of what I have elsewhere described, for want of a more expressive term, as "the swamp of 1000* miles being deep, vast, impenetrable and immeasurable," under the hottest temperature of the West Indies, being within six degrees of the equator; and well may he wonder how the unseasoned stranger could sleep a single night ashore without becoming the subject of marsh fever. The solution is to be found in the magnificent tide rivers that run through the territory, giving constant change, current, and motion, to all the waters, even the underground waters, within their influence—in the high cultivation of the cleared country—in the tide and floodgate drainage through which that cultivation has been achieved; for without these *terra firma* could not exist; all would be swamp, mud, and jungle—and in the adherence of the miasma to the umbrageous woods of the interior boundary; for as yet the settlement has been restricted to the sea or river borders alone, the reclaimed country seldom extending beyond three miles inland. The absolute flatness of the country for an immeasurable distance into the interior (the tide flows into the Essequibo more than 100 miles), strange as it may seem, forms another security, for thereby perflation is ever insured: there is no interruption whatever either to the northern or southern winds; and be it remarked that these last in the summer season are often the coolest from the proximity to the equator. Could a mountain ridge, of sufficient elevation to obstruct the breeze, be found in such a country, its base on either side would infallibly prove pestiferous. The very canals themselves furnish another security of considerable importance, for thereby the water, instead of emboguing the neighbouring lands, is preserved in bulk, and water under that form emits no miasma. How else could the tropical seas be ever navigated in safety? for the surface of the ocean every where is the freest from miasma, and the least malarious of any in nature. I have shewn, in the history of the two strangers, related in my last, how yellow fever could spring up in Demerara*; and when it assumes an epidemic form, or one of uncommon malignity, are we to be told that it was imported from Barbadoes? This would truly be a work of supererogation; in the vulgar phrase, carrying coals to Newcastle; yet, when once sanctioned by the quarantine authorities, it will pass current from mouth to mouth; for men every where evince the greatest disinclination to bring themselves to believe that their own habitations can be pestilential. Each will defend his own location†; its character and value are involved in the decision, as well as his own sagacity and penetration in having made such a choice; and guided by the quarantine officer, he will easily find amongst our thoughtless, most intemperate sailors, whose resorts are the malarious dens of the unwholesome anchorages, some victims of the rising epidemic on whom may be fixed the guilt of importation. This is the true history of all the importations of yellow fever into the ports of the West Indies, and, I will venture to say, every where else; being all either fictions of prejudice or self-interest, or delusions of panic terror.

I do not advance the foregoing as a piece of special pleading to make the worse appear the better reason; for it has ever been a mystery to me why the epidemic malarious fever of the Western Tropic should differ so much from that of the East, there being no such bursts

*Vide United Service Journal.

* I have often witnessed similar instances amongst soldiers exposed to great heat, excitement, and fatigue.

† An amusing instance of this occurred at Trinidad, when I was making an inspection of the stations in that colony along with the Deputy Quarter-Master of the Forces, which I shall here quote from my Inspection Report. "The Corregidor of Arima assured us from his bed, where he lay under the paroxysm of an intermittent fever, that no such disease existed in the country; and, when questioned in regard to his then illness, said that some days before he had imprudently drank some milk, which milk gave him the bile, and that bile always brought on the fever." To judge from his appearance he must have been following a milk diet, for attacks of fever had evidently been frequent with him.

of yellow fever in Bengal. Nor do I wish to evade any fact that can be adduced to prove the possibility of importation; and one of these alleged, the breaking out of yellow fever at the Island of Ascension shortly after the arrival of the Ban frigate from the Coast of Africa, with men on board ill of the disease, would, if it cannot be explained, decide the question in the affirmative. It was given to the world by the late Sir Gilbert Blane, one of the most philosophic and enlightened physicians of the age; but alas! in what mind, however bright, may there not be found some dark spot—and on his was imprinted the contagion of yellow fever, and indeed contagion in every shape. He had never, I believe, seen the disease, his services having been limited to a brilliant naval compaign with the great Admiral Rodney in the West Indies; and he states, in his own Memoirs, that on the never-to-be-forgotten 12th of April, there was not a case of fever in the Fleet, and scarcely a man absent from his gun.—But to return to Ascension. Is the surgeon, or other medical officer, of the Ban yet alive to throw light on the above occurrence? Had no similar endemic fever appeared there before, or has none since? Were the sick on board the Ban cases of true yellow fever, such as have been styled the pestilential contagious Bonlam? I was once a contagionist myself, and continued so till my eyes were opened—forced open—by the daily experience of my life, while living in the midst of yellow fevers—but I trust they are open still; and if the above can be answered in the affirmative, and it can be shewn that the base of Ascension Hill, within seven degrees of the equator, and nearer to it by three degrees than Sierra Leone itself, is free from the seeds of endemic fever, then will I subscribe to the importation creed; but if on the contrary the leeward base on the mountain, like all others within the tropics, should be found as malarious as Gibraltar in seasons amounting to the ordinary tropical heat, then must it be evident to every candid mind that the fever which appeared after the arrival of the Ban could be no more than a fever of degree, and the only wonder was the simultaneous outbreak with the arrival of the vessel; but such outbreaks can neither be timed nor regulated, more especially in so scanty a population, nor can the *post hoc, ergo propter hoc*, be admitted as medical evidence, until confirmed by repeated and never-failing instances of the same occurrences in the due order of sequence.

There is a strange and mysterious sympathy connected with the adventitious mixture of men, and the outbreak of disease, which would almost lead one to give credence in some degree to the dreams and impostures of animal magnetism. The inhabitants of St. Kilda*, for instance, have a tradition and firm belief that whenever a strange vessel has communicated with the people, every one on the island will be seized with epidemic catarrh; and wherever the belief, however childish it may be, is strongly impressed, the fulfilment may not be far off. So in the case of the Ban at Ascension, the alarm of having yellow fever on board, with the spectacle of the disease itself, and the *fama mendax* of its dangers, may have given power and agency to the endemic influence, which otherwise might have lain dormant. There is, in fact, a tendency to, and imitation of disease, wherever it is brought prominently forward to the mind's eye, which, independent of contagion, may account to a certain extent for the epidemic current. It may be seen any day in convulsive attacks running through a whole boarding school of young girls, because one of the number had been taken with an hysteric fit: still better may it be shewn in the comparative phenomena of Asiatic cholera, as at present hovering over our land, and as it existed during the former visitations. Sporadic cases have occurred—I may say are even now occurring—which, being treated and passed *sub silentio*, have gone out where they arose; but had a medical board been assembled to burn the bedding, fumigate the dwelling, excommunicate its inmates, and hurry off the dead (as if the dead could communicate infection, even allowing it to exist) to a remote and sequestrous grave, even before life was thoroughly extinct, (for however irrecoverable a patient may be, no one can be altogether dead to whom animal heat returns, and in some degree remains) in fact to alarm and terrify the public mind; I think there would have been as good a chance of an epidemic spread of the disease amongst ourselves as there

* Vide Clarke's Travels.

was of yellow fever amongst the inhabitants of Ascension on the arrival of the Ban! But to return to the statistics of yellow fever.

Soon after our troops took possession of Cape St. Nicholas Mole, St. Domingo, a census was made of the inhabitants of the town, when it was found that they amounted as nearly as possible to the same number as the white garrison, viz. about 1500 in each. The harbour being at that time the best and by far the most important in all the West Indies, it was indispensable to keep the garrison up to its full complement, and by the time its whole original number had been carried off, almost entirely by yellow fever, only 46 of the inhabitants of all ages had died, without a case of yellow fever amongst them. I of course except newly-arrived civilian strangers, but certainly not one amongst the free coloured population, or the white creolized inhabitants. I was surgeon to one of the regiments in garrison, and also held the appointment of garrison surgeon; so that I had the fullest opportunity of verifying all the foregoing, and can vouch moreover for there being at all times the freest communication with the people of the town, amongst whom we lived in every supposable kind of way. Nor could there be any thing in the constitutions of these seasoned people (who, by the by, were often in a state bordering on famine, for the garrison had only two fresh meat days in nine months, of which they got none) that would have exempted them from a specific contagion, had any such existed. Were their stamina of residence of higher order? or their relaxed frames better adapted to withstand the invasion of a new plague? Do they, in fact, so resist any true contagion whenever it comes amongst them? I remained in the garrison nearly two years, and before I quitted it, had a melancholy reply to my last query, for the contagion of small-pox was introduced, (where was the quarantine then?) and devastated the coloured people, for the time that it lasted, like a burst of yellow fever amongst newly-arrived troops; while the white garrison having all passed through inoculated small-pox, had not a single case. Never shall I forget the horrors of a negro hospital filled with that disease under the confluent malignant form; and will any man take it upon him to say, that the negro, who, like any other individual of the human race, is subject to all true contagions when they fall in his way, possesses a ticket of safety against that of yellow fever, if it was contagious, or that the relaxed white creolized inhabitant is gifted with a similar protection? It cannot be, nor can we possibly explain how the exemption exists upon any other ground than that of the fever being non-contagious and endemic, a disease of unwholesome locality and tropical heat, untransportable from such locality to others of lower temperature, most dangerous to all strangers who approach it from colder climates, but safe, comparatively speaking, to the natives of the country, or others, who through long residence have become habituated to the endemic influence. Does any one impugn the foregoing positions, let him inquire how often sugar ships, after losing half their crews, have sailed homeward with the yellow fever on board, and how often they imported it into England, or even carried it beyond the tropic, and he will find that it has been just as impossible to convey yellow fever into the higher latitudes, (I do not deny that it may originate there in seasons of uncommon heat) as it has been to transport the infection of typhus fever from the shores of Britain to those of the Antilles. The records of every shipping harbour in the West Indies will establish the first position beyond the possibility of doubt; and the hundreds of crowded transports that at the beginning of last war sailed in the ill-fated expedition of Admiral Christian and others will amply attest the second *, for we cannot exchange the endemics our's will not go to the West Indies, and their's will not come to us; the one being as certainly dissipated by the heat of the tropics, as the other is extinguished by the cold of a European climate.

It may well be asked how, with such facts as I have been stating, and have been stated times innumerable by other

* The medical history of the typhus fever that infested that part of the army which rendezvoused in the Cove of Cork, has been given to the world by the deceased Dr. Jackson, in a work well worthy of being consulted. I sailed after it with the wrecked and broken fragments of divers regiments—the outpourings of different hospitals—in the General Elliot, a large East Indiaman, with at least 600 souls on board; but so healthy is the navigation of the tropical seas, when vessels retain no seeds of endemic disease within themselves, that when we reached Barbadoes, there was not one human being, soldier or sailor, man, woman, or child, in the sick list.

men of the highest character and experience, the belief of the contagion of yellow fever still prevails, and is acted upon by the government of every country liable to come into communication with the disease? But contagion has been the creed of an age not yet gone by: it passed unquestioned during nearly the whole of the last century; it is even now taught in the schools, and is consequently believed by the majority of those who never saw the disease: people like to be frightened by whatever they do not understand, and the official authorities have remained in this predicament, while their fears have led them to invoke the hosts of quarantine to their aid, and to pay highly for the discovery of impossible contagions. At the seaports they have enlisted in the cause, and summoned to the discovery, the most able men there residing. No wonder, then, they found what they paid for so well: but have the health-departments of the service done their duty in thus permitting the government to be deceived, the public mind to be abused, and the commerce of the country impeded, by these mischievous restrictions? I know not whether they have or not, but I know that there has been accumulating in the records of the Army Medical Department for more than 40 years, ever since the first invasion of Martinique and Guadaloupe in the year 1793, a body of evidence the most continuous and irrefragable that ever was adduced upon a medical question. I do not say that all the reporters have been unanimous, (for on what point of medicine have professional men ever been unanimous?) but I confidently assert that the evidence of non-contagion will preponderate a hundred-fold beyond that of the contrary. Away, then, with the quarantine against yellow fever every where; and let it no longer be told of us, that at Gibraltar* more especially, we have been looking across the Atlantic for the advent of the poison that lay under our feet; and although we could easily keep out the plague, and all true contagions, by the simplest precautions of quarantine to them so properly applied, we were unable to discover that such precautions must be utterly vain against the endemic malaria of our own habitations.—I am, sir,

Your obedient servant,

W. Fergusson.

Windsor, April 24th, 1838.

* Gibraltar stands like a toll-house, in the entrance of the Levant; yet the plague of those regions—the most infectious I believe of all diseases—has not been received into the place for more than 150 years. The yellow fever, however, has attacked it at least four times within the present century, and the quarantine authorities have always at once denounced the ships that brought it from the West Indies; but how those ships, fraught with the contagion of yellow fever, came to arrive at Gibraltar with incomprehensible punctuality, at the close of such burning seasons, as assimilated its climate to the hottest quarters beyond the Atlantic, and at no other, they have not yet explained.

ANNUAL REPORT

OF

PAROCHIAL LYING-IN CASES

DURING THE YEAR 1837.

By James Reid, M.D.

Medical Superintendent of the Infirmary of St. Giles and St. George, Bloomsbury.

During the year 1837 there occurred in these parishes, under my superintendence, 270 cases of labour, and 4 of abortion.

Of the 270, 5 were twin cases; the total producing 143 male children, and 132 female—of which 18 male, and 13 female, were still-born.

The greatest number of births took place in May and July; the smallest in February and March.

Of the women delivered, the greatest proportion were between the ages of 20 and 25.

There were of the age of 17 and under, 8; between the ages of 40 and 45, 7.

Unnatural Presentations, Complications, or Accidents, occurring during or after Parturition.

Of the above-mentioned 270 cases, the

Breech presented ..... in	12
Breech and funis	1
Foot	5
Knee	1
Arm	3
Hand and forehead	1
Face	2
The placenta was retained and required removal	4
Accidental flooding occurred before delivery	1
Flooding after delivery	7
Do. from retained placenta	1
Ruptured uterus	1
Convulsions	1

Puerperal phlebitis ..... in	1
Retroversion of uterus	1
Sloughing of vagina from protracted labour	1
Painful tumor in labia during labour	1
Impaction	2
Lingering labours	13

The forceps were used in 2 cases; perforation in 2; turning in 3.

Mortality.—Of the foregoing cases, 3 women died: one of ruptured uterus; one of puerperal phlebitis; the third was brought into the house in a dying state, the result of disease, but was delivered previously to her death.

Puerperal Phlebitis following severe Hæmorrhage from retained Placenta.

Elizabeth Baldock, æt. 23, was delivered in the lying-in ward, of her first child, on October 8th, at 5 A.M., after a tedious labour of forty-eight hours' duration. Flooding came on soon after; but I was not sent for to visit her until 11 A.M. I found the discharge of blood continuing copiously, and the patient lying completely uncovered about the lower part of the body and extremities, with an open window close to her. This mismanagement was owing to the folly of a nurse, who had too literally obeyed the directions of the midwife to keep her quite cool during the temporary absence of the latter. The pulse was feeble, the extremities cold, and there was severe pain in the lumbar region, attended with sickness, yawning, tinnitus aurium, and urgent thirst. On examination, large coagula were found in the vagina, and gushes of blood followed the slightest exertion. The placenta was still adhering strongly to the fundus of the uterus; this with the coagula were immediately removed, and the hæmorrhage ceased. The woman it seems has been subject to epileptic attacks, and fell down stairs last week during one of them.

Small doses of Tinct. Opii, with Infus. Ros., were directed to be given every three hours, and a little port-wine, diluted, immediately.

After a few hours she rallied, and was apparently doing well till the 10th, when she was attacked by rigors, nausea, thirst, increased heat of skin, and other symptoms of fever. The lochia are scanty, and there is pain in the hypogastric region on pressure.

Hirud. xxiv. abdomini.—Calomel. gr. ix.; Pulv. Dover. gr. x. M. statim sum.

Mist. Febr.—Cataplasm to be applied over the abdomen after the leeches.

11th.—Has passed a restless night, and had three dejections. Profuse diaphoresis towards the morning. Pulse still frequent, and skin hot, with thirst. No pain in abdomen on pressure.

℞ Opii, gr. ss.; Calomel. gr. j.; P. Antim. gr. ij. omni 6tâ hor.

During the 12th she felt much better in every respect, and on the 13th complained only of pain in the head, for which leeches were applied to the temples. The assistant-surgeon had ordered her in the evening one grain of digitalis, which was repeated in the morning, as her pulse had increased in fulness.

14th.—Found her with a pale and anxious countenance, and unable to answer any questions; pulse 120, and very small. Diarrhœa has supervened.

Mist. Camph., with small doses of Tinct. Opii, were given every three hours, with a little wine and water. The other medicines to be discontinued.

15th.—Says she feels much better, but is feverish; pulse 130; tongue clean; skin hot, and of a dusky yellow hue; slight diarrhœa still, but no pain.

Arrow-root diet. Omit the wine, &c.

16th.—Skin not so hot, but the expression of her countenance is anxious; tongue not dry or foul; pulse 150, but not strong; tormented by thirst. No pain in any part of the abdomen, and complains of no other active symptom. No headache nor diarrhœa, and she has slept tolerably well.

An effervescent antimonial mixture was prescribed, to allay the thirst.

17th.—Has not slept much, and there is a hectic flush on her cheeks. Complains of deafness for the last three days; pulse 140, but weak; urgent thirst still; tongue clean; urine high coloured, and the dejections are dark and fœtid. No pain in abdomen, but only in the loins.

Ol. Ricini, to be followed by an effervescent mixture; with a small dose of Tinct. Opii occasionally.

From this period the diarrhœa continued to a greater or less extent; the abdomen became tympanitic, but still free from pain; nausea and cough, attended by thirst, troubled her; the pulse

was quick, weak, and latterly intermitting; the respiration hurried; tongue and lips brown and dry; occasional delirium and coma supervened, and she finally sank on the 22d.

Autopsy.—The intestines distended by gas; their peritoneal covering, as well as that of the uterus, quite free from the effects of any previous inflammatory action. The uterus and its appendages did not present any signs of disease, but the right uterine vein, from its commencement to its termination, was much thickened in its coats, and enlarged; and when divided longitudinally, the internal surface was found in some parts of a bright scarlet colour, at others lined with a coat of lymph: coagula of blood, of a firm texture, blocked it nearly up at its junction with the iliac, forming a plug partly projecting into that vein. The left uterine vein was in a similar state, but not affected to so great an extent.

The exciting cause of the inflammation in this case was, I think, without doubt, *cold* caught whilst the vessels of the uterus were in a distended state, and their extremities open, and pouring out blood; in fact, from the time I found her lying exposed so completely to a current of cold air, I had mentioned my apprehensions of her being attacked by puerperal inflammation.

It is, perhaps, a curious circumstance that this is only the third case of that disease that I have met with amongst our parochial lying-in patients for the last nine years, although within that period it occurred to such an extent at one time in a lying-in hospital situated close to our Infirmary, that the wards were closed for some months, I believe, with a view to get rid of this fatal complaint. I find, by our reports, that no deaths have taken place from it, previously to this case, for many years.

Retroversion of the Uterus.

March 5th.—Mary Tarant, æt. 35, was admitted into the Infirmary on the 23rd of February, suffering from slight fever, and in the greatest destitution. Six months previously she had received a very severe kick on the pudenda, which caused profuse hæmorrhage for fifteen or sixteen hours, and the parts were swollen and œdematous for a fortnight after. On February 16th she fell down stairs (being pregnant), and received severe contusions on her loins and hip, followed by extreme pain for three days; during the first of which she could not pass any urine, and voided it in very small quantities even on the others, with an admixture of blood. On her admission she was bled twice, had leeches applied to the part, aperients, fomentations, and injections, by the direction of the apothecary; and then, for a week subsequently, passed about a pint of water daily. On the evening of March 2nd she voided a small quantity of urine, and from that time it has only escaped guttatim. The bowels have not been relieved since the 1st; but she has concealed both these circumstances, from a fear of having the catheter passed, and from a great dislike to medicine.

On the morning of the 4th a sudden and violent pain in the abdomen attacked her, and continued throughout the day and night, accompanied by vomiting of a greenish-coloured fluid.

On Sunday, the 5th, I was first requested to visit her, and found her in violent agony, with great tumefaction at the lower part of the abdomen, severe pain following the slightest pressure on the hypogastric region. She is in her fifth month of pregnancy, (it being exactly five months yesterday since her catamenia last appeared). She felt some slight movements of the child a fortnight since, but not for the last five days. A slight shew appeared this morning, and she *says* that she has passed her urine and alvine evacuations freely every day, and even a few hours since; but this, as was soon afterwards discovered, was a false statement, owing to the reasons before stated. Leeches were immediately applied to the abdomen, followed by a hot cataplasm, and warm injections were administered per anum. She was a little relieved by these applications, and was then removed into the lying-in ward.

On examination, per vaginam, the parts were found to be hot and swollen, and the passage blocked up by a substance which proved to be the body of the uterus. On raising this up, and carrying the hand up above the pubes, the os uteri could be discovered, and was slightly dilated, a large quantity of liq. amnii escaping with the movements. Notwithstanding the repetition of her assertion as to the passage of urine previously, I introduced the catheter, and drew off *six pints* of bloody urine, by measure. This afforded great relief, and

the abdomen collapsed considerably. An enema was then given, and she passed a large quantity of scybala about two hours after. This likewise added to her relief; but as the skin was hot, pulse full and hard, and she was thirsty, I abstracted a few ounces of blood from the arm. At 11 P.M. the catheter was again passed, and three pints of urine (still mingled with blood) drawn off. Free evacuations from the bowels at 12 and 5 A.M.

On attempting to replace the uterus in its proper position, it was found impossible; the fundus could be raised a little out of the hollow of the sacrum, but the introduction of sponges and lint, to sustain it, caused so much pain, after a short time, that they were withdrawn.

March 6th.—Slept moderately well; intense pain in the head during part of the night, and in the back and abdomen.

Hirud. xii. tempor. Low diet. Mist. Febr. Cath. ad. Oij. at 8 A.M.

In the afternoon the pain in the head still continued; abdomen painful on pressure; she was much troubled with cough: blood taken on the previous night, cupped and buffed. The uterus appeared to be rather higher up; Oiij. of urine drawn off at 5 P.M., but on attempting it again at midnight, coagula of blood only came.

7th.—Head still very painful, and the bowels not relieved since yesterday; parts very sore. The assistant-surgeon endeavoured to draw off the urine early in the morning, but was unable to get any; at 11 A.M. one pint and a half was abstracted, accompanied by much blood.

The patient had not slept, owing to pain in the right hip and side; abdomen not quite so tender, but more so over the umbilical region than any other part; tongue more moist; pulse quick.

Enema. Hirud. xii. Arrow-root diet.

8th.—Head much relieved; bowels well opened three times; slept pretty well; one gill and a half only of urine drawn off last night; abdomen much less tender; pulse quick; no thirst. There is much more space in the passage, but it is still impossible to replace the uterus in its proper position.

9th.—Small quantities of urine drawn off twice yesterday, and this morning three pints and a half, but very dark and bloody; pains similar to those of labour came on at intervals through the night; head continues well; bowels not relieved since yesterday morning; abdomen more flaccid; pulse still full.

10th.—Catheter last night brought away no urine, but this morning a pint of very offensive fluid was drawn off. Bowels not relieved, though she had taken ol. ricini this morning. Head more comfortable, and free from pain; tongue and pulse good. Another pint of urine, of a most fœtid odour, had been taken away in the afternoon; so bad, indeed, that it could not be kept in the ward, and the catheter was quite blackened by it. Bowels relieved. The uterus can now be pushed up with the fingers, but it occasions great pain, and a quantity of putrid liquor amnii escapes in doing so.

Potas. Sup. Tart. dissolved in water, with Sp. Æth. N. for drink.

11th.—Very sick all night; tongue dry; skin hot; pulse quick; has not slept; no appetite; abdomen not tender to the touch. The nurse says that the patient has passed a pint of water this morning, without aid; but, on examining it, it proved to be liq. amnii, and not bloody, as the urine has always been. Great pain and scalding about pudenda. Catheter employed, and five pints of urine evacuated.

Mist. Febr. Effervesc.

12th.—The midwife drew off a pint of water early yesterday evening, and a pint and a half at 12 P.M., less bloody and offensive. Pulse quick; feverish symptoms and pain in the abdomen returned.

Ol. Ricini. Fom. Rep. Mist.

13th.—Catheterism unsuccessful twice yesterday, but two pints taken during this morning. Patient looks sallow and emaciated. Febrile symptoms continue. Hiccough yesterday, and to-day occasionally.

14th.—Symptoms the same; shreds of mucous membrane apparently are voided with the urine.

15th.—Pulse quick, but becoming weaker; tongue white; bowels opened; a pint and a half of urine drawn off this morning. No hiccough since yesterday, but sickness is now a troublesome symptom, especially after drinking. Slept a little during the night. Pain in the back and abdomen, with great irritation about the neck of the bladder. The urine very offensive again, with pus in it. The scalding

about the pudenda is the most troublesome symptom.

From this time she gradually became worse; the pain subsided, but there was a general depression, attended by violent thirst, and occasional sickness and tremors. Small doses of opium assuaged them to a certain extent; but she died on the morning of the 19th.

Autopsy. — The pubes were cut through, and the legs widely separated, so as to afford a distinct view of the position of the parts. There was about a quart of brownish fluid in the cavity of the abdomen, and extensive adhesions by coagulable lymph had formed between the intestines and the parietes, and between the intestines themselves.

The *uterus* was seen to lie in the position before described, with its fundus fixed under the sacro-vertebral projection, and occupying the hollow of the sacrum, whilst the os uteri was felt with difficulty above the pubes. A portion of the vagina was pushed down, and the tumor at its back part reached nearly to the external orifice. The position was very similar to that represented in Boivin's Plate 11, fig. 5. The uterus was healthy in its structure, and contained very little amniotic fluid. The fœtus was partly decomposed.

The *bladder* extended up nearly to the umbilicus, and was adherent to some of the small intestines. It was immensely distended, its coats thickened, and on being cut into was found to contain a quantity of dark purulent fluid; the mucous membrane in a gangrenous state at some parts (large portions of it being easily separated by the finger), and a thick coating of lymph on others. The left ureter was much enlarged, and its coats thickened; whilst the kidney of the same side was in a state of hypertrophy, and of a granulated texture.

In this case, the accident of falling down stairs during pregnancy, seems very probably to have been the cause of the retroversion, and likewise to have produced a severe effect on the left kidney; which explains the passage of blood, from the commencement, with the urine. Nearly five days had afterwards elapsed without any urine being evacuated, and to this circumstance chiefly is the death of the patient eventually to be attributed; it was not until an examination, per vaginam, discovered the nature of the patient's complaint, that her statement was found to have been a false one. The attempts to replace the uterus in its proper position, although varied, were found to be quite inefficient; and this is not a very unusual circumstance in similar cases. Baudelocque had to wait ten days before he succeeded in reducing it, in one case; and in another of Hunter's, after death, the uterus could not be disengaged until the bones of the pelvis had been widely separated. There was no occasion to puncture the uterus, as recommended by some authors in these cases, as the liq. amnii escaped freely by the os uteri on lifting up the body.

Sloughing of Vagina from protracted Labour.

Sarah Sheen, æt. 27, was seized with labour pains at 10 P.M., on the 18th of May, which continued throughout the next day, though not of a strong character. A midwife belonging to a Lying-in Hospital was in attendance, and remained with her through the night of the 19th, when the pains became much stronger, and continued so during the 20th and 21st. The child not descending properly, a medical practitioner was called in, when the midwife took her leave. The former, with a pupil, remained with her during the night of the 21st, and until the evening of the 22d; when the child's head still remaining in the same position, and the patient becoming in a precarious state, she was sent into our Infirmary at 9 P.M.

On being called on to visit her soon after, I found her with very feeble pulse, and complaining of severe pains in her legs, which she was quite unable to move, the large nerves having evidently suffered much from the long-continued pressure on them. She had had several severe rigors after her admission.

On examination, I found the vertex presenting at the os externum, but evidently much elongated. There was sufficient space to allow of my applying the forceps, and after emptying the bladder, and giving her a little brandy and water, I made use of them, and a full-sized infant was soon extracted. A deficiency of space was found to exist between the sacro-vertebral projection and the pubes.

A severe fit of vomiting followed, attended with great depression, and I

was compelled again to give her a little weak warm brandy and water.

Tr. Opii, ℥. xl. statim, ex Mist. Camphor.

23d.—Has slept well, and complains of no pain, but is feverish. No rigors.

Mist. Febr.—Warm fomentations to the vulva.

24th.—Slept well. Has taken ol. ricini ℥j. which acted freely. Passes her urine involuntarily. Has not the least power of motion in the lower extremities.

Gentle friction, with opiate liniment, to be used to them.

On the 25th, sloughing of a portion of the vagina at its anterior part took place. Warm injections per vaginam were frequently given, and a cataplasm applied to the vulva, which was swollen. She slept well; pulse was better, and her tongue clean.

Mutton diet; but without porter.

27th.—Has not slept well; skin hot, pulse quick, tongue furred; fœtid discharge from the vagina; has more command over the left leg than she had; and can partially control the discharge of urine from the bladder.

Mist. febr.; aperients. Low diet.

29th.—Has recovered partially the use of both legs. Sloughs still separating. Less fever.

June 2d.—Has been troubled by diarrhœa for two days, which was relieved by one grain of opium at bed-time. The febrile symptoms have left her, and she sleeps well; pulse weak, tongue clean, and does not complain of pain. Small shreds of sloughing still come away with the injections, but very little discharge.

20th.—Parts healthy, but not quite healed. Has generally the power of retaining or passing her urine voluntarily; but has not yet the full use of her legs. Health and strength much improved. Discharged at her own request.

There can be little doubt but that in this case an earlier extraction of the child would have prevented the serious symptoms which followed it.

Ruptured Uterus.

I was called up on March 20th, at half-past four, A.M., by a midwife, to visit Mrs. Martin, æt. 40, residing at 19, Stacey-street. The previous history of her case was as follows:—She was at her full time with her eleventh child, and had complained for the last fortnight of severe pain round the lower part of the abdomen, especially on the left side. The severity of the pain prevented her sleeping, and was accompanied by great tendency to sickness, but there was no vomiting. Labour commenced on the evening of the 19th; and the liquor amnii escaped at one, A.M. The midwife, who was then sent for, found the pains tolerably strong, but after a short time they declined in force and frequency. At two o'clock the patient got up, and walked about the room in the intervals between the pains, and took a little weak brandy and water. The midwife was on the point of administering to her a dose of ergot, owing to the lingering and ineffective state of the pains, but fortunately changed her mind; for about three o'clock, the patient, whilst lying on the bed, was seized with a pain not apparently more severe than the preceding ones, and immediately after it, exclaimed that she was dying.

As her symptoms continued to be alarming, I was at length sent for, at half-past four o'clock; and on my arrival found her without any pulse, the extremities cold, great faintness, but no sickness; respiration difficult; countenance pallid and distressed; but she was still able to answer any questions, although it was evident that she was fast sinking.

After giving her some brandy and water, I made an examination per vaginam, and found the head presenting naturally, and low down, it not having receded, and there not having been any escape of blood. As the patient was moribund, I did not consider it advisable to remove the child. She died in a quarter of an hour afterwards.

Autopsy.—On opening the cavity of the abdomen, nine hours after death, a large quantity of blood was found in it. The uterus was fully distended; and on its left side, about six inches from the fundus, a laceration, about five inches long, was seen extending longitudinally between the round and broad ligaments, through which the extremities of the infant were visible, but no part of them was protruded. There was also a second, but smaller rent, lower down on the same side. The portion of the uterus in which the lacerations had taken place seemed rather thinner than the

other parts, and was evidently in a morbid condition. In different parts of the external surface there were large patches, in which effusion of lymph had taken place to some extent, between the peritoneal and fibrous coats. This was more distinctly visible on the right side, owing to the undisturbed state of that part. The posterior surface was healthy. The infant, on being removed from the uterus, was found to be of a large size.

The rupture of the womb is, in this case, reasonably to be attributed to the change affected in the substance of the organ by the process of inflammation, especially as the infant was of larger size than usual, and the uterus consequently more exposed to injury.

Had some active treatment been employed when the pain first attacked the patient, a fortnight before parturition commenced, the accident might not perhaps have occurred; or had assistance been rendered by the use of the forceps immediately after the rupture took place, recovery might not have been impossible.

Puerperal Convulsions.

Elizabeth Cross, residing at 80, Drury-lane, was attended by a midwife, who, alarmed at the occurrence of a severe convulsive fit during a pain, immediately sent off for me. On my arrival I learned that the patient's labour had commenced at 1 P.M. the day before, the liquor amnii escaping at an early period before the os uteri was much dilated. The pains had become strong by midnight, and continued so until five in the morning. It was at this time that the first fit occurred, and a second one took place half an hour afterwards. On examination I found the os uteri fully dilated, and the expulsive pains strong, the head presenting high up at the brim of the pelvis, and not advancing. A third convulsive fit came on soon after my arrival, and I immediately abstracted from the arm thirty ounces of blood. This afforded relief. On my return at nine o'clock, I found that the patient had had three more convulsive paroxysms; but she now seemed more sensible during the intervals than she was at my first visit. She informed me that she had not suffered from headache previously to the attack, and that she had not felt any movement of the child for some hours past. I decided, on this account, on removing the infant with the crotchet, as the passage was narrow, and the head had made no advance. This operation was unattended by any difficulty, and no convulsion occurred during its performance. Some slight attacks took place during the day; but the application of twelve leeches to the temples, and a large blister to the neck, gave effectual relief, and the patient had no further bad symptoms.

REMARKS ON CARCINOMA OF THE MAMMA, AND ON THE INEFFICACY OF SURGICAL OPERATIONS IN THE TREATMENT OF THAT DISEASE.

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CARCINOMA, as it affects the female breast, may be divided into three species.

In the 1st, the disease commences in, and involves with more or less rapidity, the whole mammary gland. At an early period, the induration, which is irregular, and of a stony hardness, is partially circumscribed, and confined to the circumference or centre of the gland, most frequently to the former. As it advances, the whole substance of the breast becomes affected, but not much enlarged; it contracts adhesions to the skin and deeper-seated parts, by which it becomes more firmly fixed, and less pendulous; the nipple is retracted; the skin puckered, and frequently traversed by enlarged veins; the cellular tissue is indurated; the affected adipose substance presents a remarkably hard and condensed structure; and the glands in the axilla and above the clavicle become enlarged, and participate not only in the morbid action, but in the structural changes of the adjoining disease.

In the 2nd species, the carcinomatous disease appears in the form of tubercle, the size of which varies from a walnut to that of a large plum. The tumor is distinctly defined; and although, in the majority of cases, it is attached to, yet in all it is removed from, and not in close contact with, the mammary

gland. It is generally situated to the outside of the breast, and sometimes as near to the axilla as the margin of the pectoral muscle. It is slower in its progress than the former species; and at one time I was led to suppose that it was more manageable by operation. I am now satisfied that it is not less malignant and destructive than the diffuse scirrhoma of the gland, and that the neighbouring parts are equally liable to be implicated in the disease.

The 3rd species is of less frequent occurrence. It commences in the form of small, hard tubercles, in the subcutaneous texture of the breast. When first discovered, they are not larger than a pea, slightly prominent, and firmly incorporated with the skin. As they advance, the surrounding cellular substance becomes tumid and œdematous, and the skin covering them of a dark brown, or deep red colour. They seldom attain to a large size; but they frequently coalesce, and form clusters of an irregular shape, which run rapidly into ulceration. These ulcers are at first superficial, but they gradually become more extensive and excavated, presenting all the characters, and attended with all the painful symptoms, of the glandular form of this malignant disease. These subcutaneous tubercles are either primary or secondary. They are sometimes, though not often, observed in the skin covering the breast, while the gland is unaffected; but more frequently they are a consequence of the glandular disease, or they are not observed until after an operation has been had recourse to. In the former case the morbid action spreads from the skin through the subcutaneous cellular texture to the glandular substance of the mamma and to the axilla. In other cases, and these by no means rare, scirrhoma of the skin, which is a very different disease from the slow cutaneous cancer so often met with in advanced life, is a secondary affection, the disease having first commenced in the substance of the mamma itself.

Mrs. I., aged 50, of a lymphatic temperament, and the mother of several children, first observed, about three months ago, two years after the cessation of the menses, several small hard points over the outer side of the right breast, about two inches from the nipple. They adhered firmly to the skin, which was not discoloured, and they were the seat of slight itching or burning pain. In about two months thereafter the whole breast was covered with them, in various stages of progression, and in size varying from a small pea to a filbert. They were most numerous, painful, and discoloured around the nipple, where they had coalesced, and were in a few points superficially ulcerated. The subcutaneous cellular tissue was thickened, as if by œdema, and traversed by branches of enlarged veins. Before death, which occurred four months after I first saw her, the glands in the right axilla became affected, and carcinomatous tubercles were developed in great numbers over the left breast, and on the anterior and lateral parts of the chest and abdomen. She died apparently from effusion into the right pleura.

On making a post-mortem inspection of the external disease, the integuments were found greatly thickened and condensed, especially around the tubercles, and the cellular tissue was loaded with a firm gelatinous substance, from which oozed a thin pale fluid. The recent tubercles were hard, semi-cartilaginous, of a greyish colour and fibrous texture; but those advanced to, or in actual ulceration, were softer, and of a bloody appearance. The mammary gland did not appear to be affected. A great number of small, defined, hard tubercles, in structure similar to those on the surface, were discovered on the right costal pleura and lung, and on the surface of the liver. The right side of the chest contained about a pound of sero-sanguineous fluid.

From the rapid progress of this case, and the fatal development of the disease in the thorax and abdomen, we are led to attribute its origin to constitutional causes. In some cases of scirrhoma of the mammary gland, the disposition to the formation of subcutaneous tubercles is exceedingly striking, the disease shewing itself, not merely in the vicinity of the affected breast to which it might have been propagated by the absorbents, but in more distant parts of the body. In two cases which I had an opportunity of watching, in the Infirmary, the disposition to malignant action was well marked. During the progress of the disease, which was considered to be too extensive for removal by the knife, leeches were applied to

the breast to diminish pain, soon after which each one of the leech-bites became the seat of a cancerous tubercle. The same malignant change in the cutaneous system, in another case, resulted from the application of leeches to the hypochondrium.

Mrs. L., aged 46, and the mother of several children, was admitted into the Infirmary on the 1st of May, 1826. Both breasts were enlarged, slightly irregular, of a stony hardness, and, especially the left one, adhered intimately to the parts beneath, and to the skin, which was in some parts slightly discoloured, thickened, and tuberculated. The left nipple was retracted, and surrounded by a hard scabby areola; and in addition to enlargement of the axillary and cervical, the integuments of the breasts, as well as those on the front of the thorax and abdomen, were thickly studded with small, hard, discoloured and painful tumors, which appeared to be situated in the subcutaneous cellular texture. The disease commenced about a year before, in the glandular substance of both mammæ, at nearly the same time; and after six months' duration the subcutaneous tubercles began to form. She left the Infirmary in the beginning of June, after having tried a variety of local applications without any benefit. Previous to her death, which occurred in about six months, ulceration of the left mamma took place, and ultimately thoracic disease supervened*. I saw other two cases in 1826, and in both the mamma was affected, and there existed the same disposition to the formation of scirrhous tubercles in the subcutaneous texture. The first patient was an emaciated, unhealthy old man, who, after having had for several months a distinct carcinomatous tumor in the left breast, was affected with painful tumors under the skin in different parts of his body, several of which proceeded to ulceration, In the other, about four months after a scirrhous mamma was extirpated, the edges of the cicatrix became indurated, painful, and tumid; but before they gave way, subcutaneous tubercles formed, and the disease proved speedily fatal. Since that time I have repeatedly met with secondary tubercles of the skin after unsuccessful attempts at extirpation, the operation appearing in all the cases to have greatly accelerated the progress of the disease.

In the following case, which is the only one of the kind I have met with, the disease appeared to commence in the thorax, so that the external tubercles which afterwards formed were secondary in their origin and importance.

In the end of January, 1831, I was consulted by Mrs. G., 48 years of age, and of a nervous and lymphatic temperament. Four months previously, about a year after her menses had ceased, and six years after the birth of her fourth and last child, she began to complain of dyspnœa, frequent dry cough, and pains in the right side of the chest. These gradually increased, and were not relieved by general and local bleeding, blisters, antimonials, &c. which were employed with great diligence by the surgeon under whose care she had previously been. About three months thereafter she observed a number of small, hard, colourless tumors on the surface of the left breast. At first these were not attended with pain, but as they increased in size and numbers the integuments got inflamed and painful. When I saw her for the first time she was confined in a semi-recumbent position, and was much emaciated. Her countenance was anxious, and of a dark leaden colour; respiration rapid and laborious; cough urgent, but without expectoration or much pain; limbs anasarcous; urine scanty; bowels and stomach irritable; tongue dry and chapped; pulse 120, feeble. She complained of a sense of fulness in the right hypochondrium, and of pain in the back, with leucorrhœal discharge. The liver was found enlarged, and slightly irregular, and the os uteri open, thickened, and indurated, but not irregular or ulcerated. The integuments on the front of the chest, upper part of the abdomen, right side of the neck, from the clavicle to near the angle of the jaw, and on the upper and anterior part of the left thigh, but especially those over the mammæ, were studded with scirrhous tubercles, some of which were superficially ulcerated, had a livid appearance, and discharged a thin bloody fluid.

On examining the right side of the chest, the sound on percussion was dull, and the respiration was imperfectly heard. In some places a curious grat-

* See Clinical Reports, p. 44.

ing noise was recognized, not unlike that produced on the motion of a sprained joint, when fibrin has been effused among the tendons. She died in the end of February, about five months from the commencement of the disease.

On dissection, the structure of the external tumors, and the condensed state of the cellular tissue surrounding them, were similar to what has been described in the last case. The right cavity of the pleura contained sixteen ounces of turbid serum, and a few recent adhesions existed between the upper lobe of the lung on that side, and the parietes of the thorax. The lung was almost entirely covered with hard opaque tubercles, which were seen and felt projecting beyond its surface. In some places these were of irregular shape, and had coalesced; and the portions of the lung surrounding them were condensed and more vascular than natural. The pleura costalis of the same side was enlarged, and had imbedded in it a number of small, round, flattened tubera, of different sizes. The os uteri was apparently in the first stage of scirrhous induration, and the body of that organ contained a carcinomatous tumor the size of a chesnut.

This case strikingly illustrates the constitutional origin, rapid progress, great extent, and unmanageable character of this species of carcinoma. From its destructive progress we observe how completely the system was under the influence of the malignant action, and how utterly hopeless were any means we could employ to arrest its progress. In none of the cases I have seen, even at the outset of the disease, when the tubercles were confined to the skin, could an operation have been undertaken with the slightest prospect of success. The moment attempts are made, either by the knife or by caustics, to remove or destroy these superficial tumors, the more rapidly are they regenerated, and the sooner are the vital organs implicated.

Sir A. Cooper, in describing the progress of a carcinomatous mamma, states that the cellular membrane becomes inflamed and hardened, and little tubercles form in the absorbent vessels under the integuments*. I cannot suppose, from the peculiar shape and scattered position of these bodies, whether occurring in the integuments of the breast as primary or secondary formations, that they are at all connected with enlargement of the absorbents. When these vessels are the seat of simple or specific inflammation, the subsequent induration is rarely if ever confined to small, isolated, circumscribed, and scattered points, but extends along the affected vessels in continuous lines, and presents appearances very different from those observed in the development and progress of subcutaneous tubera.

Sir Charles Bell, in alluding to this form of the disease, seems to consider that the glandular substance of the mamma is its primary seat, and that the affection of the skin is only secondary. He also affirms that the *glandular texture* of the skin is the peculiar seat of the tubercles, and that they most frequently commence in the dark areola around the nipple*.

After frequently watching the rise and progress of the tubercles, and examining them carefully after death, I am inclined to believe that the carcinomatous inflammation, which is generally of a chronic character, begins in, and is confined to, one or two cells of the subcutaneous tissue, and that these become filled with the solid matter of the disease. In this way we can explain more satisfactorily their peculiar shape and irregular distribution; and, besides, we know, from recent pathological investigations, that the cellular tissue is one of the principal seats of this malignant disease.

In all the varieties of mammary carcinoma, after the axillary glands have become diseased, the arm of the affected side is liable to swell and become painful. This colourless intumescence generally shews itself at, or immediately above the elbow, and extends to the fore-arm and hand; but sometimes it begins in the dorsum of the hand, and gradually spreads upwards until it involves the whole limb, and ultimately extends from the axilla and shoulder to the integuments of the thorax and abdomen. The swelling from its commencement feels hard and brawny, and seldom pits on pressure; and it gradually increases until the arm is enor-

* Lectures, by Tyrrell, vol. ii. p. 178.

* Medico-Chirurgical Transactions, vol. xii. p. 219.

mously enlarged, and the power of motion destroyed. It gives rise to severe constitutional excitement and violent pain, and rarely fails in accelerating the fatal event.

It is generally supposed that this swelling depends on the loss of absorption, from the combined effects of pressure on the axillary veins, and destruction of the normal texture of the lymphatic glands. It is extremely probable that both of the causes may operate to a certain extent in producing and maintaining this hypertrophied condition of the cellular tissue, especially at an early period; but it is difficult to see how these morbid changes alone can account for the subsequent progress of the swelling. Although the axillary gland be obliterated by the pressure of the enlarged glands, yet the venous blood from the arm may be transmitted through other channels, without any infiltration of the extremity taking place. I have twice seen this vein tied, after having been accidentally wounded in the removal of tumors from the axilla, without the venous circulation of the arm having been much affected. Besides, we sometimes find this enlargement of the upper extremity during the progress of a carcinomatous mamma, when the glandular disease in the axilla is not so great as to obstruct the vein, or materially interrupt absorption.

I was led, from the resemblance which the swelling of the upper extremity bears to phlegmasia dolens, to suppose that it is, in a great measure, referrible to phlebitis. It has been ascertained that inflammation of the veins of the uterus has resulted from cancerous ulcers of that organ, and phlegmasia dolens been produced; and it is surely not unreasonable that the veins of the mamma and axilla may be affected from a similar cause. I have not been able to find on record any dissections to illustrate this point, but I have met with two cases in my own practice which establish the accuracy of my conjectures.

A married lady, 52 years of age, and of a sanguineous temperament, had been subject to diffuse carcinoma of the right mamma for about eighteen months before it ulcerated, which occurred in the beginning of February 1836. The breast was enlarged, and the integuments were indurated and traversed by enlarged veins, presenting that form of the disease which has been called carcinoma œdematodes. In two months the ulceration, which took place at the outer edge of the breast, had extended to the size of a dollar, and in depth about half an inch. At this time the subcutaneous cellular tissue of the arm, immediately above the inner condyle, began to swell, and the swelling gradually extended in all directions, accompanied with numbness and pain. The integuments of the axilla were indurated, and the glands, although much enlarged, were still moveable. She had frequent rigors, occurring at irregular periods, which were always followed by an increase of the general excitement; she began to lose flesh rapidly; her countenance was pale and anxious, with a slight leaden hue; she complained of increasing pains in the breast and arm, and in the loins, thighs, and shoulders, with occasional dyspnœa and cough.

On the 4th of May I discovered a large, hard, irregular cord, passing from the axilla about three inches down the arm, in the direction of the brachial vessels, and two deeper seated bands, of smaller size, passing towards the affected mamma; they were painful on firm pressure, and the integuments covering them, although indurated and œdematous, were not inflamed.

On the 28th of June I found her confined to bed from increasing exhaustion, and from the unwieldy state of her arm, which was enormously distended, and lay immoveable by her side. The whole limb was involved in the swelling, which also filled the axilla, and extended above the clavicle, and along the side of the chest and abdomen to near the spine of the right ilium. She complained of acute deep-seated pain in the left thigh, about three inches below the trochanter. The muscles were here much wasted; but a distinct swelling, as if from chronic enlargement of the periosteum, was recognized.

When attempting to turn herself in bed, on the morning of the 14th of July, the left femur was suddenly fractured at the affected part by the action of the muscles. The fractured ends of the bone admitted of free motion without much pain or distinct crepitus; and several days elapsed before any decided retraction of the limb took place. A bandage was applied, and the limb

place on an inclined plane made of pillows. She died on the 9th of October.

On inspection, the subcutaneous cellular tissue of the arm, shoulder, axilla, side of the chest and abdomen, was filled with a solid substance, giving the membrane the appearance of being excessively hypertrophied; and the same morbid enlargement of the inter-muscular cellular tissue was observed, both in the arm and fore-arm. The carcinomatous structure of the mamma, and of the axillary and supra-clavicular glands, was distinctly marked, and was in various states of progression. The pectoral muscle was involved in the disease, and its fibres separated by deposits of solid carcinomatous matter. The deep-seated veins of the affected breast, to their termination in the axillary, the axillary vein itself, the brachial to near the elbow, and the outer two-thirds of the subclavian, were obliterated, their coats being thickened; and they contained in some places matter of a greyish colour and granular appearance, and in others a purulent looking fluid, mixed with blood and flaky matter. This was found in greatest quantity close to the sound part of the subclavian vein, where, from the highly vascular appearance of the vessel, recent inflammation appeared to have existed. In two or three places there were irregular masses of solid matter, of a carcinomatous appearance, in contact with the affected veins.

The fracture of the femur was not united, nor did there seem the slightest tendency to the formation of callus. The surrounding muscles were glued together, of a pale red colour, and firm, almost cartilaginous, in texture. About two inches of the upper end of the fracture, and nearly an inch of the lower, were absorbed, and had an irregular spiculated appearance. The bone was softened to a still greater extent, and scirrhous tubercles were found on its cancellated structure. The lungs and liver contained several tubera of a similar texture; and mixed up with these, especially in the lungs, were small superficial deposits of pus, alone or joined with the solid matter of cancer.

In this case I am inclined to believe that the œdematous swelling was in a great measure caused by the phlebitis. This probably commenced in the deep-seated veins of the mamma, in connexion with the ulcer, and slowly extended to the axilla, and then, of course, it partook of the malignity of the disease in which it originated. This was obvious, both from the peculiar appearance of the solid matter with which the veins were plugged, and from the distinctly cancerous structure of the tubercular deposits exterior to them. While this morbid state of the veins was progressing slowly, and giving rise to local changes, the malignant disease which first appeared in the mamma was developing itself in the lungs, liver, and osseous system. Ultimately a portion of the subclavian vein appears to have become more actively inflamed, from whence probably pus, mixed with cancerous matter, was poured into the circulation, occasioning the purulent deposits in the lungs, and accelerating the fatal event. I think we are entitled to consider that this inflammation of the veins is of a specific character, and that in this way cancerous action may be propagated, and cancerous matter transmitted into the system, as readily as we acknowledge it to be by the absorbents.

This œdematous enlargement of the arm does not often commence until after the mamma has ulcerated, even although the glandular disease in the axilla be extensive, which tends still farther to confirm the opinion that it depends on inflammation of the veins. In the following case phlebitis originated in the mamma, after a central portion of the disease had begun to soften and break up, but before external ulceration actually occurred.

Mrs. W., a sallow unhealthy looking woman, 59 years of age, was admitted into the Royal Infirmary under my care, on the 9th November, 1835. The left mamma was enlarged throughout, very hard and irregular, and the seat of severe lancinating pains. Towards its outer edge there was a round prominent point about the size of a walnut, which was much softer than the other parts of the swelling, and covered with integuments of a dark red colour. The superficial veins were numerous and prominent, and there was a large moveable gland in the axilla.

Having declined to operate, she left the Infirmary in a few days after her admission, but I had an opportunity of frequently seeing her afterwards at her

own house. In the beginning of December she began to complain of frequent rigors, followed by profuse perspirations, increase of pain in the breast, and great exhaustion. On the 4th, I found several of the superficial veins near the softened portion of the tumor, inflamed, hard, and painful on pressure. In a few days almost all the external veins, as well as those in the axilla, became affected. By the application of leeches, and the internal use of calomel and opium, the mammary phlebitis and the constitutional symptoms abated. Nearly a month elapsed before I saw this patient again, during which time she continued to complain of violent pains darting along the left arm, which were increased on motion and by pressure in the axilla. The axillary, and nearly four inches of the upper end of the brachial vein, were nearly as large as the little finger, and very hard, but not particularly painful. The softened part of the mamma had terminated in a foul corroding ulcer. The arm was œdematous, the swelling having commenced in the dorsum of the hand. She died on the 7th of April, apparently exhausted by the pain and immense swelling of the arm, without any decided symptoms of visceral disease having shewn themselves.

On inspection, the cellular tissue of the arm, both subcutaneous and intermuscular, as also of the left side of the chest, was immensely thickened; and from some parts of it a good deal of serous fluid was discharged. The mammary gland, and the diseased absorbent glands in the axilla and above the clavicle, shewed the carcinomatous structure very distinctly. The superficial and deep-seated veins of the breast were converted into impervious cords, and in various places were connected with masses of diseased glands, and tumors in the surrounding cellular tissue, of a scirrhous character. The axillary and brachial veins, to near the elbow, and about an inch a half of the commencement of the subclavian, were obliterated, their cavities being filled with a firm, apparently organized substance, of a greyish or bloody appearance. Both lungs, and the costal and pleura in both sides of the thorax, contained a number of small opaque tumors.

These cases shew that inflammation may attack the veins of a cancerous mamma, and gradually extend to the axillary and brachial vessels, so as to obliterate them; and, by impeding the return of blood, cause gradual but extensive hypertrophy of the cellular texture of the arm. This carcinomatous phlebitis, if it may be so called, would appear, from the two cases detailed, to be of a chronic character; but I have lately heard from a medical friend, that he had met with a case, a short time ago, in which the same form of disease proved rapidly fatal. In fact, we may expect to find that specific mammary phlebitis, although it may greatly accelerate the progress of the malignant disease, increase or complicate the sufferings of the patient, and lead to the more rapid introduction of cancerous matter into the system, may yet remain quiescent for a considerable time, or be partially arrested, and not prove so very rapid in its progress, or so immediately destructive in its consequences, as the more acute form which we meet with after surgical operations.

[To be continued.]

ABDOMINAL TUMOR

OPENING THROUGH THE INTEGUMENTS COMMUNICATING WITH THE ILEUM.

By Geo. Freckleton, M.D.

Fellow of the Royal College of Physicians, London.

Jan. 13th, 1838.—About twelve months ago I was consulted upon the case of a lady, aged 54, who had for many years, at different intervals, been under medical treatment, and who stated that the general opinion had been, that she had an affection of the liver. During the past year she had suffered much from prurigo podicis, which, after resisting many remedies, had yielded to the Ung. hydrargyri. In the summer, while on a visit to her friends in Ireland, she had a severe attack of vomiting and purging, and was so much reduced in strength, that she had been sent home, as she expressed herself, to die in her own house. She was married, and had had one child many years ago. Immediately before I saw her she had laboured under pain and tenderness in the abdomen, relieved by leeching, &c. Her bowels had generally been constipated, and for years she had been accustomed to take strong purgatives, and to use enemata. On examination, I

found the abdomen enlarged and tense, and I could readily detect several hard tumors in different parts of it. One was situated in the left, and another in the right hypochondriac regions; and in each iliac region a hard oval tumor was perceptible; that on the left side was the largest, which appeared to extend into the pelvis. These tumors had been gradually enlarging for the last fifteen years. The liver did not appear altered in size. The umbilicus was inflamed and prominent; and directly under it was a small opening, which had first made its appearance during the preceding night, and through which a considerable discharge of purulent and bilious matter had flowed, and still continued. I attended her for some time, and the progress of the symptoms varied little. Occasionally the different parts of the abdomen corresponding to the tumor became painful to the touch, and more tense; and leeches were applied. Her strength and spirits also varied; and when depressed, a more generous diet and tonic medicines produced a favourable effect. Her appetite was generally good, and only slight constitutional irritation was at any time present. After some months she seemed gradually to improve in health, and was able to take exercise in the open air. I did not keep any regular notes of the case, for my attendance varied. In the course of it, however, two more openings in the neighbourhood of the umbilicus made their appearance, and through these purulent matter flowed sometimes, in considerable quantity, especially on slight pressure. Several times I remarked, when she became uneasy, depressed in spirits, and restless, that after a few days' continuance of these symptoms they went off again upon the appearance of a large bilious discharge through the original opening, which had first appeared near the umbilicus. She seemed to gain flesh and strength during the summer, and though complaining of thirst, and occasionally of want of sleep, she did not appear to suffer from any particular pain, constitutional irritation, or febrile symptoms. The alvine evacuations were copious and natural; and she now seldom required any, and then only slight aperient, medicines. About ten or twelve weeks before her death she called my attention to a small hard substance which she had found lying upon one of the openings, and which looked like a portion of calcareous matter; but I doubted whence it had come, as she had been applying poultices to the part, which had become sore and excoriated by the constant discharge, which had continued from the first appearance of an opening. But about a fortnight afterwards I was sent for, and on examination found two sharp hard points projecting from an opening above the umbilicus; and upon pressing them, they appeared to be the extremities of some hard body lying under the thin layer of skin which separated the two orifices of the opening. Upon drawing a lancet across this and dividing it, I extracted a substance which at first view much resembled a piece of carious bone, and which, upon analysis, proved to be composed of phosphate of lime. I supposed that it might have proceeded from the ribs, but I could not detect caries either of these or of the spine. The opening looked healthy, and no particular symptoms followed. Ten days before her death violent sickness came on; and though I succeeded in alleviating this, she gradually sunk, and died without pain, perfectly conscious of her situation.

About three days before her death, I should state that a small quantity of stercoraceous matter had passed through the opening under the umbilicus. Some substances resembling partially digested food, mixed with dark-coloured bile, had occasionally passed at intervals before, but never in any quantities, or regularly. I may also remark, that often, upon making pressure in its vicinity, an escape of air would take place through this opening. The case was obscure; and once during the summer Mr. Bickersteth saw her, at the request of her friends, in consultation with me.

The day after her death we examined the body, and found the abdomen diminished in size, and much flatter than I had ever observed it during my attendance upon her. Though it had gradually, during the latter end of the summer, become less distended and hard, the integuments assuming a more natural appearance, and moving freely upon applying the hand, which was not the case in the first instance; but I never could detect any fluctuation.

Upon dividing the integuments, we found that the intestines adhered in many places to each other, and to the

peritoneum; and that the opening under the umbilicus communicated with one of them, which proved to be the ileum, which was perforated about the middle of its length, and through this opening the bile and other matters had passed. The liver was healthy; the gall-bladder full. There was no serous effusion. On the left side of the abdomen was a large oval cyst, of a dark greenish colour, containing a quantity of pus, and nearly filled with bony concretions of very irregular figure, and of different sizes, but of the same character as that which had passed through the opening during life; and upon introducing a probe, it passed freely from the superior external opening into the cyst. The pelvis was completely packed with a mass of tumors; these were connected with others in the cavity of the abdomen by peduncles of cellular texture, and admitted of being easily detached from each other. We carefully removed the whole mass of these and the uterus, which was much diminished in size in consequence of the pressure, and found the tumors to be attached to the fundus of the uterus, but merely by cellular texture. They were in number about thirteen, and nearly filled a wash-hand basin. They were of three distinct characters, several being decidedly carcinomatous; others containing pus and bony concretions; and some few containing fluid-like hydatid cysts. They were of different sizes and shapes, but generally oval or round, varying from the magnitude of an ostrich egg to that of a marble; and attached to each other by cellular texture, generally in a peduncle. There was scarcely any vestige of the proper structure of the ovaries; they, as well as the Fallopian tubes, being small in size, and degenerated chiefly into cellular texture.

Independent of the extraordinary character of this case, it is interesting as affording another example of the difficulty of accurately ascertaining the nature of abdominal tumors during the life-time of the patient. Many of the symptoms would, I think, have warranted the opinion that in this case there was disease of the liver, and that an abscess of that visus had discharged itself by the opening first observed. At the same time it was evident that tumors were present in the cavity of the abdomen; and from the situation of some of these, suspicion might be entertained of disease of the ovaries; whereas, a post-mortem examination shewed that the tumors were independent of both these organs, and connected only with the uterus by a peduncle of cellular texture similar to the connexions existing between the several tumors forming the mass.

Liverpool, May 16, 1838.

GLASS BEAD IN THE AIR-PASSAGES—TRACHEOTOMY.

To the Editor of the Medical Gazette.

Sir,

Your insertion of the following case of successful tracheotomy will, I hope, induce other medical men to have recourse to it promptly on a similar occasion.

Some years ago I witnessed the death of a fine child, 3 years of age, in consequence of the delay in the operation, in which the symptoms were very similar to those narrated below, and were occasioned by a small plum-stone slipping into the trachea whilst the child was lying on its back.—I am, sir,

Your obedient servant,

William Gibbon,
Member of the Royal College of Surgeons.

Kettering, May 12, 1838.

In January last, a little girl, aged 5 years, whilst playing with a glass bead in her mouth, accidentally swallowed it.

She was immediately attacked with a distressing cough, which soon led her friends to fear that the bead had "gone the wrong way," as it is called: after this cough had continued about two hours, the child became easier until she attempted to lie down, when the paroxysm returned with great violence, and the poor child was much distressed, crying out that the bead stuck in her throat, and would choke her. When I saw the child on the following morning, I felt confident of the nature of the case, and had so little expectation of her obtaining relief from her own unassisted efforts, that I should have operated directly, had it not have been suggested to me, that as the child was so comfortable for long intervals, when in the erect posture, the bead might possibly make its way into the substance of the lungs, and so find an outlet, or cease to occasion irritation. I acceded

to this proposal, that I might not appear to be rashly eager to cut the poor child's throat. After waiting three more nights, the mother begged that I would have recourse to an operation, if it afforded any prospect of relief, as the child's sufferings daily increased.

On the morning of the fourth day after the accident, I operated in the way recommended by our standard surgical writers. The thyroid gland was large, and bled rather freely, which was the principal cause of delay in the operation, as I was anxious to avoid every source of irritation after opening the trachea.

When the bleeding had ceased, I slit open the windpipe, immediately below the cricoid cartilage, to the extent of full half an inch; the air rushed out with great violence, and much cough was produced, but the bead was not expelled until I held the sides of the aperture widely open with two hooks. I first tried forceps, but could not hold open the rings with them.

The bead was coughed out in a few seconds after the aperture was *well expanded*. I lay a great stress on this part of the operation, as it appeared to me that no effort of the child's would have expelled the bead until this was done. The child rapidly recovered, and the wound healed favourably.

ON THE CHEMICAL NATURE OF THE EXTERNAL ENVELOPE OF THE FROG'S SPAWN.

By Alfred Smee,
Student at King's College, London.

The nature of the envelope of the spawn of the frog does not seem to have been investigated chemically. Some authors stating generally that it is of an albuminous, others that it is of a gelatinous nature. To me, however, it appears to be neither of the above substances, but rather a form of mucus, as the following observations shew:—

1. When the envelope is separated from the ova it gradually sinks in water.

2. It is not soluble either in hot or cold water, but swells considerably when first placed in it, after which it may be dried without losing any of its former properties, for on the addition of water it will again swell to its former size and appearance*.

3. When portions of the external covering were boiled in water it was not dissolved, nor was their any coagulation.

4. When the spawn was treated with nitric acid there was no coagulation; but after a short period the external envelope was dissolved, leaving the ova.

5. Sulphuric acid had the same action on the spawn as the nitric.

6. Muriatic acid, like the preceding acids, did not coagulate, but dissolved the envelope.

7. Acetic acid, even when pretty strong, produced little or no effect on the spawn.

8. The action of the oxalic acid was found to be the same as the acetic acid.

9. The envelope was then treated with a solution of caustic potassa, when it was dissolved.

10. Ammonia produced apparently no effect on the envelope.

11. When the ova were treated with alcohol no coagulation ensued, nor was the envelope dissolved.

12. Tincture of galls was also found to produce no effect on the envelope.

13. When the ova were placed in a solution of prussiate of potass, no coagulation or other change ensued.

14. When the envelope was placed in a strong solution of corrosive sublimate in alcohol, no change took place.

15. Solutions of muriate of ammonia and common salt produced no change.

16. The solution of the envelope in nitric acid was treated with ammonia, when it was not again reprecipitated.

17. The solution was then treated with potash, when the result was the same as before.

18. A solution of tannic acid gave a white precipitate with the acid solution.

19. A solution of gallic acid failed to give a precipitate with the solution.

20. When the acid solution was neutralized and treated with ferrocyanate of potassa, no change ensued.

21. The acid solution was found to

* It does appear, after a long time, to be dissolved, as after five or six weeks; but probably here decomposition may, in some manner, alter its nature.

give no precipitate with acetate of lead.

22. The gelatinous, or rather the mucous envelope, was found to be neutral, neither changing the colour of litmus paper to red, nor restoring reddened litmus paper; and it was also found to have no action on turmeric paper.

From these experiments it is decidedly proved that this covering is neither albuminous nor gelatinous; for had it been the former, many of the above tests would have coagulated it; and had it been the latter, water would have dissolved, and tincture of galls had a sensible effect on it, &c. The nearest animal product, then, which it approaches in its general characters is, mucous, which is known to differ in different situations of its secretion.

This mucous envelope is found to be a product exclusively of the oviduct; for the ova, at no period of their existence in any way possess it; the proof of which is obtained from killing a frog just before spawning is expected, when the ova in the ovaries will be found to have no covering, and when placed in water do not swell out. Those ova, however, which have found their way into the oviduct, possess the mucous covering, and when placed in water do swell out. The oviducts do not form the mucus suddenly when the ova are about to be discharged, but keep forming it for a considerable period, as I have found the oviducts to be distended with it six months at least previous to the period of spawning, and even immediately after the ova have been discharged are not entirely free from it, though they are much contracted in size.

That the substance contained in the oviducts and the envelope of the ova, after their discharge is identical, is proved by the following experiments:—

1. When a portion of the oviduct was placed in water it swelled, as the envelope when first excreted; and if a frog, before spawning, be killed and cut open, and then placed in water, the contents of the oviducts will swell to an almost incredible size.

2. When the contents of the oviducts were placed in acid they dissolved, and did not coagulate.

May 5th, 1838.

REMARKS ON MAMMARY ABSCESS.

To the Editor of the Medical Gazette.

Sir,

By inserting the following observations in an early number of your journal, you will oblige,

Your obedient servant,

K.

April 1838.

Mammary abscess is a complaint which is sufficiently often met with in practice, and one which not unfrequently proves very troublesome both to patient and to practitioner. Cases are now and again occurring which set all remedial efforts at defiance. One abscess, for example, forms, and this has no sooner discharged its contents than a new one appears; and in this way a succession of such abscesses arise, reducing very much the powers of the system, and in some cases bringing even life itself into jeopardy. I believe that the means hitherto adopted in the disease in question exercise but little control over it. I would even venture to say, that part of the usual routine of practice is not only useless, but positively injurious. The means I have in my view at present, and which I deem specially reprehensible, are poultices, and sucking the affected breast, either by natural or artificial methods. But before entering upon further therapeutic details, it may not be amiss to state shortly the views which we entertain respecting the pathology of the disease, the treatment of which we have selected for a few observations*.

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* The minute anatomy of the human mammary gland is, I believe, still a desideratum. The general opinion is, that the milk is secreted in the most remote parts of the gland, in a structure supposed to be of a vesicular form. The milk which is thus supposed to be secreted only by the most remote parts of the gland, is poured into tubes, called lactiferous, which terminate in ducts ten, twelve, or more, in number, opening on the nipple. I have not had many favourable opportunities of examining the mammary gland of the human female, but have paid some attention to it in the cow, being much larger, and more easy of examination in this animal than in the human subject. I am quite aware that corresponding parts in different orders of animals may differ considerably in general appearance and structure; but I am inclined to think that, as respects the gland in question, its general structure and economy is pretty much the same in the

Different and not very satisfactory views of the pathology of mastitis (which term we consider synonymous with mammary abscess), seem to be entertained by different writers. Some authors tell us that it is primarily an inflammatory affection of the lactiferous vessels. Others, that an obstruction to the free egress of the milk from these tubes is the general cause of mastitis. Others suppose that the inflammation originates in the cellular membrane connecting the lobules of the gland. Another class of practitioners suppose that the disease, in some cases, begins in the interior of the lactiferous tubes, while in others it begins on their exterior. There can be but one opinion as to the importance of acquiring correct views of the pathology of the disease in question. Much malpractice, I apprehend, has resulted from indefinite or erroneous views respecting this subject. Those, for example, who consider that mastitis results from obstruction to the free excretion of the milk, are, I believe, almost always mistaken; and this theoretical error naturally suggests a practice which is generally hurtful in place of being salutary. Some will say, cautious sucking of the breasts can never produce mischief; but the operation, even when very gently performed in mastitis, is painful, and generally aggravates the inflammation and swelling it was intended to allay. In such cases, moreover, how little milk we succeed in bringing away must have been remarked by all who have paid particular attention to the subject. I am quite aware that some females have occasionally such an abundant secretion of milk that the breasts become engorged, and decided relief is obtained by emptying the lactiferous tubes, either by the natural process of sucking, or by artificial suction. It is this state of the breasts which sometimes ends in what is called milk abscess, a very absurd term, in my opinion, for a simple accumulation of milk in a preternaturally dilated part of one of the lactiferous tubes. This state of lacteal engorgement of the breast may properly be treated by occasional suction; but it is a very different affection from mastitis, or inflammation of the mammary gland, which so often terminates in abscess. Simple lacteal engorgement of the breast very rarely, indeed, gives rise to an inflamed state of the mammary gland, and I believe, if left to itself, nature would in due time remove the engorgement.

In mastitis the inflammation, I believe, primarily affects the tissue immediately exterior to the lactiferous apparatus. (By lactiferous apparatus, I mean the glandular structure seated immediately beneath, and opening on, the the mucous surfaces called milk tubes.)

The first effect of the inflammation, as in most other compound glands, is to diminish the secretion of the organ affected. The mammary gland in incipient mastitis, especially when the deeper parts of it are affected, presents a very peculiar feel to the hand when examined. One or more irregular knobs, and occasionally swellings of considerable size, hard, and irregular, and painful to the touch, are felt at affected parts of the gland. These irregular swellings frequently present such a feeling as to lead the practitioner to fancy that they consist of tubes distended with milk. This deceptive feeling has, I am convinced, led many to recommend sucking to remove the supposed lacteal engorgement. In such instances the operation in question must prove detrimental, and appears to us as irrational as it would be to expose an inflamed eye to a glare of light, or to irritate an inflamed kidney by strong diuretics.

In mastitis, by what agency the secretion of milk is diminished is not known, any more than why other glands have their secretions lessened by acute inflammation. It is sufficient that we know it to be a fact established by repeated observation. It is a fact, moreover, which should be borne in mind by every one when called upon to prescribe for the disease in question.

I believe observation will be found to confirm the view which we have taken of the pathology of mastitis, or mammary abscess. Here, as in every other disease, it will be found that the

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cow as the human subject. If I am correct in this view, the statements of preceding writers will prove to be somewhat inaccurate. From my observations of the mammary gland of the cow, I infer that the structure secreting the milk is diffused through the whole gland, from the very beginning to the termination of the lactiferous tubes. This structure secreting the milk, consists of small compound glands, of a yellowish colour, seated immediately under the mucous canals, called milk tubes. These glands open by minute ducts into the milk tubes. I conceive the structure of the human mamma is similar to this. Anatomists can easily decide this point.

more correct our pathological views are, the more rational and successful are our therapeutic measures likely to be.

We have already stated that the common methods of treating mammary abscess have generally little control over the disease. It is painful to see (as we have too many opportunities of doing) females harassed by a succession of such abscesses, enduring much suffering, while practitioners are forced to stand by and witness the inutility of their means for staying the progress of the disease. Seeing, then, the inefficacy of the ordinary plans of treating this annoying complaint, the promulgation of any means likely to enable us to exercise more mastery over it, must prove acceptable to the profession. With this impression I have little hesitation in coming forward to recommend a new plan of treating the complaint, simple in its nature, safe and easy of application, and more efficacious, I venture to say, than any single remedy hitherto proposed. It is not of a very imposing character, consisting of nothing more than a common roller, properly applied. I do not advance it as a panacea, or as suited to every case and all stages of the disease. Were I to do so I would be sure to bring it into unmerited neglect, for much mischief is frequently done by overrating the virtues of the remedies, and by not sufficiently discriminating the kind of cases to which certain remedies are more especially applicable. Even at the present time our periodical medical literature shews that there exists in the profession an immoderate love of novelties, and an ignorant desire of discovering remedies of specific virtues, or of something like universal application. Thus we find several new forms of medicine of late pompously blazoned forth in our journals as certain remedies for different complaints. Again, some gentlemen, finding a certain medicine, or combination of medicines, very useful in some cases of disease, begin to give it something like universal reputation. This indiscriminate way of vaunting remedies is sooner or later injurious both to the authors and their remedies. In medicine, useful facts are not accumulated without labour and considerable application. Neither the laws which regulate the production and progress of diseases, nor the means applicable for their prevention and cure, can be expected to be discovered at random. Much circumspection—an ardent desire after truth, and truth only—a determination not to be misled by excited fancy, or immoderate self-love—are requisite in those from whose labours we may expect to see medical science advanced, and the healing art improved.

Before proceeding to discuss more particularly the plan of treatment which we propose for mammary abscess, we may take a hasty glance at the usual plans of treating this complaint. It must be confessed, that this disease is not treated of with that fulness which might have been expected from the frequency of its occurrence, and the urgency and obstinacy of its symptoms.* In Dr. Burns' system of midwifery we find, perhaps, the best account of its symptoms and progress, and the fullest and most satisfactory enumeration of the means usually employed in treating it. We shall, therefore, content ourselves with a few extracts from this author:—"It is a very difficult thing," says Dr. Burns, "to prevent inflammation of the mammary gland from ending in suppuration. It is to be attempted, however, by purgatives, and the application of a tepid poultice of bread and milk, or cloths moistened with tepid water. If there be only a little diffused fulness, with some degree of pain, gentle friction with warm oil is useful. If the breast be distended with milk (but I would ask, how are we to know certainly when this exists), it will be proper to have a little taken away occasionally, when this can be done easily and without increasing the pain. [A very proper proviso this, but better, I think, to proscribe the practice altogether.] When the pain becomes throbbing, a warm bread-and-milk poultice is proper to assist the suppurating process. After the induration has abated, and matter is formed, it ought to be freely let out by an opening of sufficient size, provided there be no appearance of the abscess bursting soon of its own accord. . . . After the abscess bursts, or is opened, there is for some time a discharge of purulent matter, which frequently is mixed with milk; then the surrounding hardness gradually abates. The poultice may be continued for several days, as it promotes the absorption of the in-

* In Velpeau's work on Midwifery, and in most of the continental works on the diseases of females, it is very briefly noticed.

durated substance. [I think this statement very questionable.] It sometimes happens, if the constitution be scrofulous, the mind much harassed, or the treatment not at first vigilant, that a very protracted and even fatal disease may result. The patient has repeated and almost daily shivering fits, followed by heat and perspiration, and accompanied with induration or sinuses in the breast; she loses her appetite, and is constantly sick; suppuration slowly forms, and perhaps the abscess bursts, after which the symptoms abate, but are soon renewed, and resist all internal and general remedies. On inspecting the breast, at some point distant from the original opening, a degree of œdema may be discovered, a never-failing sign of the existence of deep-seated matter there; and by pressure, fluctuation may be ascertained. Poultices bring forward the abscess, but too slowly to save the strength, and, therefore, the new abscess, and every sinus which may have already formed or existed, must be, at one and the same time, freely and completely laid open; and as soon as the new gland suppurates the same operation is to be performed." This is terribly severe practice; and before having recourse to it, humanity would make me try the effect of intermitting poulticing, and of applying well-regulated pressure to the breast by means of a bandage. But more of this hereafter.

Dr. Burns has said nothing of bleeding; but it, and the means recommended by him, are too often found inefficacious, or positively hurtful. Not long ago, Dr. Kennedy, convinced of the inefficacy of the usual plans of treating mammary abscess, proposed tartar emetic in nauseating doses, as a remedy for this complaint, and adduced several cases in which he had tried it with success. Although I do not by any means intend the remark to apply to Dr. Kennedy, yet I must be allowed to state, that cases are often published upon which little reliance, as respects the operations of medicines, can be placed. This is especially the case where an enthusiastic mind, actuated as it may be by the best of motives, but displaying little discrimination, sets itself to the task of sppporting a particular line of practice by a one-sided appeal to cases. Such a mind acts in respect to the selection of cases, just as a person does in respect to facts, who has a particular hypothesis to support. Both parties see with great facility what they wish to see, and overlook with equal facility what may not be exactly suited to their views. I do not mean to deny the occasional utility of tartar emetic in mammary abscess. Although I have tried it myself without success, other practitioners have found its effects more in accordance with the statements of Dr. Kennedy. Thus, in a late communication to the London Medical Gazette, it is very favourably spoken of. I see no reason why it may not continue to receive further trial. Perhaps the circumstances under which it acts most favourably, or the causes to which it is more especially applicable, might thereby soon be determined.

[To be continued.]

MEDICAL GAZETTE.

Saturday, May 26, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

POISONING BY MISTAKE.

On the 21st of April an inquest was held at Little Missenden, in Buckinghamshire, on Joseph Smith, an infant aged about five weeks, who was poisoned by mistake. It appeared that the mother of the child had sent the grandmother to the shop of a Mr. Douglas, a draper, grocer, and vender of medicines, at Great Missenden, for a pennyworth of "child's cordial." We do not learn what passes under the name of child's cordial in that part of Bucks, but may presume it to be a variety of Dalby's Carminative, or some other popular opiate of that kind. When the messenger arrived, the shop was beset with customers, and the draper-druggist was indisposed, so that the grandmother was served by the younger Douglas, aged fifteen. A table-spoonful of some liquid was put into a dirty phial brought by the grandmother, and had the same appearance in it that the cordial would have had. The mother gave the child

about half a tea-spoonful, which it resisted as well as it could, by screaming and putting out its hands; and it was observed that a portion spilt upon the clothes burned them. This naturally excited the mother's suspicions, and on tasting the medicine, she exclaimed it was not child's cordial, as it burned her lips. The grandmother then returned to the shop, and the son frankly confessed that the supposed cordial had been served from a bottle of spirits of vitriol (dilute sulphuric acid), which was similar in size and colour, and stood near the one which held the medicine. The marks on the bottles denoting what they contained had been worn away, so that the Dalby *à la Malthus*, or positive-check spirits, passed instead of the intended narcotic. The account says, curiously enough, "such a mistake had never before occurred in that shop, and the medicine had been vended by Mr. Douglas for many years;" as if a certain average of such mishaps was to be expected in the shop of every grocer-draper-druggist, and the singularity had been that some years had passed over without even one solitary poisoning. Perhaps, however, in this odd phrase the truth may unwittingly have been hit upon, and not only in these piebald and highly-compounded shops, but in those of a more simple and elementary kind, such mistakes may occur sufficiently often to allow their frequency to be calculated, provided each fatal substitution came to light. But the probability, or rather certainty, is, that as in the case of other accidents and offences, so here likewise a small minority only becomes known; as for the rest,

> "——— illacrymabiles
> Urgentur, ignotique longâ
> Nocte, carent quia vate sacro;"

or, in modern language, the cases do not become known, and tears are not shed over the destroyed, because no newspaper reporter was present.

Mr. Douglas, who is described as a person of unexceptionable character, and who appears to have been a well-meaning man, sent a surgeon to the child; but, in spite of his assistance, the little patient expired in about twenty hours. The infant had been previously indisposed, but its death was clearly occasioned by the sulphuric acid.

The son gave his evidence most unreservedly, and seemed, as well as his father, deeply affected by the lamentable accident. The coroner, in summing up the evidence, informed the jury, that if they thought that gross inattention and negligence had been shewn in vending these drugs, so as to endanger the public safety, it would be their duty to treat it as a criminal case, and give a verdict of manslaughter; if, on the other hand, they thought it an accidental occurrence, they would declare it so by their verdict.

The jury almost immediately returned a verdict of "Accidental death." The coroner then addressed Mr. Douglas, and cautioned him to have his bottles properly labelled, as a similar occurrence might not be treated with the same lenity.

There is a story in the books that a man was once tried for some grave offence before a jury, who found him not guilty, but at the same time advised him—never to do so again. This Janus-verdict, or double-faced opinion (which is, perhaps, not *quite* so absurd as it seems at first sight,) in the instance before us, was divided between the jury and the coroner; the former affirming that the druggist had not been grossly negligent; the latter saying, in substance, "Don't be so grossly negligent again, or you may rue it."

In the old anecdote, the jury, of course, meant to say, "In our hearts we believe you guilty; or, at any rate, have such a strong suspicion of it, that if the same occurrence took place in

our own household, we should proceed to execute justice upon the supposed offender; but as we do not like to hang a man for stealing a spoon, we find you not guilty." This is pretty nearly equivalent to the verdict of "not proven" in Scotland. In like manner the Little Missenden jury and the coroner contrived, between them, to give the druggist a jobation for his carelessness, without subjecting him to so serious a punishment as a mere trial for manslaughter, even attended by an acquittal, would necessarily have been.

The unfortunate truth is, that the negligence with which medicine is made up is so great and so common, that unless fair warning was given that a new era was to commence, an age of rigidly accurate labels, and perfect compounding, the Bucks grocer could hardly have been incarcerated, without exhibiting a spectacle of exceptional justice, not many degrees better than injustice itself.

If we wished, indeed, to take a wide view of the subject of poisoning by mistake, it would be necessary to include those numerous cases where the error is not in the compounding but in the prescription; where non-professional persons, seduced by old herbals, or modern compendia, venture beyond their depth; or where legitimate practitioners, in their zeal for a favourite remedy, push it beyond all reasonable limits. John Hunter's Treatise on the Venereal Disease contains a remarkable instance of a mistake into which it was natural for a mere layman to fall. A gentleman had been taking extract of hemlock in doses which were gradually increased, until they reached the enormous quantity of two ounces and a half (the extract being, no doubt, very ill prepared.) The remedy was then discontinued; but some time afterwards the patient resumed its use, of his own accord, and took ten drachms, which proved fatal; whether from the brain having regained its sensibility to the narcotic powers of the drug, or from the extract having been better prepared, does not appear.

The remedy which we suppose to have been intended in the fatal case, near Missenden, has doubtlessly destroyed many lives. Dr. John Clarke, in his "Commentaries on some of the most important Diseases of Children," mentions a case which he saw, where forty drops of Dalby's Carminative killed an infant. Dr. Merriman gives two similar instances, where death was caused by a single dose of Godfrey's Cordial; and he once saw a child within the first month thrown into a state of excessive stupor, by taking one dose of a mixture containing four drops of laudanum, the actual quantity swallowed scarcely amounting to a drop *.

Some examples of the bad effects arising from pushing the use of powerful remedies too far, occur, where many would not expect to find them, in the practice of our neighbours the French. The current opinion that the remedies in use across the Channel consist in little more than *ptisane*, *eau gommée*, *sirop de guimauve*, &c. must be taken not with a grain, but a whole bushel of salt; indeed, on some occasions the French administer remedies with a boldness unknown in this country. Those who would learn in what doses digitalis has been given in the Parisian hospitals may consult some papers by M. Joret, in the *Archives Générales* for January and March, 1834. It appears common to give ten, fifteen, and up to forty-eight grains of the powder in a day; and in one case the infusion of three ounces and twelve grains of the fresh leaves was taken in eight days. These enormous doses often excited the most

* Underwood on the Diseases of Children. With notes by Dr. Merriman, pp. 134 and 200.

violent vomiting, but none of the patients died.

It is, unhappily, much easier to see the evils above alluded to, than to devise any plan for their prevention. Those which arise from the indiscreet zeal of medical practitioners can be obviated only by a more solid education, and a more perfect acquaintance with the history of our art; those which spring from the mistakes of the laity will last till the public has learned the danger of playing with edge-tools; and those which depend on the mal-administration of the shops, may perhaps be mitigated by subjecting them to some kind of effective inspection, and requiring chemists and druggists to undergo a preliminary examination.

ANIMAL MAGNETISM.

We understand that Dr. Roget, Mr. Mayo, Mr. Wheatstone, Mr. Kiernan, Mr. Owen, and Dr. Grant, all Fellows of the Royal Society, have formed themselves into a committee to investigate the subject of animal magnetism. There is one remark which we take the liberty to make in connexion with this subject —namely, that no person who has not had long and ample opportunities of becoming practically acquainted with the diseases of females, can be aware of the almost marvellous things that women will do, and say, and suffer, under some forms of hysteria. Have the gentlemen above named, with the exception, perhaps, of Dr. Roget, had this advantage?

Meantime the performances in the theatre of University Hospital, of which Mr. Mayo has given so full an account in some of our late numbers, continue to be very striking and very attractive. Sir Wm. Molesworth, indeed, approves of them so much, that he has presented the hospital with thirty guineas, and a letter highly complimentary to Dr. Elliotson and mesmerism. By the way, we should like to know what impression was made on a woman of Mrs. Somerville's acute and scientific mind, by the exhibition which she witnessed on a recent occasion. Was she as much astonished and as thoroughly satisfied as the gentlemen were?

MEDICAL CERTIFICATES IN LIFE INSURANCE.

To the Editor of the Medical Gazette.

Sir,

I coincide entirely in the opinion which you have so ably supported, as to the propriety of a proper remuneration being made for medical certificates in life-assurance; but as it is advantageous that important matters of discussion should be frequently adverted to, and viewed in every variety of aspect, I beg to trouble you with a few observations on the subject.

If the office applied to for effecting an insurance required the applicant to lay before them a professional opinion as to his health and habits, he would have to procure it when and how he could; and to arrange for it with a medical gentleman when it was obtained.

Should the directors, on the other hand, consider it more satisfactory to make the application themselves, and merely to require from the candidate for insurance the name of a referee, they would necessarily incur the honourable obligation of seeing to the remuneration of the individual to whom, for their own advantage, and without the slightest claim upon him, they give the trouble and responsibility of answering their queries.

A life-office, however, it is said, pays its own medical men, whose official duty it is to consider, and report on, the health of those who present themselves for insurance, and it should not be subjected to a double expense. So likewise, it may be answered, does a fire-office the services of its surveyor. But if, in order to assist the surveyor in making out his report in the most advantageous way for his employers, the aid of another person is sought for, and demanded by them, because he may know more of the premises, from having been at one time or other employed to construct or repair them, the office itself must, in all fairness, pay the additional expense.

Surveyors and solicitors would not submit to lose their regular payments, nor would directors dream of expecting them to act for nothing; but when a medical man talks of being remunerated for the assistance which he gives, in a regular money-making, mercantile transaction, in which he has not a

shadow of interest, the insured says, "I never applied to you; I did not ask you for a certificate, and know nothing about it; I merely mentioned your name to the office, as a respectable practitioner known to me, and you may thank me for the compliment. You have no claim on me." The office, on the other hand, stoutly asseverates, "We know nothing of you; you were not of our selection; in writing to you, we merely obeyed the instructions of an applicant for insurance; look to him, you have no claim on us." So that in this way the unfortunate son of Æsculapius is placed very much in the position of the butcher in Æsop, who, between his two respectable customers, is minus his mutton.

I quite agree with you in thinking that the higher orders of the profession should lead the way in evincing a determination not to submit longer to the injustice complained of; and as a doctor, in former times, was warned against giving too much credit to a patient, in the adage,

> "Accipe dum *dolet*,
> Nam postea medicus olet;"

I would recommend, in respect to the inquiries of a life-insurance office, the adoption of an aphorism equally doggrel, but not less appropriate, namely,

> "Accipe dum *quærit*,
> Nam credens deceptus erit."

But it has been maintained by a very able and highly respectable resident director,* that Insurance Societies are *philanthropic institutions*, and have therefore a fair claim on the support and assistance of professional gentlemen, independently of pecuniary considerations. If directors and actuaries did their business for the pleasure alone of indulging benevolent feelings, there might be some plausibility in this position; but it may be said, with equal justice, that the butcher and the baker are great philanthropists, and should have their stalls and shops kept in repair, their diseases cured, and, if you please, their lives insured, gratuitously, because they supply us with the meat and bread without which we could not live. They sell us their commodities, not, however, for humanity sake, but that they may subsist by the profits, which is just the case with insurance offices; nor ought the medical profession, which contributes its services so largely to the poorer orders of society, without pecuniary recompense, be expected to present the anomaly of foregoing a reasonable remuneration, from those who can well afford to pay it.

The charges which prepare for, and precede the effecting an insurance, are part of the legitimate expenditure of the establishment, and should be provided for by the premium paid by the insured.

If the calculations, however, are so curiously and nicely adjusted, as not to bear the augmentation of a small fee for an important additional safeguard, then let the office either require it to be advanced by the candidate, in the first instance; or, as you very judiciously suggested some time since, itself pay this little *quiddam honorarium*, and have it refunded by a charge upon the policy.

But the amount of the remuneration required is so trifling, as to make it extraordinary that the respectable and liberal-minded men who usually compose the directors of life-insurance offices, should ever think of withholding it.

If a person of 30, insured his life for 500*l.*, he would pay, on the average of the terms of half a dozen offices which are now before me, about 11*l.* per annum, or 2*l.* 4s. per cent. The probable duration of his life, according to the Northampton tables, is about 28 years; and the sum expected to be received by the Company, in annual payments, is about 311*l.* Taking, therefore, the outlay of a professional fee of a guinea at the commencement, as making it necessary for the office to receive 314*l.* instead of 311*l.*, from each person insuring under similar circumstances, there will only be an addition of 5d. per cent. required on the annual premium, or about half-a-crown in the whole annual payment.

One word more. I am aware that the editors of a very respectable quarterly Medical Review (Dr. Forbes, of Chichester, and Dr. Conolly, of Warwick) think it no hardship for medical certificates to be given gratuitously; and their opinion has been extensively and triumphantly blazoned about by insurance offices.

When Charles the First, in an audience given to the Bishops of Ely and Winchester, asked the former, "if he

* Mr. Farren, of the Asylum Office.

did not think it lawful to levy money on his subjects without the consent of Parliament?" "Certainly," he replied, "for your Majesty is the breath of our nostrils." "You hear, my Lord of Winchester," observed the King, "what the Bishop of Ely says, now what say you?" "That your Majesty may most assuredly take my brother of Ely's money, for he gives your Majesty full liberty to do so."

No applicant for insurance, or insurance office, can therefore be justly chargeable with illiberality or injustice in accepting, without remuneration, the professional certificates of the two learned physicians whose opinions I have now noticed.

I have the honour to be, sir,
Your faithful and obedient servant,
A Retired Physician*.

March 28, 1838.

PHYSICAL SOCIETY, GUY'S HOSPITAL.

April 21, 1838.

Mr. Edward Cock in the Chair.

Injuries of the Brain.

Mr. Coales read two cases of Injury to the Brain.

The first was that of a man æt. 36, who was admitted into the hospital under Mr. Key, on the 20th of November last, with a severe, contused, and lacerated wound of the right side of the head, caused by a kick. The symptoms on admission were, great pain in the head, dizziness, confused intellect; pupils rather contracted; pulse labouring. There had been much bleeding from the wound.

The treatment was at first of an antiphlogistic character; but the parts around the injury took on a sloughy appearance, and a great deal of blood was lost on several occasions from sloughing of the arterial branches about the temple. It became necessary, therefore, to use styptic and stimulant applications to the part, and to support the constitutional power by means of quinine, ammonia, porter, &c. Under this treatment all his bad symptoms had subsided, with the exception of the headache, which persisted at all times in greater or less degree. This improvement continued till the 20th January, two months after the receipt of the injury, when he was seized with a violent paroxysm of throbbing and pain in the head.

"On the 21st, pain in the head continued; there was a great alteration in his aspect; he appeared vacant and inattentive, but yet capable of being roused, and answered rationally. Right pupil dilated; left contracted. Between two and three o'clock on the following day he fell suddenly into a state of complete coma, like an apoplectic fit, except that the breathing was scarcely stertorous. There had been no previous hemiplegia. He died in about two hours from this seizure.

"Post-mortem inspection shewed that the bone was denuded to the extent of three inches square, and necrosed. The soft parts were healthily granulating. Within the calvarium, the inner table was also necrosed, but to a less extent, and was defined from the surrounding parts by a serrated interval, partly filled up by puffy granulations, apparently from the dura mater."

Below the dead bone the dura mater was pulpy and discoloured; the two layers of arachnoid were adherent for the space of a shilling. Upon slicing away a portion of brain at this spot, an abscess containing about two or three drachms of pus was discovered, a quarter of an inch beneath the surface; it passed downwards and backwards, and opened into the right ventricle, into which it seemed to have recently burst, near the outer edge of the corpus striatum.

The second case was admitted also under Mr. Key, on the 23d January. He was 25 years old, and had received a contused wound on the head with a pewter pot. The symptoms, on admission, were similar to those of the first case. The wound appeared very slight, and the bone was not laid bare. Antiphlogistic measures were adopted; headache, however, persisted; the edges of the wound became sloughy; the bone became affected, and several pieces exfoliated away. He had occasional numbness in his fingers, and frequently vomited. This lasted seven weeks. On the 25th March he had deafness and much headache. On the 27th there was an irregularity in his manner and intellect. On the 28th he had a rigor, intense headache, drowsiness, great febrile heat of body, intellect deranged; he imagined phantoms were around him, and had convulsive twitchings of the right upper and lower extremities, and of the abdominal muscles, and those of the back. The muscles of the face and of respiration were, in the author's opinion, entirely unaffected. Pulse 145; left pupil natural, right dilated, but both obeyed the stimulus of light. There was deep-seated headache, and the convulsive twitchings alternated with partial fits of insensibility.

* The above letter is from the pen of a very distinguished provincial physician.—Ed. Gaz.

Mr. Key was induced to remove a piece of the diseased bone. The outer shell alone was found diseased; the internal still adhered to the dura mater. This membrane looked healthy, and no relief resulted from the operation. Much blood was lost from branches of the temporal artery, which were divided in enlarging the external wound, and a further depletion from the arm was practised. The headache was relieved. He was ordered calomel, gr. j. every second hour.

He went on thus, without appearing worse, till the 30th, when he had an epileptic paroxysm, which convulsed the right more than the left side: this lasted five minutes; and in the evening he was delirious. On the 31st erysipelas had attacked the wound; he was becoming more sluggish, and his respiration almost comatose. On exposing the wound, the dura mater was found very much bulging, and fluctuation was felt. Mr. Cock opened it, and let out about a drachm of sero-purulent fluid. The brain underneath seemed also baggy and fluctuating, but Mr. Cock did not deem it prudent to proceed further. The patient seemed relieved for a time, but he soon relapsed; his vision became impaired; all his symptoms became worse; a hernia cerebri appeared. He became insensible, and so sunk early on the morning of the 2nd of April.

Post-mortem inspection shewed a universal arachnitis on the side affected, with considerable sero-purulent effusion. The substance of the brain was healthy, with the exception of the part around the hernia cerebri, which was discoloured and softened, but not to a great extent—about three-quarters of an inch deep, and about the space of a shilling. The arachnoid of the opposite side was also slightly inflamed.

Mr. Coales concluded by contrasting together these two cases, which our space does not permit us, unfortunately, to give at greater length. He endeavoured to draw a distinction between the symptoms of simple arachnitis, and of suppuration within the brain; and called the attention of the Society to the circumstances which appeared to him to possess most prominent interest in each.

A discussion of a conversational and purely practical nature followed, in which several members participated; at the conclusion of which the thanks of the Society were presented to Mr. Coales, for his interesting communication.

The PRESIDENT announced that this was the last meeting of the Society for the present session, and said, that the diploma of "Fellow of the Physical Society" had been adjudged to Mr. Chevers, Mr. Aspland, Mr. Brereton, and Mr. Gorham, for the papers these gentlemen had presented to the Society this session.

In conclusion, the thanks of the Society were unanimously given to Mr. Harrison, the Treasurer of the Hospital; to the Presidents of the Society; and to the Honorary Secretaries, Dr. G. O. Rees and Mr. Tweedie, in acknowledgment of the efficient manner in which these gentlemen had fulfilled the duties of their several offices.

The Society now stands adjourned till the first Saturday in next October.

ANOMALOUS CASES OF FRACTURED CLAVICLE.

To the Editor of the Medical Gazette.

SIR,

As those cases in whose course or nature any irregularity or deviation from the usual routine occur, appear the most deserving of notice, inasmuch as they excite a degree of interest otherwise unusual, and give an importance which is otherwise unnecessary, I am induced to throw myself on your liberality in requesting the insertion of the following remarks in an early number of your valuable journal, and this more especially, as I think minute examination may lead to the detection of an error which, from the high authority in which it emanates, receives all the weight of authenticity.

It is in most surgical works laid down as one of the most infallible symptoms of fractured clavicle, that when the accident occurs in any part internal to the coraco-clavicular ligament, or in other words, in that extent of bone included between the above-mentioned ligament and the sternum, that the patient is disabled from raising the hand of the affected side to his brow; and that if such an action can be performed, we may without further inquiry pronounce that the injury does not consist in fracture of that bone. Mr. S. Cooper, in his Surgical Dictionary, says, "The patient finds it impossible to raise his hand to his forehead, because this action renders a semicircular motion of the humerus necessary, which cannot be done when the humerus has lost its *point d'appui;*" and again, when addressing his numerous class of surgical students at the London University, he makes use of the following words:—"Now you are to observe, this infirmity" (viz., inability to raise the hand to the head) "will *always* present itself unless the fracture be outside the coraco-clavicular ligament;" and an examination of the bones which enter into the composition of the shoulder-joint will not only serve to validate this assertion,

but appear to stamp it with truth. The clavicle extending horizontally, from the sternum to the scapula, and connected firmly to both bones, serves not only to preserve them at their proper distance, between which it seems to perform an office somewhat analogous to that of the key-stone in an arch; but also has to support the whole weight of the arm, and from this circumstance alone is rendered extremely liable to injury, from the propagation of shocks received upon the hand; and it appears physically impossible that any circular motion of the humerus can occur, the clavicle being broken, and obliged to rotate on its own axis, having lost its fulcrum of support. The following case, however, proves that these facts (if such they be) do not hold good under all circumstances; and that they may even be productive of harm, by dispensing with a manual examination when no fracture is supposed to exist, from the facility with which the hand is moved to the head; and here we have another example of variance occurring between experience and theory; which, however much they may sometimes seem dependent upon each other, and however closely they may sometimes be linked in the chain of medical science, at others appear entirely unshackled, and even stand in open opposition to each other.

H. P———, while hunting, was thrown from his horse, falling on his right hand and side, while his forehead, striking against a stone, was severely bruised; complains of great pain in the right shoulder and collar-bone, which part is swollen; *he can easily raise his hand to his head without increasing the pain, and has considerable motion with the limb.* On examination, the shoulder on the injured side was approximated to the sternum, and on tracing along the bone with the fingers a depression is perceptible *in its centre*, together with a crepitus, and the fractured end of the inner portion, or "rising end" of the bone, as it is improperly called, can be plainly felt: on drawing the shoulder outwards, backwards, and upwards, the bone is brought in its normal situation, and the deformity disappears, and scapular bandage was applied. The marked nature of the symptoms occurring in the above case are sufficient evidence of its nature, without leaving the slightest doubt, however much we might be inclined to disbelieve it, without a minute examination; and as otherwise we cannot distinguish it from a common bruise, the symptom alluded to must become of little absolute value. How to account for this anomaly I am at present ignorant, although I am inclined to believe that the direction of the fracture may have some influence in its production, for it seems possible that when this extends obliquely from within, outwards, and above downwards, the inner portion may become in some degree a support to the outer, and allow it a limited rotation; this is, however, purely hypothetical, and by no means satisfactory; for in the present instance a displacement in the long axis of the bone occurred, and then the outer fragment is drawn by the action of the muscles below the inner. But as I am afraid of trespassing too much on your pages, I will conclude with observing, that it seems not only possible, but highly probable, that practitioners may have frequently overlooked the presence of fracture in this bone, from a too implicit reliance on what has been considered an established diagnostic mark; but whose validity I must deny, with all due deference to the illustrious members of the profession by whom it has been promulgated.

Your humble servant,

OBSERVER.

April, 1838.

PETITION AGAINST EMPIRICISM.

To the Editor of the Medical Gazette.

SIR,

THE following is a copy of a petition which has been sent to Lord Dundas, to be presented to the House of Lords, and a similar one to J. H. Lowther, Esq. for presentation to the House of Commons.

The petitioners include every practitioner but one, residing in, or in the suburbs of, the city of York.

I have the honour to be, sir,
Your obedient servant,

WM. ANDERSON,
Hon. Sec.

York, April 28, 1838.

To the Right Honourable the Lords Spiritual and Temporal, in Parliament assembled.

The Petition of the undersigned medical practitioners, residing in York, humbly sheweth,

That it is notorious that many persons who have received no medical education whatever, and who have not submitted to the examinations required by the existing laws, are now practising the several branches of the medical profession in England and Wales.

That in consequence of the ignorant administration of medicines, and unskilful performance of surgical and obstetrical operations by such persons, the lives and health of her Majesty's subjects are greatly endangered, while the regular practitioner, who has duly prepared himself for the responsibilities of practice by an arduous and expensive course of study, and has proved his fitness by undergoing the strict examinations required by law, is exposed

to an illegal, unfair, and most injurious competition.

That the only redress now open to the qualified practitioner, and the only protection afforded to the public against unqualified and ignorant pretenders to medical knowledge, consists in a recourse to the costly, tardy, and, as experience has proved, ineffectual provisions of the existing Apothecaries' Act.

Your petitioners, therefore, pray that your Right Honourable House would be pleased to institute such inquiry into the evil complained of, as may enable it in its wisdom to apply thereto such legislative remedy as may effectually secure the public from the dangers of ignorant and empirical practice, and may protect the duly qualified medical practitioner in the exercise of his profession.

And your petitioners will ever pray, &c.

Baldwin Wake, M.D. Physician to the York Lunatic Asylum.
James Atkinson, senior surgeon.
Wm. Stephenson Clarke, surgeon.
Wm. Vane Hope, surgeon.
John Hopps, surgeon.
Benjamin Dodsworth, surgeon.
George Goldie, M.D.
George Brown, surgeon.
Aswald Allen, surgeon.
Amos Coates, surgeon.
Wm. Anderson, surgeon.
H. S. Belcombe, M.D. senior physician to the York County Hospital.
John Thomas Dolman, surgeon.
Thomas Rigg Wisker, surgeon.
Frederick Swineard, surgeon.
Thomas Hanley Barker, surgeon.
James Allen, surgeon.
Richard Robinson Alderson, surgeon.
Wm. Dalla Husband, surgeon.
Edward Wallis, surgeon.
Wm. Coates, surgeon.
George Clark, surgeon.
W. F. Rawdon, M.D.
Caleb Williams, surgeon.
Richard Hey, surgeon.
Henry Crummack, surgeon.
Thomas Simpson, M.D. physician to the York County Hospital.
Henry Russell, surgeon to the York County Hospital.
Edward Thomas Allen, surgeon.
Henry Gibson, surgeon.
Thomas Laycock, house-surgeon and apothecary to the York County Hospital.
Thomas Abby, surgeon.
Thomas K. Walker, surgeon.
John Wilson, apothecary to the York City Dispensary.
Wm. Matterson, jun. surgeon.
Wm. Matterson, surgeon.
J. P. Needham, surgeon.
James S. Overton, surgeon.

EXTERNAL USE OF CALOMEL IN OPHTHALMIA.

By Dr. Fricke, of Hamburgh.

Dr. Fricke being at Lausanne in the summer, had his attention directed by Dr. Mayor to the fact that he frequently applied calomel to the eyes with advantage, in cases of scrofulous ophthalmia; and since he learned this, he has tried the practice, extending it to a greater number of diseases. For his first experiments he selected such inflammations of the eye, as from their character, duration, and obstinacy, in some measure justified the use of a new remedy. To this class belonged rheumatic, catarrhal, and scrofulous inflammations of the eye, which had passed their first stage, and had put on a chronic form, and in which other remedies had been long used without advantage. These inflammations had for the most part attacked several structures of the eye, and were complicated with degeneration, morbid growths, or protuberance of the conjunctiva and iris, exudation in the pupil, opacity and spots of the cornea, &c. Afterwards, indeed, the remedy was used in other and slighter forms of ophthalmia. The result of his observation was, that calomel applied in this manner was very beneficial. It was especially efficacious against the symptom which so often accompanies rheumatic and scrofulous inflammation—*intolerance of light*. The method of using the remedy is very simple. A miniature hair-pencil moistened, and then dipped in alcoholized calomel, is applied to the eye-ball; and this is repeated every twenty-four hours or oftener. The sensations which the calomel produces in the eye vary extremely; in healthy eyes it usually causes hardly any sensation, or sometimes a slight feeling of heat, which soon goes off. When the eye is inflamed, pain is caused, but this is generally very trivial, and was complained of by the patients only in a few cases. The pain goes off entirely in a period varying from half an hour to two hours. Some patients, however, did not feel it, and one even thought the remedy cooling. Dr. Fricke never observed any bad consequences from its use, except in the two following cases:—

Case I.—A girl of the town, aged 21, was attacked with rheumatic inflammation of the left eye on the 1st of April. The iris suffered, and the pupils were distorted outwardly. The inflammation was mitigated in a few days by means of leeches, cathartics, rubbing in ointment, &c.; but the pupils remained distorted, and their edges were irregular. On the

6th of April calomel was applied, and on the following day *the eye was very violently inflamed.* The eyelids were considerably swollen, and hot tears, mingled with much mucus, flowed copiously from the eyes. None of the calomel could be discovered. The intoleranee of light, and the spasmodic contraction of the eyelids, were so intense, that it was not possible to examine the eye-ball.

The inflammation yielded, though very slowly, to vigorous antiphlogistic treatment, combined with derivatives applied to the skin and the alimentary canal.

Case II.—A tailor's wife, aged 43, was suffering from a chronic but unimportant inflammation of the eyelids, and specks upon the cornea, for which calomel was applied to both eyes. At four in the afternoon the surgeon in attendance was called to her. She complained of a violent burning in both eyes, which had begun about half an hour after the application of the calomel, and had gone on continually increasing. Not only the eyelids, but the cheeks, and particularly the nose, were much swelled and reddened. Scalding tears mixed with much mucus streamed from both eyes; and the intoleranee of light was so great, and the eyelids were pressed together so forcibly, that it required some force to open them. The conjunctiva of both eyes was violently inflamed throughout its whole extent, and swelled out with chemosis, so that it surrounded the cornea, (which had a somewhat powdery appearance,) like a high wall. The remains of the calomel in the eye had put on a greenish-yellow tint. The eyes were immediately cleared, almond oil was dropped in, a decoction of mallow was applied in the form of a poultice, and an antiphlogistic and derivative treatment was adopted; so that the inflammation gradually decreased, but had not entirely disappeared after the elapse of eleven days, when the case was drawn up.

Dr. Fricke believes the violent effect of the calomel to have arisen from both patients having taken the hydriodate of potash; the first had taken it 20 days for a syphilitic eruption, the latter 36 days for chronic ulcers of the feet, which were probably of syphilitic origin. Now since, when the preparations of iodine are used internally, most of the secretions and excretions become impregnated with it, he is of opinion that in both cases the iodine in the tears (where it has been detected by Buchanan and others) united with the mercury, and thus produced a powerful irritant.

In order, however, to prove more clearly that the pernicious effect of the calomel in both cases depended *solely* on the newly-formed iodide of mercury, he selected a female patient who had taken the hydriodate of potash for twenty-two days for a syphilitic eruption, in order to try the effect of the calomel upon a healthy eye. Reagents showed that the urine of the patient was strongly impregnated with iodine. Calomel was accordingly put into one eye in the morning; and in half an hour the patient felt a burning sensation in it, on which, of her own accord, she washed the eye out. Two hours afterwards, nothing morbid could be detected in the eye. Calomel was now applied to the other eye, and the patient was carefully watched. In three hours' time the eyelids had swelled, and there was a copious flow of tears with a secretion of mucus. The conjunctiva was puffed out like a bladder on the lower segment of the eye, while the upper half was red. The calomel still remaining in the eye was of a faint colour approaching to yellowish. To prevent the inflammation from spreading, the eye was immediately washed out with lukewarm water, almond oil was dropped in, poultices with decoction of mallow, leeches, purgatives, &c. were employed, with the effect of arresting the inflammation; but the intolerance of light and the lacrymation lasted a couple of days longer. That syphilis of itself had no share in these affections was demonstrated by many experiments, in which Dr. Fricke introduced calomel into the eyes of syphilitic patients, without producing the smallest reaction.

In order to try by direct experiment whether iodine was present in the tears of patients who were taking hydriodate of potash, fine linen moderately dressed with starch was wetted with tears, but no blue tint was produced; nor did this effect follow when the moistened bit was just touched with nitric acid, or solution of chlorine; although when the urine of these patients was mixed with solution of starch and chlorine-water a strong reaction instantly followed.

Hence the greenish tint which the calomel assumed could be ascribed only to its being partially changed into ioduret of mercury; in which case a part of it might by possibility be turned into corrosive sublimate.—*Schmidt's Jahrbücher.*

[This paper affords some striking instances of the taste for experimental medicine now so prevalent upon the continent; sound eyes are made diseased, in order to test the properties of a new remedy intended to make diseased eyes sound—to the utter neglect of the best rule in medicine—*saltem non nocere.*—Translator.]

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns, May 22, 1838.)

	Price.		Duty.	Duty Paid.	
				In 1838 to last week	Same time last year.
	£ s. d. to £ s. d.		s. d.		
Aloes, Barbadoes, D.P. c	12 0 0 to 30 0 0	B P. lb	0 2	52,361	33,692
Hepatic (dry) BD..... c	5 0 0 14 0 0	F. lb	0 8		
Cape, BD. c	1 16 0 2 0 0				
Anise, Oil of, German, D.P.... lb	0 9 6 0 9 6	F. lb	1 4	——	251
E. I. ..... lb	0 7 0 0 7 6	E. I.	1 4	383	517
Asafœtida, B.D. c	2 10 0 5 0 0	c	6 0	35	1
Balsam, Canada, D.P. ..... lb	0 1 3 0 1 4	lb	0 1	2,745	401
Copaiba, BD. ..... lb	0 7 6 0 8 0	c	4 0	51	109
Peru, BD. ..... lb	0 4 3 ——	lb	1 0	1,311	224
Benzoin (best) BD..... c	25 0 0 50 0 0	c	4 0	47	40
Camphor, unrefined, BD..... c	9 0 0 ——	c	1 0	283	236
Cantharides, D.P. ..... lb	0 5 0 ——	lb	1 0	7,709	11,952
Carraway, Oil of, D.P. ..... lb	0 9 0 ——	lb	4 0	——	585
Cascarilla or Eleutheria Bark, D.P.c.	1 15 0 ——	lb	0 1	4,031	2,096
Cassia, Oil of, BD..... lb	0 7 0 0 7 6	lb	1 4	1,764	1,244
Castor Oil, East India, BD ..... lb	0 0 6 0 0 10	c	1 3	2,208	2,750
West I. (bottle) D.P. 1½ lb	0 2 3 ——		——		
Castoreum, American ..... lb	1 15 0 ——	lb	0 6	460	147
D.P. Hudson's Bay ..... lb	1 0 0 1 4 0				
Russian..... lb	none				
Catechu, BD. Pale c	1 5 0 ——	c	1 0	7,612	15,366
Dark	3 0 0 ——				
Cinchona Bark, Pale (Crown).... lb	0 2 0 0 3 6	lb	0 1	53,986	81,010
BD. Red ..... lb	0 2 0 0 4 0				
Yellow ..... lb	0 2 3 ——				
Colocynth, Turkey ..... lb	0 2 6 0 4 0	lb	0 2	3,998	4,003
D.P. Mogadore ..... lb	0 3 0 ——				
Calumba Root, BD. c	1 4 0 2 5 0	lb	0 2	11,942	13,451
Cubebs, BD. c	5 0 0 ——	lb	0 6	11,828	12,408
Gamboge, BD. c	5 0 0 15 0 0	c	4 0	51	47
Gentian, D.P. c	1 4 0 ——	c	4 0	243	318
Guaiacum, D.P..... lb	0 1 0 0 1 8	c	6 0	13	16
Gum Arabic, Turkey, fine, D.P... c	8 0 0 9 0 0				
Do. seconds, D.P. .. c	5 0 0 7 0 0	c	6 0	2,614	1,530
Barbary, brown, BD. c	3 0 0 3 3 0				
Do. white, D.P..... c	4 15 0 ——				
E. I. fine yellow, BD. c	3 0 0 3 10 0	c	6 0	1,730	819
Do. dark brown, B.D. c	1 15 0 2 5 0				
—— Senegal garblings, D.P. c	4 15 0 5 0 0	c	6 0	4,130	242
—— Tragacanth, D.P. c	8 0 0 12 0 0	c	6 0	193	143
Iceland Moss (Lichen), D.P. .... lb	0 0 2½ 0 0 3	lb	0 1	4,225	1,746
Ipecacuanha Root, B.D. ..... lb	0 1 9 0 2 0	lb	1 0	8,695	5,340
Jalap, BD. ..... lb	0 2 4 ——	lb	0 6	14,693	30,418
Manna, flaky, BD. ..... lb	0 4 0 0 5 6	lb	0 3	3,596	13,042
Sicilian, BD. ..... lb	0 1 7 ——				
Musk, China, BD. ..... oz	1 0 0 1 8 0	oz	6 0	651	819
Myrrh, East India, BD. c	5 0 0 14 0 0	c	6 0	64	73
Turkey, BD..... c	2 0 0 11 10 0				
Nux Vomica, BD..... lb	0 8 0 0 9 0	lb	2 6	556	838
Opium, Turkey, BD. ..... lb	0 14 6 ——	lb	1 0	10,976	13,699
Peppermint, Oil of, F. BD..... lb	0 19 0 ——	lb	4 0	324	515
Quicksilver, BD. ..... lb	0 3 6 ——	lb	0 1	108,671	88,217
Rhubarb, East India, BD..... lb	0 2 6 0 3 3	lb	1 0	13,506	13,265
Dutch, trimmed, D.P. lb	0 3 6 0 4 0	F. lb	1 0	2,475	3,182
Russian, BD. ..... lb	0 8 3 ——				
Saffron, French, BD. ..... lb	0 18 0 ——	lb	1 0	1,903	2,276
Spanish ..... lb	0 19 0 ——				
Sarsaparilla, Honduras, BD..... lb	0 1 0 0 1 9	lb	0 6	39,097	49,110
Lisbon, BD. ..... lb	0 2 0 ——				
Scammony, Smyrna, D.P..... lb	—— ——	lb	2 6	3,612	3,856
Aleppo ..... lb	0 12 0 0 15 0	E.I. lb	0 6	29,379	46,335
Senna, East India, BD. ..... lb	0 0 3 0 0 4	Other sorts	0 6	32,309	30,610
Alexandria, D.P..... lb	0 1 9 0 1 10				
Smyrna, D.P. ..... lb	0 1 0 0 1 3				
Tripoli, D.P. ..... lb	0 1 0 0 1 3				

‡§‡ BD. In Bond.—c. Cwt.—B. P. British Possessions.—F. Foreign.—D. P. Duty paid.

NOTE ON THE DIAGNOSIS OF MALIGNANT TUMORS WITHIN THE THORAX.

To the Editor of the Medical Gazette.

Sir,

In the last number of the Medical Gazette there is an interesting communication from Dr. Charles Cowan, chiefly relating to malignant tumors within the thorax. In that paper reference is made to the chapter on this subject in Dr. Stokes' late valuable work; and from this it would appear that both Dr. Cowan and Dr. Stokes have passed over some interesting cases of this description, published by Dr. John Sims, some years since, in the 18th volume of the Medico-Chirurgical Transactions. In two of these cases, if my memory serve me (for I have not the work at hand to refer to), but certainly in one, Dr. Sims was enabled to form a correct diagnosis, very much on the same principle (or that of exclusion) which has been successfully followed by Dr. Cowan.

I do not at all wish to detract from the merit and interest of Dr. Cowan's cases, nor should I have troubled you with these observations, had not Dr. Cowan stated that he believes this to be "the first instance recorded of the successful application of physical diagnosis" in a case of this description.—I am, sir,

Very respectfully yours,

John Thurnam.

Westminster Hospital,
May 12, 1838.

RUPTURE OF THE CÆCUM DURING LABOUR.

The subject of this case was a woman with her first child, who had enjoyed good health during the whole period of pregnancy and a favourable accouchement. The mother and husband, however, of the patient insisted that they heard at the moment when the child was expelled a peculiar noise. During the first twenty-four hours no unfavourable symptoms presented themselves; the patient complaining only of griping, and a slight pain in the vicinity of the right groin. Fever, vomiting, acute pain in the abdomen, &c., then came on, and the patient died 72 hours after delivery.

On post-mortem examination a transverse rent, two inches in extent, was found in the cæcum, through which a considerable quantity of fæcal matters were effused into the peritoneal cavity*.

* Preuss. Med. Zeit.; and Amer. Journ. Feb. 1838.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, May 17, 1838.

Charles Morgan, Lantwit-Major.—James Henry Kent, Stanton, Suffolk.—Edward Ray, Clare, Suffolk.—John George Passmore, Bath.—Alderman John Dunning Partridge, Colchester.—Edward Newton, Hexham.—Henry Edmonds.—Thomas Brettell Barrett, Shrewsbury.—William Taylor, Leicester.—William Murray Lilke, Bridgwater.

WEEKLY ACCOUNT OF BURIALS.

From Bills of Mortality, *May* 22, 1838.

Abscess	2	Hooping Cough	3
Age and Debility	20	Inflammation	19
Apoplexy	6	Bowels & Stomach	1
Asthma	8	Brain	12
Childbirth	1	Lungs and Pleura	8
Consumption	47	Liver, diseased	3
Convulsions	20	Mortification	2
Croup	2	Small-pox	20
Dentition or Teething	1	Sore Throat and Quinsey	1
Dropsy	3	Stricture	1
Dropsy in the Brain	4	Thrush	1
Fever	15	Unknown Causes	53
Fever, Scarlet	1		
Fever, Typhus	3	Casualties	6
Hæmorrhage	1		

Increase of Burials, as compared with the preceding week . . . 43

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

May.	Thermometer.		Barometer.	
Thursday . 17	from 24	to 60	29·76	to 29·79
Friday. . . 18	26	57	29·79	29·83
Saturday . 19	38	58	29·88	29·77
Sunday . . 20	46	63	29·61	29·52
Monday . . 21	47	63	29·52	29·58
Tuesday . 22	46	56	29·52	29·62
Wednesday 23	41	58	29.69	29·77

Winds S.E. and S.W.

Except the 17th and 18th, generally cloudy, with frequent rain.

Rain fallen, 0875 of an inch.

An error in the self-registering thermometer used on the nights of the 14th, 15th, and 16th, renders it necessary that six degrees should be added to the temperature marked in our journal on those days—viz., on the 14th, for 24 read 30; 15th, for 17 read 23; 16th, for 15 read 21.

Charles Henry Adams.

NOTICES.

Mr. Jeffreys' concluding paper on Ventilation shall appear next week; it was impossible for us to insert it in the present number.

The papers of Mr. Phillips and Dr. Cowan we also purpose to publish in our next.

Could not "Londinensis" extend his paper a little, and attach his name to it?

Mr. Eden's letter has been received.

M. D., Andreapolis, next week.

Veritas cannot be inserted without the real name of the writer.

Wilson, Pon & Srinters, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 2, 1838.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine,

By Southwood Smith, M.D.

Physician to the London Fever Hospital.

Lecture XII.

Diseases of the Heart capable of producing sudden Death — Pericarditis — Hydrops Pericardii — Adhesion of the Pericardium — Acute Carditis — Partial Carditis, terminating in Ulceration, or Abscess — Softening of the Muscular Substance — Induration — Tuberculation — Hypertrophy — Aneurism of the Substance of the Heart — Aneurism of the great Blood-Vessels — Accumulation of Fat — Osseous and Cartilaginous Depositions in the Muscular Substance — Thickening, Ossification, and Warty Vegetations of the Valves — Angina Pectoris — Rupture.

Diseases of the heart capable of producing sudden death. — I go on with a consideration of the diseases of the organs of the chest and abdomen which are connected with sudden death. Among the many organic affections of the heart, there is scarcely one which may not be the cause of sudden death. Several of these affections remain latent for a considerable time, or produce so little inconvenience that their existence is unsuspected; but at length some exciting cause calls them into violent action, and life is suddenly and unexpectedly extinguished. Some of these affections occasion natural death, in a manner so similar to death by poison, that it becomes necessary, with reference to forensic medicine, to study them with peculiar attention; and they have this additional interest, that some of these maladies, formidable and alarming as they appear, are nevertheless curable: you may be called in before death has taken place, and by the prompt and persevering administration of the proper remedies, you may have the satisfaction of saving that life into the cause of the extinction of which you would otherwise have had to inquire.

1. *Pericarditis.* — The affections of the investing membrane of the heart, the pericardium, whether acute or chronic, generally occasion so much suffering, and are attended with such peculiar and characteristic symptoms, that their existence during life is no matter of doubt or uncertainty, and the suddenness with which they ultimately prove fatal is an event foreseen and predicted. Still, however, pericarditis, in its most acute form, sometimes attacks so suddenly, and proves fatal with such extreme rapidity, that it may appear doubtful whether the death can be natural; while, in its chronic form, it may be so obscure as to be overlooked. Death may at last be produced by it, quite suddenly and unexpectedly, and the true cause of the fatal event may be wholly unknown to every one about the deceased. Still, if the persons about the deceased have been familiarly acquainted with him, it is seldom that you will not, on inquiry, be able to discover that indications of the disease were really present during life; at all events, inspection of the body after death can leave you in no doubt as to the true cause of the fatal event. If, for example, you find preternatural redness of the pericardium, either general or in small scarlet specks, with a natural colour of the intervening membrane, thus giving to the membrane a dotted or marbled appearance; if, together with this vivid mottled redness, you find an effusion of lymph, or of sero-purulent fluid; or if the serum having become absorbed, and the lymph organized, you find the pericardium generally or partially adherent to the heart, you will have found an adequate cause of

natural death from obstruction to the circulation, occasioned by inflammation of the pericardium, and from morbid products the result of that inflammatory action.

2. *Hydrops pericardii.*—It sometimes happens that the serum effused by the inflammatory process is not absorbed, and consequently that the pericardium is prevented from adhering to the heart, by the fluid which is interposed between the two surfaces: the fluid continues to increase, and in this manner is formed the disease termed hydrops pericardii. Often the effused fluid amounts to a pint, and occasionally to three or four, or even five pints; and sometimes, in consequence of the compression exerted upon it by the fluid, the heart itself is reduced in volume, as if it were atrophous.

3. *Adhesion of the pericardium.*— When, on the contrary, adhesion of the pericardium takes place, and becomes complete, it almost always induces other diseases of the heart—generally hypertrophy with dilatation. "I have observed," says Dr. Hope, "that cases of adhesion terminating in enlargement, often hurry to their fatal conclusion with more rapidity than almost any other organic affection of the heart; and I have, on the other hand, repeatedly seen patients die from the consequences of adhesion, the history of which I could trace back eight, ten, or more years; yet such individuals would not unfrequently represent their health to have been perfect during the greater part of that period, and would not admit, until closely interrogated, that they had been more or less short-winded. Hence I infer, that though adhesion may not for a time create much inconvenience, its effects are ultimately fatal."

The fatal result constantly happens quite suddenly, and it may happen under circumstances which give rise to the suspicion of poisoning; but this suspicion, inspection of the heart will, of course, enable you completely to remove.

4. *Acute carditis; partial carditis, terminating in ulceration, or abscess.* — In like manner, acute carditis, or inflammation of the muscular substance of the heart, although it generally proclaims its existence by signs too manifest and distinctive not to ensure its recognition during life, or to obviate all doubt or ambiguity as to the true cause of death, yet it occasionally proves fatal with a rapidity which can scarcely be supposed to belong to natural death. Dr. Latham records a case of most rapid and acute inflammation of the muscular substance of the heart, in which death took place after an illness of only two days, and in which the whole heart was deeply tinged with dark-coloured blood; its substance softened, and here and there, upon the section of both ventricles, innumerable small points of pus oozed from among the muscular fibres. This is a very rare disease; but partial carditis, terminating in ulceration or abscess in the walls of the heart, is not very uncommon, and is one of the most frequent causes of another suddenly fatal affection hereafter to be noticed, namely, rupture.

5. *Softening of the muscular substance.*— There is reason to believe that one consequence of inflammation of the heart is a peculiar change of its muscular fibres, which gives rise to an affection interesting in the highest degree, pathologically, practically, and jurisprudentially; an affection often rapidly mortal, yet the fatal progress of which is sometimes capable of being arrested by the immediate adoption of the proper means.

The following cases afford striking illustrations of this affection:—A young lady was talking cheerfully with her husband; suddenly she complained of being faint, and desired to be laid down. She was placed in bed; her husband continued to support her there, and supposed she had fallen asleep. Afraid to disturb her, he held her in his arms for about the space of twenty minutes; he then rang for the servant, who, on entering the room, exclaimed, "My mistress is dead!" And so it was.

An elderly man, who had recently recovered from a maniacal affection, and who was apparently in sound health, fell suddenly from his chair; he breathed short and feebly for a few minutes, and then expired.

A lady was delivered of twins. After the birth of the second child she appeared a good deal exhausted. The discharge of blood was very moderate, and the accoucheur thought it best to defer the extraction of the placenta. She recovered a little; but in about two hours from the birth of the second child she grew suddenly faint; she breathed short and feebly, and died in about half an hour.

To these cases I add one which occurred to myself not long ago:—An elderly lady was suffering from a slight attack of bronchitis, to which she was subject on any considerable change of the weather. There was reason to believe that she was labouring under organic disease of the heart. From the history of her illness it appeared that two years before I first saw her she had suffered a severe attack of regular acute carditis. From this illness she had never perfectly recovered. She now complained of want of strength, short respiration, especially on quick motion, and occasional fluttering at the heart.

These symptoms were accompanied with a change of complexion from a clear and rosy hue to a pallid colour, assuming somewhat of a leuco-phlegmatic character.

She was now recovering, as I have said, from a slight attack of bronchitis. I saw her at mid-day. She continued well during the afternoon, conversing cheerfully with her friends; and as they were tenderly attached to her, they had been rendered happy by the more than usual excellence of her spirits. At six o'clock she drank tea. Immediately afterwards she complained that she felt rather tired: her maid undressed her, and assisted her into bed. As soon as she was in bed she said, "I now feel quite easy, and am sure I shall sleep well, and be quite well in the morning. Pray," added she, turning on her side, "let no one come into the room until I ring." The servant, who was leaving the room, thought she saw her countenance change; she ran up to her, and found her in what she thought a fainting fit. In less than ten minutes she was dead.

You see at once under how great a variety of circumstances cases of this class may give rise to the suspicion of poisoning—how truly alarming they are under all circumstances—how anxiously friends and relatives must wish to know the exact cause of death—and how earnestly the well instructed practitioner will endeavour to ascertain it as clearly as possible, as a most interesting scientific fact.

There is no difficulty in discovering the real nature of the affection; it is plain, palpable, and uniform. In all cases the morbid appearances in the dead body are the same. The muscular substance of the heart is morbidly changed, both in consistence and in colour. It is soft, flaccid, and easily torn—sometimes so easily, that it is broken down by slight pressure with the fingers. The change of colour which accompanies this softening differs in different cases, apparently according to some differences in the nature of the malady. Either the natural redness of the muscular fibres is increased, so that they become of a claret or violet colour, denoting a preternatural accumulation of blood in the vessels; or, on the contrary, their redness is diminished, so that they become of a faint yellow or fawn colour, compared by Laennec to the tint of the palest dead leaves, denoting a deficiency of blood in the vessels. That this structural change in the substance of the heart may be the result of inflammation is indubitable; for the first species, that with increased redness, existed in the case recorded by Dr. Latham, in which pus was infiltrated throughout the whole muscular substance of the heart. But the second species, that with diminished redness, may also be the result of inflammation; for inflammation of other organs, as of the brain, uterus, kidneys, spleen, &c. often terminates in the softening of their substance. Commonly, however, the second species is supposed to be the consequence of diminished nutrition; and it is generally observed to occur in subjects who have long been in a cachectic state, and in whom some chronic disease has enfeebled the nutritive functions. In this state of the system it is conceived the muscular fibres of the heart do not receive a sufficient quantity of blood duly to nourish them, and therefore are incapable of contracting with sufficient energy to carry on the circulation. The whole arterial capillary system is supposed to be in a state of atony, in consequence of which they do not return to the heart sufficient blood to excite it to contraction; while the blood-vessels of the heart itself, partaking of the general atony of the vascular system, do not receive an adequate supply of arterial blood to nourish the organ; whence its gradual change of structure.

There is reason to believe that this condition of the heart may exist for an indefinite period, without producing any very striking symptoms of disease, as long as no external circumstance occurs materially to embarrass the circulation; and sometimes even when such a circumstance does occur, and life is put in the most imminent danger, that danger may be removed by the prompt employment of the proper remedies. You may be called before life is extinguished, and it may be in your power to avert the fatal event.

A gentleman was attacked suddenly with an uneasy feeling in the thorax; short and difficult respiration, and a most oppressive sense of sinking; the action of the heart was so feeble that it could hardly be distinguished; the pulse was scarcely perceptible, beating not more than twenty in a minute, and greatly oppressed; at the same time the vessels of the skin and the tunica conjunctiva of the eye were loaded with blood. The most active stimulants, consisting of brandy, ammonia, and ether, with a blister at the scrobiculus cordis, at length succeeded in rousing the action of the heart; the circulation became gradually restored to tolerable vigour, and in twenty-four hours the patient was out of danger.

Some time ago I was called to a young lady labouring under very similar symptoms. She had previously been in tolerable health, but she was very corpulent. She had heard suddenly a piece of news that distressed her exceedingly. Instantly she was taken ill with vomiting and purging. These symptoms lasted two or three

days, and then she was seized suddenly with an intolerable sense of sinking at the pit of the stomach: the entire surface of the body was pallid and cold: the action of the heart was scarcely perceptible; the pulse was a mere thread, the expression of the eye was anxious and wild, and she had the feeling that she must die, unless the uneasiness at her heart were speedily relieved. Active stimuli, vigorous friction along the spine with camphorated spirit, very active friction over the whole surface of the body with a hard flesh brush, a blister to the region of the heart, at length roused the heart to action, restored the circulation, and in less than twenty-four hours, relieved her, as she expressed it, of an overwhelming load.

In all these cases the proper remedies, which must be most actively employed, are the horizontal posture, brandy, wine, ether, ammonia, vigorous friction, a pint of camphorated mixture, thrown up as an enema; all the medicines and drink warm, and warmth applied to the surface, and especially to the region of the heart, and to the extremities.

6. *Induration.* — But the muscular substance of the heart, instead of being softened, sometimes undergoes a process of induration, which has been found carried to such an extent, that the heart, when struck, sounded like a dice-box, or hollow horn vessel; and the scalpel, on making an incision, experienced great resistance, and produced a singular crepitating noise; yet the fleshy substance possessed its proper colour, and did not appear to be converted either into an osseous or cartilaginous, or any similar substance. This affection is extremely rare.

7. *Tuberculation.* — But there are other organic diseases affecting the muscular substance of the heart, which are capable of producing death as suddenly as the most rapid variety of poisoning with hydrocyanic acid; and when the duration of the symptoms is unknown, they often do give rise to the suspicion of poisoning. The organic derangement is often apparently trivial, and it may lie altogether latent until the moment it causes death. These two remarkable facts you must not forget. An illustration of this is afforded in the sequel of a case to which I adverted in a former lecture—that of the man who was discovered in a state of insensibility by his workmen, who died in a few minutes after this sudden attack, and who, having been ill treated by his wife, was suspected of having been poisoned by her. On the inspection of the body, the only appearance of disease that could be detected was a considerable tuberculation of the septum cordis, and of the anterior parietes of both ventricles. Slight as this organic derangement was, it was unquestionably the cause of death, although no symptom had indicated any disease of the heart. But in the face of a suspicion of poisoning, was this slight organic disease sufficient to warrant the inference that it *must* have been the cause of death? Might not the man have died of poison, the disease of the heart continuing latent? Certainly he might. From the dissection you could in this case only infer that there was sufficient to account for death in the disease of the heart. In the absence of all suspicion of poisoning, you would have no hesitation in referring the death to the disease of the heart; with a suspicion of poisoning, you could infer that this disease was actually the cause of death, only from other facts. Poisoning, however, as we have seen in this case, was out of the question, for the man was dead in ten minutes after the first attack of illness. He expired at two; he had breakfasted at nine; he had taken nothing whatever into his stomach in the interval. Now, as has been already shewn, no poison but one of the most active narcotics, taken in a large dose, could cause death with this rapidity; and the operation of such a poison in such a dose could not possibly be suspended for five hours. Poisoning, therefore, being utterly impossible, the only cause of death that remains is the organic disease; that is sufficient to produce death; and the just inference therefore is, that it was the true cause of death.

8. *Hypertrophy.* — I need merely direct your attention to another morbid condition of the muscular substance of the heart as the frequent cause of sudden death, the converse condition to that of softening, namely, hypertrophy, which consists of an augmentation of the muscular substance from increased nutrition. With the varieties, the causes, and the diagnosis of this affection, you are familiar; but notwithstanding the certainty with which the existence of this disease may in general be detected during life, yet many cases actually occur in which it proves fatal without its existence having been suspected. The death produced by it, therefore, though really a slow process, may appear to be surprisingly sudden, and may occur under circumstances which give rise to a suspicion of poisoning. Now, in order to determine whether the different chambers of the heart have undergone any real enlargement, it is necessary to have an accurate conception of the natural dimensions of the organ; but since these dimensions vary according to age, sex, and other circumstances, they cannot be judged of by any positive standard. It is, therefore, necessary to determine by the eye, and an experienced eye

is necessary, whether the proportion of the heart to the system, and of its several parts to each other, are perfectly natural.

The proportions assigned by Laennec, which approach, perhaps, as near the truth as it is possible to arrive, are as follows:— "The heart, comprising the auricles, ought to have a size equal to, a little less or a very little larger than, the fist of the subject. The walls of the left ventricle ought to have a thickness a little more than double that of the walls of the right; they ought not to collapse when an incision is made into the cavity. The right ventricle a little larger than the left, and having larger columnæ carnæ than the left, notwithstanding the inferior thickness of its walls, ought to collapse after an incision has been made into it. Reason indicates, and observation proves, that in a sound and well-built subject the four cavities of the heart are within very little equal to each other. But as the walls of the auricles are very thin, and those of the ventricles have much thickness, it results that the auricles form scarcely a third of the total volume of the organ, or the half of that of the ventricles. In the fœtus, and very young children, the thickness of the left ventricle does not exceed that of the right to the extent described.

The right cavities are larger than the left; and this is not owing to sanguineous distension attendant on dissolution; for the disparity is found, though in a less degree, in animals destroyed by hæmorrhage.

Dr. Hope states, that the four cavities of the heart are very nearly equal in capacity; but as the parietes of the auricles are very thin, and those of the ventricles are thick, the auricles, when simply full and not distended, form only about one-third of the total volume of the organ; or what is the same thing, the volume of the auricles equals about half that of the ventricles. The right auricle being generally found in a state of distension, and being of a more elongated flattened form than the left, has the appearance of being considerably larger, though in reality it is only a little so.

9. *Aneurism of the substance of the heart.*— Another affection of the heart capable of producing sudden death is aneurism of its substance, which, though a rare disease, still occasionally occurs, and you must be prepared to meet with it. The drawing before you presents an excellent representation of this affection.

10. *Aneurism of the great blood-vessels.*— It is far more common for the great blood-vessels in immediate connexion with the heart, to be dilated into an aneurismal sac, the progressive growth of which, and its ultimate and sudden rupture, as a not unfrequent cause of sudden death, is familiar to you.

11.—*Accumulation of fat.*—Often the heart is overloaded with fat, which "deposited between the pericardium and the muscular substance, not only covers the organ externally, but frequently penetrates a considerble depth between its fibres; while the walls themselves, as if losing, (probably by the pressure) what the adipose tissue gained, become attenuated and flabby." In this manner the preternatural softening of the muscular substance, which has been already described, and this morbid obesity, are often combined, and both concur in producing the fatal event.

12. *Osseous and cartilaginous depositions in the muscular substance.*—Osseous and cartilaginous depositions in the muscular substance are not common, yet they do occasionally occur, and in the specimen before you you see a lamina of bony matter covering nearly the whole of the left ventricle. Of course, such a deposition of bony matter must greatly impede the action of the heart, and may lead to such an obstruction to the circulation as to occasion instant death.

13. *Thickening, ossification, and warty vegetations of the valves.*—A far more frequent affection of the heart is thickening and ossification of the valves, about the mouths of its great vessels, and more especially of the semilunar valves of the aorta and pulmonary artery.

These diseases of the valves are much more rare on the right than on the left side of the heart; Dr. Latham thinks, in the proportion of one to three. Dr. Hope says, "I have notes of eight cases in which valvular disease existed in the right, and can recollect several others. In six of the eight, the left side was simultaneously affected, and generally to a much greater extent; but the proportion which the whole numbers mentioned bears to the cases I have seen of diseases on the left side, is less than that indicated by Dr. Latham, not exceeding, I think, one in four and a half to five. It is remarkable, that in all my own cases, and nearly all those of the authors quoted (with the exception of Dr. Latham, who is silent on this point) the induration on the right side was merely cartilaginous. When the two sides are affected at once, it very rarely happens that the disease on the right side is greater than that on the left; in general it is much less, being comparatively slight and incipient.

Warty vegetations of the valves bear a close resemblance to venereal warty vegetations on the external organs of generation. Their form is in general irregularly spherical, oval, or cylindrical; their size varies between that of a small pin's head

and a large pea; but when isolated, they are occasionally as large as a horse-bean. They are found either isolated, in clusters, or in closely agglomerated patches like cauliflowers. Their number is various; sometimes there are only one or two, and sometimes they pervade the whole of the valves, the tendinous cords, and a great portion of the auricle. Their texture is fleshy and slightly translucent, like the exuberant granulations of an ulcer. In consistence they are in general soft and humid, as if only recently and imperfectly organized, and they can be easily scraped off with the handle of the scalpel; but sometimes they are firm, and cannot be detached without tearing with the nail or cutting with the edge of the scalpel. Diseases of the valves, whatever be their nature, whether osseous, cartilaginous, or warty, have for their common effect to obstruct the orifices of the heart, and this they do, either by contracting the apertures, or by encumbering the valves in such a manner as to prevent them from opening and closing with suitable accuracy and facility. A mechanical obstacle is thus presented to the circulation, and from the obstruction and embarrassment which it occasions, are derived the symptoms of valvular disease. The exact time and manner of the fatal termination, in valvular disease, as in every other organic affection of the heart, is very uncertain. Sometimes the patient is reduced gradually to an extreme degree of emaciation and debility, and dissolution is duly announced by the usual premonitory symptoms. Sometimes he expires suddenly after any trifling exertion or emotion, though the malady have made comparatively little inroad on the constitution. In this case the event must be attributed to the obstruction having attained, by the progress of the disease, such a point that the heart can no longer maintain the circulation against it.

14. *Angina pectoris.*—That peculiar disease familiar to you under the name of angina pectoris, often, though by no means invariably, connected with ossification of the coronary arteries, is a very frequent cause of instantaneous death. It is, indeed, seldom that this affection proves fatal in the first attack; perhaps it never does so; yet, after frequent attacks it constantly proves fatal at last, quite suddenly. Now it is very possible that it may prove fatal under circumstances which give rise to the suspicion of poisoning; and when this happens you will be able to ascertain the true nature of the case, as well from the previous history as from the dissection, and from the absence of the proper signs of poisoning.

15. *Rupture.*—The last affection of the heart, as the cause of sudden death, to which I shall direct your attention, is rupture. Of course this event cannot be occasioned by the causes which ordinarily precede it, and which thus immediately produce sudden death in a previously sound state of the organ. The disease is usually preceded by ulcer, abscess, softening, dilatation, ossification, &c. These diseases may be latent for a long time, and when they have proceeded to a certain extent, then any thing that produces a powerful effect on the nervous system, or that greatly accelerates the circulation, may prove instantaneously fatal. To die of a broken heart, is often not merely metaphorical. A physical change slowly but progressively goes on; and when it has reached a certain point, some great moral cause, some powerful passion or emotion, produces the event.

Philip V. died suddenly on being told that the Spaniards had been defeated; on opening the body, the heart was found ruptured.

The father of a numerous family lost his wife, whom he tenderly loved; he was suddenly seized with laborious respiration, and died at the end of two days: on opening the body, the heart was found ruptured.

A robust and plethoric female, aged 22, addicted to intemperance, complained for some time of slight and apparently rheumatic pains. Within a day or two of the fatal event, she had been deserted by the man to whom she was engaged in marriage. In consequence of this, her mind became deeply affected. After having supped on the preceding night, she retired to rest as usual. In the morning she was found dead in bed. She lay in a bent position, on the left side, and was supposed at first to be in a profound sleep: neither the countenance nor the limbs were in the least distorted. On dissection, it was found that the pericardium contained two ounces of coagulated blood, and two of serum; that the heart on all sides was covered with it; and that it was much loaded with fat. The breach from which the effused blood had issued was at the summit of the aortic ventricle. The parietes of the ventricle around the rupture were much thicker than in the natural state, and on close examination a very sensible fluctuation was distinguishable to the extent of an inch on one side of it, from which floeculi of a cheese-like substance were discharged on pressure. The pericardium also presented traces of inflammation.

It is stated by Dr. Hope that it is generally in the left ventricle that the rupture takes place—a circumstance which at first appears remarkable, since this ventricle is

the stronger; but for the same reason it contracts more energetically, and as the rupture occurs during the contraction, we have thus an explanation of the phenomenon. It might be objected that, supposing the strength of the muscle and the energy of its contraction to be in the direct ratio of each other, the explanation offered would not account for the phenomenon. To this it might be replied, that it is only strong muscles which *do* undergo rupture from the energy of their own contraction. Hence rupture of the auricles is much more rare than that of the ventricles. You must bear in mind that rupture of the heart or of the great vessels into the pericardium, may not prove immediately fatal, because a solid coagulum or a fibrous concretion may arrest the hæmorrhage for a few hours. Of ten cases of rupture mentioned by M. Bayle, eight died instantaneously, one in about two hours, and another in fourteen.

Such, then, are the main diseases of the organs of the thorax which produce sudden death, and which may satisfy you, whenever you meet with them, that you have an adequate cause of natural death.

LECTURES ON BLOOD-LETTING.

Delivered from time to time,
At the General Dispensary, Aldersgate Street,

By Henry Clutterbuck, M.D.
Formerly one of the Physicians to that Institution.

Lecture VI.

Of the Quantity of Blood required to be drawn for Medicinal Purposes; and the ill consequences of excess.

It were much to be wished that any general rule could be laid down, upon which we might rely with confidence, for determining the quantity of blood required to be taken away in particular cases of disease, (due reference, of course, being had to age and other obvious circumstances) in order that while enough was done to insure the end we have in view, we might avoid carrying the evacuation to a hurtful extent. But however desirable, it will be found a most difficult task to accomplish. The circumstances requiring attention, as more or less influencing the result, are so numerous and various, some of them, probably, but imperfectly understood, that we may despair of arriving at any thing like certainty or precision on the subject. Not only age and strength, but mode of life and peculiarity of constitution—season and climate—the nature, stage, and even the particular seat of the disease, have all a marked influence on the result. Thus it is found that in one individual the abstraction of six or eight ounces of blood accomplishes all we wish; while, in another, twenty or thirty ounces are required for the same purpose. On one occasion, the disease yielding to a single blood-letting; while, in others, many repetitions of the operation are required, to produce the desired effect. These differences do not depend merely upon the strength of the patient, but upon the disposition there may be at the time to the continuance of the diseased action, whatever this may be. This disposition is not only different in different individuals, but appears to be continually changing; while it is not possible to judge of it by any obvious signs. Hence the uncertainty of our art, that has been so long and so justly complained of.

Speaking generally, however, in regard to quantity, it may be stated, that in adults of middling stature, and of good general health, from eight to ten or twelve ounces may be considered a moderate bleeding, and that from sixteen to twenty ounces may be deemed large. There are, however, many cases of violent and recent disease, in which the largest quantity here mentioned may be safely and properly exceeded. On the other hand, in diseases of long standing, where a cure is only attainable after a considerable lapse of time, it is hardly ever advisable to bleed largely at once. In such cases, the quantity of blood proper to be drawn at one time should seldom exceed five or six ounces; this being commonly enough to make a salutary impression on the system. A repetition, however, of the evacuation to the same amount, is generally requisite on such occasions, and may be borne for a great number of times, at reasonable intervals, without inconvenience, and with the most unequivocal advantage: as in many chronic affections of the lungs, heart, and various other organs, which you will find to be most successfully treated in this way.

There are, no doubt, many urgent cases of disease in which it may be justifiable to carry blood-letting to a much greater extent than I have now stated, both as regards quantity and repetition. No precise rule, however, can be laid down on the subject. All I shall attempt in this respect is, to shew you, by examples drawn from various and unexceptionable sources, what, in particular instances, has actually been done, not only without any immediate danger, but with decided advantage to the patient.

Hippocrates, as well as other of the

ancient writers, recommend bleeding the patient to syncope in acute disorders; but without, in any instance, mentioning the precise quantity of blood proper to be drawn. Galen seems to have been the first to do this, and he cites, as an extraordinary event, a case of violent fever, in which 54 ounces of blood were taken in a single day.

Vanswieten observes* that he knew a woman who, on account of some mental disorder, had been bled more than sixty times in the space of a year; during which, she grew extremely corpulent. Dodart†, a French physician, remarks, that upon 16 ounces of blood being taken from a man, previously in good health, the loss was repaired in the short space of five days—meaning, I suppose, that the body acquired, in that time, its former weight. This, however, you will observe, is rather equivocal; for the loss of weight might have been partly supplied by aqueous absorption from the atmosphere. Dr. Rush‡, of Philadelphia, says, that 90 ounces were taken at one time from his friend, Dr. Dewees, and with advantage. The latter physician, Dr. Dewees, himself states that he took 80 ounces of blood, within a few hours, from a young and delicate woman, who had been seized with convulsions towards the end of pregnancy; and from another similarly affected, at the commencement of labour, he drew 120 ounces within five or six hours, and 20 more on the following day. The patient notwithstanding, he says, recovered rapidly, but became blind, and continued so for a fortnight, and did not perfectly recover her sight for six months after. I may observe here, by the by, that you are not hastily to conclude, as some might do, that the blindness in this instance was caused by the loss of blood; a much more rational cause is to be found in the affection of the brain that first gave rise to the convulsions. The same writer cites various other instances, where a similar practice was pursued, and with the same good effect §.

Dr. Frank, of Vienna, relates, that he bled a person, 80 years of age, nine times, in order to relieve him from pneumonia. The late Dr. James Currie, of Liverpool, was bled to the extent altogether of 200 ounces, between the months of October and May, and was apparently rescued by this from pulmonary consumption. He lived for many years after in the extensive practice of his profession, and in the enjoyment of good health, though he ultimately fell a victim to the same disease*. Dr. Barlow, one of the writers in the Cyclopædia of Practical Medicine (art. "Plethora"), mentions the case of a feeble and emaciated boy, who was labouring under diabetes, and from whom 209 ounces (lb. 13) were taken at twelve successive bleedings, in the space of 51 days; which is at the rate of 19 ounces to each bleeding: he was bled twice a week, on the average. The effect, Dr. B. says, was striking: from a state of feebleness, hardly admitting of an erect posture, the lad acquired a degree of vigour which enabled him to hold the plough for several hours a day. The blood, too, from a dark grumous coagulum, of loose texture, exhibited afterwards the thickest and firmest buff. In another case, of a weakly and delicate female, without any special disease or local ailment, except pains irregularly alternating in the head and chest, 106 ounces (nearly lb. 7) of blood were taken, we are told by the same author, at seven bleedings, (*i. e.* 15 ounces each time, on an average), with a similar change in the appearance of the blood, and a well-marked improvement in the general strength.

The following, as well as many of a similar nature, came under my own observation at the General Dispensary, and were noted down at the time.

E. J., æt. 14, a delicate girl, affected with chorea. She was bled 17 times between the months of December and March, 1823, losing from 4 to 6 ounces at a time. At the end of this period she had recovered her ordinary health and strength, and the convulsive movements had almost totally ceased.

I find mention, also, in my note-book of October 1832, of the case of a young woman, 18 years of age, who, three years before, was a patient at the Dispensary, labouring under epilepsy, which had recurred frequently for a year or more previously. She was treated principally by small bleedings, from 6 to 8 ounces at a time, and was bled at least 60 times in the space of a year and a half—often at her own urgent request, on account of the relief she always experienced from the evacuation. The fits left her gradually; and, for the last year and a half, have not recurred. She is now in good health, and as florid in complexion as if no blood had been drawn.

July, 1836.—Mrs. M., whom I saw in consultation with my friend Mr. Coward, has suffered various attacks of inflammation at different periods, for some years back. The uterus, the chest, and (latterly) the brain, have all been affected in turn,

* Commentaries, 12mo. vol. i. p. 282.
† Histoire de l'Acad. des Sciences, an. 1772, 134.
‡ Med. Inquiries, vol. iv. p. 239.
§ See Dictionnaire de Méd. p. 23, et seq.

* Life of James Currie, M.D. by his Son.

and with so much violence, as to call for frequently-repeated blood-letting, which has always been productive of great, though only temporary, relief. A minute account has been kept of the different bleedings she has undergone, from which it appears that she has been bled 58 times in the arm, and been cupped 5 times; the quantity of blood taken at a time varying from six to eight, to ten or twelve ounces; besides which, she has had applied, at different times, at least 1000 leeches. Her complexion, after all this loss of blood, is not remarkably pallid, nor is she much emaciated, although her diet has been of the simplest and scantiest description. For the last year or more she has (with very few exceptions) subsisted solely on Scotch oatmeal, a few teaspoonsfuls at a time, taken in the dry state, and washed down with water or weak tea; the stomach refusing all other food.

I saw lately a young and generally healthy man, who, on account of disordered action of the heart, which was supposed to originate in inflammation, had been bled five times in as many days—to the extent of two pints on each of the two first bleedings—a pint and a half on the two succeeding ones—and a pint on the last occasion. This quantity seemed to me to be unnecessarily large; and, in fact, it had not accomplished the object aimed at—that of quieting the disordered action of the heart. This, however, was effected in the space of two or three weeks, by the use of small doses of opium, with a moderately nutritious diet. No *œdema* of the extremities followed, nor other inconvenience, and the patient soon recovered his ordinary strength*.

The statement you have just heard is not brought forward as a matter for imitation, on ordinary occasions, but only to shew, that where urgent circumstances appear to demand the employment of such active means (as being presumed necessary to the immediate safety of the patient,) we are justified in having recourse to them, where other means fail. At the same time you should constantly bear in mind the evils that may arise from an intemperate or excessive use of the remedy, so that you may at all times resort to it with all the caution that the circumstances allow of.

Before quitting this part of our subject, I cannot avoid adverting to some late attempts that have been made to lay down precise rules for the safe and effectual administration of blood-letting, because they appear to me not sufficiently in accordance with general experience, and liable, if acted upon, to lead at times to dangerous results. I feel myself the more called upon to do this, on account of the distinguished rank which the author holds in the profession, as well as from his being a teacher in one of the most respectable medical schools of this metropolis—circumstances that cannot fail to give considerable weight to his opinions*. The rule he lays down is thus enunciated:—

"If, in any case, we place the patient upright, either sitting or standing, and bleed to incipient syncope, we abstract precisely the quantity of blood which the patient will bear to lose, and also which the disease requires to be withdrawn for its relief." "These two facts," it is added, "are precisely commensurate with each other; so that to bleed to incipient syncope, is to bleed precisely according to the exigencies of the case." Nothing can be more simple or intelligible than this, and nothing, certainly, more important in regard to practice, if well founded; but this I am disposed to question.

There are here two distinct propositions, either of which may be true or false, without affecting the other. The first is, that an approach to syncope (where the patient is bled in the erect posture) is a test of the quantity of blood that he can bear to lose; or, in other words, that may be drawn with safety, or without danger to life. This may be, and, generally speaking, probably is true; but I know of no sufficient proof derived from actual experience of its being so. It is plain that such a fact could not be established by previous reasoning, or upon physiological grounds. Nothing but actual observation, and that upon a large scale, or else direct experiment, could establish such a point. But it is not, and has not been, the custom to bleed patients in this manner; so that nothing like general experience can be appealed to in its behalf. As little can it be imagined, that it is in the power of

* For examples of boldness in the use of the lancet, I may refer you to the practice of some of our surgeons. In one of the great hospitals of this metropolis a case occurred lately, where 128 ounces of blood (8 lbs., or 1 gallon!) were drawn at one time, in order, by inducing syncope, to facilitate the reduction of a dislocation of the thigh. The patient lived a week afterwards, and then, as is said, died from inflammation of the vein punctured. A still more extraordinary account is given in a late Treatise on Blood-Letting, where it is stated by the author, in an unqualified manner, that "he has known instances of delicate females losing, *daily*, from half a pint to a pint of blood, from hæmorrhoidal tumors; and that for many months, and even years!" I confess I have seen nothing approaching to this.

* See an Essay on Blood-Letting, by Dr. Marshall Hall; and also a paper of his, on the same subject, in the Cyclopædia of Practical Medicine; likewise, the abstract of his lectures, as published in some late numbers of the Lancet.

any individual to institute a sufficiently extensive series of experiments on such a subject; and it is a matter of too great moment, practically speaking, to be admitted on narrow or trivial grounds. It is not known what quantity of blood the system will bear to lose consistently with life, the quantity probably differing greatly in different individuals. But it is certain that very different quantities may be lost before syncope occurs, this depending not merely upon the posture in which the patient is placed, but upon the manner of drawing, and the rapidity with which the blood is taken, as before stated. The approach of syncope, therefore, can never afford a precise rule for our guidance, unless it could be shewn that quantity has no influence in the production of that state. It is far from certain that actual syncope can at all times be induced without danger to life; nor can we regulate its approach with any certainty. A person, it is well known, will bear to lose a much larger quantity before he faints, when the blood flows slowly, than when it is rapidly drawn. The drawing it slowly is much the same, therefore, as if he were bled lying down. If, then, as the author admits, it be dangerous to bleed to complete syncope in the recumbent posture, the same danger is likely to be encountered where the patient is bled slowly while erect; so that the safety of the rule here laid down may be justly questioned.

The author's second position, namely, that "to bleed to incipient syncope is to bleed precisely according to the exigencies of the case," appears to me to be equally untenable, and in opposition, indeed, to almost daily experience. It would seem to be the author's opinion, that the occurrence of syncope is the circumstance upon which the cure depends, and not at all the quantity of blood lost. It is true, doubtless, that during the state of syncope the diseased as well as the healthy actions of the system are suspended; but this suspension is in general only temporary, the diseased action being renewed as the patient recovers from the fainting state, and not seldom with all its former violence. This is especially likely to happen where fainting comes on when only a small quantity of blood has been lost. The occurrence of syncope, therefore, and still less an approach to it, is not a precise measure of the quantity of blood required to be drawn for the relief of any particular disease. Many diseases cannot be cured without repeated abstractions of blood; nor is the necessity for these lessened, in such cases, by the occurrence of syncope, which, indeed, contrary to what is here stated, takes place more readily on the first bleeding than on the subsequent ones. The rule is objectionable, too, in another respect, as leading, in many instances, to an unnecessary waste of blood. The abstraction of eight or ten ounces of blood, for example, will often suffice for arresting the progress of inflammation, without the least tendency to syncope being perceived; so that the disease will immediately begin to decline, and at length go off altogether, without any further loss of blood, though several days may perhaps elapse before the disease wholly subsides. By carrying the bleeding, therefore, to the extent of inducing syncope in every case, as here suggested, an unnecessary quantity of blood will often be lost—a thing always to be deprecated.

Of the Evils resulting from excessive Loss of Blood, and the means of obviating them.

Many objections have been made to the practice of blood-letting, some of which are of a very frivolous nature. It is, for instance, a vulgar, I hardly need add, an unfounded notion, that the first bleeding always cures. In reliance upon this notion, the remedy is sometimes postponed till its use becomes equivocal, if not improper; or, at all events, much less advantageous than it would have been if employed at an earlier period. Another objection is, that if the practice of bleeding be once begun, a habit will be formed, so that it cannot afterwards be dispensed with. But there is little more foundation for this opinion than the former. It is objected, again, by some, that the practice is an unnatural one, and that it is even irreligious, and in opposition to Scripture; which says, "The blood is the life," and that "the taking away life is prohibited by the laws of God and man." This hardly merits a serious answer. We are daily called upon to avert a greater evil, by voluntarily encountering a lesser one. We part with a limb, in order to preserve life. If by blood-letting we can prevent the body from falling a prey to an otherwise indomitable malady (as is unquestionably often the case,) surely no argument is needed to prove its propriety. When I tell you confidently, as I am warranted by general experience in doing, that blood-letting is, in innumerable instances, essential to the immediate preservation of life; and, further, that by the neglect of it, a foundation is often laid for diseases of a chronic kind, that are even more fatal in their consequences than the most acute, you will not, I trust, be deterred, by futile and unfounded objections, from the performance of a great and obvious duty. In resorting to this, as well as to all other powerful remedies, we have only to inquire whether the object sought be of sufficient magnitude, in com-

parison with the existing evil, to warrant the adoption of such a remedy; if it be, we have our justification, and we incur great moral responsibility if we hesitate. You are to remember, that the blood is in a state of constant renewal by the food that is daily taken; so that, in favourable circumstances, the loss sustained is soon again supplied. Blood letting, therefore, does not necessarily occasion any permanent loss to the system; nor does it, generally speaking, produce other unfavourable changes in regard to the general health—always, of course, supposing that the remedy is used with due discrimination and judgment. The arguments, in short, that may be adduced in favour of the practice, (when properly conducted,) outweigh, a thousand-fold, every thing that can be urged against it.

A great dread of blood-letting prevails, (a dread in which not a few of our professional brethren seem to participate,) on account of its supposed tendency to produce dropsy, if carried at all too far. Under this apprehension, practitioners often abstain from bleeding, even in acute cases of inflammation, or, at least, are apt to discontinue its use before the object is sufficiently attained. The consequence often is, that the inflammation continues, though perhaps in a subdued and half-cured form, yet quite sufficient to lay a foundation for chronic diseases, of different kinds and denominations, according to the nature of the part affected. Thus, (to give you two or three examples,) if the lungs be the part primarily affected, the long continuance of the inflammation, even in the slightest degree, gradually alters their structure; they become indurated, (tubercular), impervious to air, (hepatized, as it is absurdly called,) and more vascular, giving a tendency to hæmorrhage, (hæmoptysis). These, again, prove sources of disturbance to the whole vascular system, exciting a febrile state, (hectic,) with its usual concomitants, night-sweats and emaciation: in a word, giving rise to that frightful train of generally irremediable symptoms that constitute what is familiarly called Consumption or Decline, the great destroyer of life at the most interesting period of human existence, and which, in a large proportion of instances, may be traced to neglected or half-cured inflammation.

In like manner, where the mucous membrane lining the air-passages becomes affected by inflammation, (catarrh, or bronchitis, as it is now more generally termed,) especially when the disease extends pretty generally throughout the bronchial tubes, the affected membrane swells, and becomes thickened in its texture, so as to offer a mechanical impediment to the passage of air into and out of the lungs; with other symptoms that I need not here dwell upon. Now, in favourable cases of this sort, the inflammation generally subsides in the space of a few days; terminating, commonly, by a large formation and expectoration of mucus. But if the tendency to the disease be strong, and the inflammation often renewed, or if it be imperfectly cured, either by the employment of inadequate means, or by an insufficient use of such as are really effective, (especially blood-letting, which, in severe cases, is the only one deserving confidence,) the membrane is permanently thickened by the inflammation, till breathing becomes a matter of labour and difficulty; subject also to occasional exacerbations, that require immense efforts on the part of the respiratory muscles, in order to sustain life while the paroxysm lasts. Here you have a picture of confirmed or periodical asthma—a disease over which, when confirmed by habit, art has little influence, even so far as to afford temporary relief.

So again, if the membrane lining the close cavities of the body, the cranium, chest, or abdomen, be attacked by inflammation, and this be continued for a certain time, and in a certain degree, it is followed either by adhesion (a growing together of the inflamed surfaces), or by a preternatural secretion (effusion, as it is unmeaningly termed) of serous fluid into the cavity; and thus dropsy is formed—not produced, as commonly supposed, by blood-letting, but rather by the neglect of it, the inflammation being allowed to run on till it has produced one or other of the consequences mentioned. In short, dropsy is very generally, I believe I might say universally, a mere consequence of inflammation; at least I have never seen an instance that seemed fairly ascribable to blood-letting. The utmost in this respect that can be attributed to loss of blood, however occasioned, is an œdematous swelling of the lower extremities, owing, probably, to the weakened state of the capillaries and absorbents, but which hardly merits the name of dropsy, and which, as far as I have seen, is never formidable in its results.

The greatest loss of blood consistent with life, is that, probably, which occurs in cases of flooding in females; and in regard to this, I have repeatedly made inquiries of the most experienced accoucheurs, whether they ever witnessed any determined case of dropsy (I do not mean a mere œdematous swelling of the lower extremities, which is only a temporary inconvenience), occasioned solely by the loss of blood. Their answer has always been decidedly in the negative.

These, however, are not the whole of the evils that have been ascribed to loss of blood, when carried to excess. In the writings of Dr. M. Hall, above alluded to, you will find a variety of symptoms, and even primary forms of disease, attributed to this source—whether justly or otherwise remains to be seen. It is asserted, for example, that "delirium occurs as an immediate effect of loss of blood; and that mania or delirium, in a more or less continued form, is not an unfrequent occurrence, as a more remote effect of the same cause—that puerperal mania generally arises from loss of blood in those who are predisposed—and that a form of mania, not to be distinguished from puerperal mania (except by the history of the case), occurs likwise from loss of blood, independently of parturition;" and he goes on to say, that "coma, amaurosis, deafness, and paralysis, are other forms of the effect of loss of blood."

If such are really and unequivocally the effects of loss of blood, it is somewhat strange that they should not have been particularly noticed by practical writers in general. But, as far as I know, this has not been done. The author goes even further than this, and points out the following as taking place in what he terms "exquisite cases of the sort":—"intoleranee of light and sound,—agitated sleep,—delirium,—noises in the head,—flashes of light,—a sense of pressure or tightness in one part of, or around the head, as if the skull were pressed by an iron nail, or bound by an iron hoop;" and along with these, "a throbbing of arteries in the head, and beating of the carotids; with an increase of action of the heart and arteries in general." The train of symptoms here described, taken together, I harely need remark, used formerly* to be considered as indicating phrenitis or inflammation of the brain; and indeed, if they do not do this, it is hard to say of what use at all symptoms are, for the purposes of diagnosis. Here, however, they are ascribed to re-action, (a favourite term) by which is meant a state of general arterial excitement, or increased arterial action, and altogether independent of inflammation. It would almost appear, indeed, from the statement here made, that the greater the loss of blood, the greater will be the re-action that is to follow. That such a train of symptoms, however, should occur, as an effect of loss of blood simply, is scarcely credible in itself; and is not in accordance with any facts I have ever witnessed; nor is it supported, I believe, by any satisfactory testimony That loss of blood occasionally (though by no means constantly) induces irregularities in the general circulation, and, more especially, violent action of the cerebral arteries, is well known; but such effects, besides that they are by no means constant, are temporary only, and always accompanied by diminished action in the rest of the arterial system. Indeed it seems physically impossible that the whole, or nearly the whole arterial system, should, as here stated, be throbbing with violence, after it has been emptied of a large proportion of its blood. I would appeal to cases of simple hæmorrhage, without disease—such as occur after accidents or surgical operations—to prove that such a train of symptoms as is here described is not the natural or ordinary result of loss of blood.

The fact, I believe, is, that the author has drawn his conelusions from the puerperal state principally, and not from ordinary cases of hæmorrhage, whether the result of injury or induced by art. The cerebral symptoms described above as the direct effect of loss of blood, are occasionally, no doubt, met with in puerperal cases, where considerable hæmorrhage may have taken place; but the same symptoms occur where there has been little or no loss of blood. They are not, therefore, justly ascribable to the cause here assigned. Inflammation of the brain is an event far from infrequent after parturition, and would, of course, be attended with, or followed by, the symptoms here mentioned, — namely, "intolerance of light and sound — agitated sleep—delirium—noises in the head—flashes of light, and throbbing of arterics in the head," &c.; and, along with these local signs of inflammation, "an increase of action of the heart and arteries in general." All the circumstances attending child-birth, it is to be remembered, are powerful causes of excitement to the brain, and fully adequate to the production of inflammation in this part: such are, for example, the mental anxiety, the dread, and the great muscular efforts, that necessarily precede and accompany parturition. And if a predisposition to inflammation in the brain exist, as appears to be the case in some women, the same effect will be likely to occur in successive child-births; and that in spite of hæmorrhage, or of artificial blood-letting; for neither of these is absolutely, and at

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* *Géronte*—"Il n'y a qu'une seule chose qui m'a chequé; c'est l'endroit du foie et du cœur. Il me semble que vous les placez autrement qu'ils ne sont; que le cœur est du côte gauche et le foie du côté droit.

Sganarelle—"Oui, cela etoit autrefois ainsi; mais nous avons changé tout cela; et nous faisons maintenant la medecine d'une methode toute nouvelle.

Géronte—"C'est que je ne savois pas, et je vous demande pardon de mon ignorance."—*Le Medecin malgré lui.* Act 2; Scene vi.

all times, adequate, either to the prevention or cure of inflammation.

With respect to the other symptoms referred by the author to loss of blood, namely, "mania, coma, amaurosis, deafness, and paralysis"—these are merely the remote effects of the preceding inflammation in the brain, and in no way attributable to mere loss of blood.

I have dwelt thus upon the points in question, on account of what I conceive to be their injurious tendency in regard to practice. If loss of blood be really capable, as here supposed, of producing the effects mentioned, we might well hesitate to employ the lancet in such cases, however strong the indications for the use of it apparently might be; naturally enough considering, that what is capable of producing such symptoms, would not be likely to relieve them.

But although it be true that many of the objections made to blood-letting are without foundation, still you must ever bear in mind that it is an agent quite as capable of doing harm as good, and that the most injurious consequences may result from an excessive or intemperate use of this remedy. Syncope, convulsions, and death itself, are the well-known and immediate effect of loss of blood, when carried to a certain extent; while general debility, with an imperfect, and often irregular performance of some or all of the functions, are the equally well-known remote consequences, if the patient survive. Altogether, they afford a powerful reason against all unnecessary waste of the vital fluid, although they furnish no argument against the judicious, or even the most liberal employment of it, in violent and dangerous forms of disease that are of a nature to call for such a remedy; for the life of the patient may depend (as indeed it often does) upon an unhesitating application of this frequently-indispensable means of cure.

I shall next treat of the application of blood-letting to diseases individually.

REMARKS ON CARCINOMA OF THE MAMMA, AND ON THE INEFFICACY OF SURGICAL OPERATIONS IN THE TREATMENT OF THAT DISEASE.

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[Concluded from p. 382.]

I shall now advert briefly to a very important point, the right understanding of which will materially influence our views regarding the chances of success in the treatment of the disease. Is scirrhus of the mamma of local or of constitutional origin? Upon this point the opinions of medical men are much divided. Some maintain that the disease is of local origin; that it may occur in the healthiest individual; is generally referrible to external injury, and curable by ablation, provided all the diseased parts be removed; and that if it does contaminate the system, and become a disease of the constitution, it does so only secondarily. Were this view correct, we would not expect to find any difficulty in obtaining a permanent cure, and provided we got beyond the local disease, we would neither have to dread its reappearance in the system, nor in the vicinity of the part from which it had been extirpated. But are these favourable results realized in practice? On the contrary, do we not find that the disease of the mamma is almost always preceded by, and accompanied with, constitutional symptoms; and that on the intensity and violence of these symptoms, the progress of the external and the development of the internal or visceral disease, essentially depend? The fact, also, that this cancerous diathesis, which may be either congenital or acquired, occurs at a certain period of life, when the uterine functions have ceased, and important constitutional changes are manifesting themselves; and when we observe that all the tissues and textures of the body to which the cancerous action extends, however dissimilar, are equally involved in the ravages of the disease; that it may occur simultaneously in an internal organ and in the breast—parts which may have no direct connexion or communication with each other; that its reproduction, after unsuccessful operations, takes place not merely in or near the cicatrix, but also in internal and distant organs; we are constrained to acknowledge that it is of constitutional origin, and that until we can succeed in combating this peculiar state of the system, upon which the specific action of the disease depends, all our local applications and surgical operations must prove abortive. This opinion has been decidedly gaining ground of late years, and the researches of modern pathologists have aided in confirming it. We do not know what

the peculiar condition or disposition of body is which seems to precede and regulate these manifestations of cancerous disease, but it has been shewn by Professor Carswell that the material elements of the disease itself are contained in the blood *.

In regard to the treatment of carcinoma of the mamma, I shall consider shortly the three different surgical methods at present in use, for the purpose of attempting to estimate their value, and of ascertaining how far they have proved successful in eradicating or palliating the disease.

1. *By Escharotics.*—This mode of treatment has been long known, and its history has been diversified by the employment of a great variety of caustic remedies. Among the profession it has in a great measure fallen into disuse, having been found very painful and uncertain; but it has been, and continues to be, extensively employed by empirics. Within the last four or five years, however, the attention of the profession, both in this country and in France, has been again directed to this mode of treatment; and attempts have been made to convince us that it is less painful and more certain in eradicating the disease than extirpation.

Drs. Canquoin and Riofrey have published memoirs shewing the great advantage obtained from, and the numerous cures effected by, the use of the chloride of zinc, when employed in the form of paste. In illustration of its efficacy, they relate cases in which the whole mamma, and all the tissues between the breast and the arm-pit, with the diseased glands in the axilla, were destroyed, exposing the muscles and laying bare the blood-vessels, not only with impunity but with success. After a careful examination of the histories and progress of several of these cases, I am not prepared to view them as having been all decidedly cancerous; and in some of the others, where the malignant nature of the disease is less equivocal, I doubt much if the apparent cures proved permanent. From what I have experienced, and I shall afterwards mention, of the great want of success in removing the disease by the knife, when the diseased parts are more immediately under observation, and can be followed out and more certainly extirpated, I am satisfied that, in other hands, the sanguine anticipations of the advocates of this new caustic treatment will not be realized.

During my late attendance at the Infirmary, and since that time in private practice, I have employed this paste in four cases of scirrhous mamma: in two cases where the disease had returned after ablation, and in several cases of cancer of the lip, and in cutaneous cancer of old people. In the latter affections, which are almost uniformly local, I have succeeded; but not in the former. On the contrary, although I have destroyed large portions of the mamma, and obtained for a time firm and healthy looking granulations, yet new tumors uniformly sprung up in the old site or its vicinity, and the general progress of the disease was always accelerated. The application, which had to be frequently repeated, produced great pain, and was not without its injurious effects on the constitution. In two of the cases, in which the mammary tumors were large, the febrile excitement was excessive, of more than two weeks' duration, and complicated with a violent gastro-enterite, which I attributed to the action of the medicine; and in patients of a cancerous diathesis, this constitutional excitement unquestionably hastened the progress of the internal disease.

2. *By Compression.*—This method of treatment has also had its periods of popularity and neglect. Young, in this country, and Recamier in France, have been its most powerful advocates. The former commenced this practice in 1809, and employed, for the attainment of his object, adhesive straps, sheet lead, tin plates, compresses, and bandages. He gives a few successful cases where the mamma only was affected, and also in which there were diseased glands in the axilla; and he alludes to others in which the practice was unsuccessful *.

Recamier, as one of the physicians of the Hôtel-Dieu, of Paris, has enjoyed ample opportunities of witnessing this disease, and of testing the efficacy of continued and methodical pressure, which he employs by means of compresses of agaric, bandages, &c †. Of

* See the article *Scirrhus*, in the Cyclopædia of Medicine, vol. iii.

* Cases of Cancer, &c. London, 1816.

† In estimating Recamier's success, it may be well to recollect, that the French physicians and surgeons are accustomed to apply the term scirrhus to every hard and indolent tumor, whether malignant or not, many of which would

100 patients treated by him, 30 were completely cured by compression alone (but he does not state in how many of these the disease was seated in the mamma); 21 were benefited by it; 15 submitted to ablation, after compression had failed; 6 were cured by compression and cauterization combined; and the remaining 28 were either incurable, or not benefited by any of the means employed *. On the other hand, this practice has been fairly tried by others, but without success. In the year 1816, Sir Charles Bell had recourse to it in 16 cases of occult and open cancer, in the Middlesex Hospital, without any apparent benefit †. My own experience of it has not been great. I have tried it, however, in several cases, and seen it practised by others, but without any decided advantage. It appeared to render the tumor harder and more compact, causing absorption, not of the diseased structure, but of the interstitial fluids, and surrounding adipose substance. It is, besides, a tedious and irksome process, to which few would willingly submit, if the slight chances of benefit were explained to them. Were it, however, to prove as successful in the hands of others as it appears to have done in those of Recamier, it would certainly be a milder and more efficient plan than any of the others we employ. The good effects of steady and well-regulated pressure in dispersing indolent swelling have been well known to the profession. When these indurations are chronic, and not of a specific character, the advantages of compression in promoting absorption and suppressing arterial action are undoubted; but I cannot bring myself to believe that in genuine cancer of the mamma this practice will be entitled to the commendations it now receives. In an immense majority, if not in every case, scirrhoma of the breast is of constitutional origin, and therefore not capable of being permanently eradicated by local means. Besides, it is not possible, even should we succeed in dispersing by compression a carcinomatous tumor of the breast, that we shall, instead of insuring the patient's recovery, accelerate her fate. The specific and malignant matter of cancer must be carried into the circulation by the absorbents, so that we are only exchanging an external for an internal disease.

3. *By Extirpation.*—This has long constituted our chief plan of treatment, and continues to be employed both as a means of cure and as a palliative.

In a curative point of view, the operation of ablation of the mamma continues to be extensively practised; and of all the local means we possess, it certainly holds out the best hopes of a permanent cure. If undertaken at an early period, when the constitution is but little impaired, and the countenance is unchanged—when the tumor is small, isolated, and indolent, and the axillary glands are sound, we are told that it seldom fails in eradicating the disease, provided all the morbid structure be removed. I admit, so far as the external disease is concerned, that its return to the same locality is often to be attributed to an imperfect operation, too much of the integuments or surrounding soft parts being left behind; but, at the same time, I have never seen a case, even of the most favourable description, in which the disease did not return, although every precaution was adopted to render the operation successful. We generally find that the disease is more extensive than we anticipated, and that, although we go far beyond its apparent boundaries, we seldom, if ever, succeed in preventing its reproduction. There may be points of disease in the absorbents, lymphatic glands, or surrounding adipose substance, so minute as to elude detection, or, as happened to Professor Camper and Sir A. Cooper, the absorbent vessels may carry the disease from the mamma to the glands *under* the sternum, where it could be neither discovered nor reached.

The peculiar state of the system upon which the disease seems to depend, presents another and still more serious obstacle to the success of our operations. There are, unfortunately, too many cases on record, and of daily occurrence in practice, in which scirrhoma of the mamma is shewn to be, in its origin, not local. All the cases I have detailed point out the co-existence of mammary and internal scirrhus, or the rapid and fatal supervention of the latter form of the disease. I have frequently seen cancer

yield to proper local treatment. Were the disease to be entirely and permanently removed by compression, I would feel satisfied that it was not malignant, for I cannot believe that pressure, however carefully and methodically applied, can possibly subdue the *specific action* of the disease.

* Recherches sur le Traitement de Cancer, tom. i. p. 550.

† Surgical Observations, vol. i.

of both breasts, and of the uterus and breast, occur at the same time, and oftener still, the external cancer is complicated with disease of the lungs, liver, &c., in which organs its progress may be so obscure and insidious that we cannot fix its locality, or assign to it precise limits. We are therefore not authorized in promising success, even from the earliest ablation of the mamma, since we cannot assure ourselves or our patients that the tumor we extirpate is the only part that has undergone this morbid change.

But the best of all tests for determining the efficacy of this operation is its success. It has been adopted for so long a period, and so generally, that were medical men to acknowledge candidly, and record faithfully, the results of their observations, we could have no difficulty in estimating the chances of ultimate success. Unfortunately, great diversity of opinion prevails as to its advantages; and whilst a considerable number of the profession acknowledge the liability of the disease to return, they are not less decided in recommending and performing the operation.

Mr. Hill, of Dumfries, who published, in 1772, on the results of operations in this disease, seems, at first sight, to have met with greater and more permanent success than has fallen to the lot of any surgeon before or since his time. He operated upon 88 cases of all descriptions, and only one in seven of these had a return of the disease. But, on examining the details more minutely, we find that in five of the cases only was the mamma extirpated; that in two of these the wound did not heal; in another, the disease returned; and the remaining two continued well at the date of publication*.

Sir E. Home has asserted that this disease is of local origin, and that it is capable of being safely and effectually extirpated†. Richerand, while he acknowledges that the disease returns in about four cases out of five, either in the original situation or in some distant part, states at the same time that the proportion of permanent cures is such as to justify the operation‡.

Sir A. Cooper states, "that a large proportion of cases return; but fewer than formerly, if the patient, immediately after recovering from the operation, undergoes an alterative course of medicine*." This implies that there is something wrong in the constitution, and that by rectifying this morbid condition, the return of the local disease is often prevented. It is acknowledging in fact, though not in direct terms, the constitutional origin of cancer of the mamma, and at the same time assigning to constitutional treatment more efficacy than I have ever seen result from it. I acknowledge the utility, in such cases, of attending to the uterine and alimentary functions; and I believe that if we shall ever succeed in controlling this formidable disease, it will be by acting on the general system; but as yet, we certainly do not possess any constitutional means of either warding it off or preventing its recurrence. Mr. S. Cooper states, "that modern experience has given ample encouragement to the early performance of an operation, and even to making an attempt to cut away the disease, in every instance, both of the occult and ulcerated kind, when such a measure can be so executed as not to leave a particle of the cancerous mischief behind†."

Professor Sym has met with "repeated instances in which, though other circumstances were by no means favourable, the tuberculated kind of carcinoma was extirpated with the happiest result‡."

Mr. Travers states, that "the poison of cancer does not act upon the system during the integrity of the tubercles, since persons generally recover, and finally, in whom the disease is freely removed in this early stage. On the contrary, if the tubercle be softened, and undergoing ulceration or absorption, the disease recurs, however freely the parts be removed §." It does not accord with my experience, as I have already stated, that free operations in the early stage of the disease are generally followed by absolute recovery; nor can I subscribe to the opinion, that the system does not become contaminated until softening or ulceration of the tubercle occurs. I admit that absorption goes on more rapidly during the soft or ulcerated stage, but at the same time I

* Cases in Surgery.
† Observations on Cancer. London, 1805.
‡ Nosographie Chirurgicale, 3e. edit., tom. iv. p. 424.

* Lectures, by Tyrrell, vol. ii. p. 198.
† Surgical Dictionary, article "Cancer."
‡ Principles of Surgery, p. 392.
§ Medico-Chirurgical Transactions, vol. xv. part 1, p. 225.

have seen many cases in which the disease has been conveyed to the axillary and the subclavicular glands, and into the system, while the primary disease of the breast retained its original hardness and density.

The utility of the operation has also been denied by many high authorities, both of ancient and modern times. Hippocrates*, Celsus†, Galen, and others, of the older writers, are decidedly opposed to it. Dr. Alexander Monro had the candour to announce his great want of success, which seems to have discouraged, for a long time, many surgeons from having recourse to it. He only met with four cases out of sixty, in which the disease did not return within two years after the operation‡. In 100 cases operated upon by Boyer§, where the disease was seated in the breast or other parts of the body, not more than four or five were radically cured, in consequence of which he declares, and the same opinion seems to have been entertained by several eminent French surgeons of his day, that an operation ought never to be undertaken when the disease has been ascertained to be genuine cancer. Delpech also acknowledges that the operation is rarely if ever successful‖, and the same opinion is entertained by a number of other authorities whom I need not mention. Believing that amid such discordant opinions we can only arrive at a just and satisfactory conclusion by an appeal to facts, and an accumulation of individual experience, I feel myself bound to contribute the results of my limited observations towards the fulfilment of so desirable an object. The momentous question must be finally settled by the combined experience of the profession, and not by a reference to the records of our hospitals and infirmaries, where we find the cases dismissed as cured so soon as the operation wound has healed.

Of thirty-two cases operated upon by myself, in which the carcinomatous nature of the disease was distinctly ascertained, the cure was not permanent in a single instance. The ages of these patients varied from 42 to 59 years; 23 were married and had children, and nine were unmarried. In eighteen cases the right breast was the seat of the disease; and in fourteen, the left. In twenty, the glands in the axilla were more or less affected, but not extensively; they were all removed, and in the remaining twelve no disease in the axilla could be detected. In ten the disease was in the form of tubercle; and in twenty-two the whole gland was affected. In nine cases the disease returned in the integuments of the chest, or in the axilla, within a period varying from six weeks to three months after the operation; in thirteen cases, from three to nine months; in four, from nine to twelve months; in three, within two years; and in one, nearly three years elapsed before its return was discovered. Two of the operations proved fatal; one from pleurisy, and the other from erysipelas. In many of these cases symptoms of pulmonary and hepatic affections were well marked, and seemed to have occasioned death; in others the symptoms were too obscure to lead to accurate diagnosis. The lungs appeared to be most frequently implicated, but the proportion cannot be accurately stated, as in several of the cases no post-mortem examination could be obtained.

Besides these cases which have occurred to myself, I have, on inquiry of a few of my medical friends, who have had opportunities of witnessing this disease, ascertained *the results of* 86 *additional cases*, in which the mamma was extirpated for well-marked carcinoma, and in not one of these was the cure permanent. Without going into details, I may shortly state, that in a majority the operation was performed at an early period, and under the most favourable circumstances; the affected parts were freely and extensively removed, and in many there was no distinct indication of constitutional deterioration; yet in all the disease returned, both externally and internally, and proved fatal. It was also observed, that in robust women of a sanguineous temperament, the reappearance of the disease after the operation, and its subsequent progress, were more rapid than in those of a nervous or lymphatic temperament, and that the internal organs were sooner affected.

It sometimes happens, especially when the disease occurs in very old persons, that it may remain for many

* Aphor. xxxviii., Sect. vi.
† De re Medica, Lib. V., cap. xxviii.
‡ Edinburgh Medical Essays, 3d edition, vol. v., p. 346.
§ Traité des Maladies Chirurgicales, tome vii., pp. 240, 241.
‖ Sur les Maladies Chirurgicales, tome iii.

years in a quiescent state, without much pain or tendency to shorten life. The anxiety of mind arising from a knowledge of the nature of the disease, sometimes induces such patients to forego the comparative tranquillity they enjoy, and attempt to free themselves of the disease by an operation. I could adduce several patients who had laboured under this malignant disease, for ten, fifteen, and twenty years, and who were cut off in three or four months by an operation.

Ablation of the mamma is also recommended and practised as a palliative, in cases where there is no prospect of a radical cure. It is had recourse to chiefly in the ulcerated stage of the disease, to rid the patient of pain and annoyance arising from the presence of an open ulcer, and the fœtid discharge proceeding from it. Seeing that we have other and milder means of soothing the distressing symptoms, and mitigating the patient's suffering, I must confess that this painful and dangerous procedure, and for effects so partial and inefficacious, appears to me cruel and unjustifiable; and I am satisfied from what I have seen, that by it the progress of the disease is painfully accelerated, the knife appearing to rouse it into fatal activity.

Of late years it would appear that in the hands of some surgeons the knife has frequently succeeded, and there seems to be rather an increasing confidence in its efficacy, and a disposition to employ it, under the belief that it will cure the disease. There are certainly on record a good many cases in which no relapse took place; and we find at the present moment women alive upon whom the operation was performed years ago. But after the unfortunate results I have detailed, the question naturally occurs, "Were these cases of genuine cancer?" We often meet with tumors in the female breast of a benign character, which so greatly resemble scirrhus even in their structure, as to render it difficult for us to classify them. Indeed, in the majority of mammary tumors which present themselves to our notice, the disease is not malignant at all, but consists of an indolent enlargement of the gland, of an adipose tumor, a deep-seated chronic abscess, a strumous tubercle, or of a hydatid cyst. In some of these cases even the most experienced may be deceived, and be led, from the disease not having returned, to assign more value and importance to the operation, than it is justly entitled to, and thus to take credit for cures which were never performed. When these mistakes, which are by no means uncommon, originate in ignorance, they may be pardoned, as we are all liable to them, but when the real nature of the disease is designedly concealed, and they are held out as cases of cancer cured by operation, we cannot but lament that there should be found in the profession an individual or individuals so devoid of honesty and candour as to attempt to mislead, in a matter of such serious importance.

It may appear to some that I have taken too unfavourable and gloomy a view of the results of operations for the cure of this formidable disease. If I have done so, it has not been done hurriedly or unadvisedly. The painful truth has been slowly and reluctantly forced upon me, by an uninterrupted series of unsuccessful cases, and I feel myself bound to state as my decided and conscientious opinion, that in no stage or form of the disease is an operation to be depended upon, either as a means of permanent cure, or as a palliative. On the contrary, I believe that while it never arrests, it almost uniformly accelerates the progress of the disease. Why should we, therefore, continue to recommend and practise it? Why subject our patients to the torture and risk of an operation which we believe to be not only useless but injurious? It would be better for our patients, and more creditable to surgery, were the operation altogether abolished; and the melancholy fact at once acknowledged by the profession, and made known to the public, that we cannot eradicate the disease by the knife; but thats till much may be done, and by milder means, to alleviate the painful symptoms which attend upon it.

By refusing to operate upon what is usually considered a favourable case, it may be said that we are depriving the patient of the only chance she has of recovery, and consigning her to a state of hopelessness and misery. It is surely better, by a candid statement of facts, to dissuade from an operation which we believe to be useless, than to hold out prospects of a cure which will never be realized. The operation is submitted to only under the belief that it will

eradicate the disease, and when it proves ineffectual, the mental as well as the bodily condition of the patient is worse than if it had never been performed. It is doubtless a painful duty to be obliged to confess our inability to afford the relief and assistance which is expected from us; but would it not be more discreditable to our professional character, were we, in the teeth of accumulated observation and daily experience, cruelly to persist in operations which have been found not only useless but pernicious? By persisting in such a practice, we are doing an act of gross injustice to our patients, without having the candour to forewarn them of the result; and by resting contented with the operation as a means of cure, which every day's experience tells us that it is not, we are retarding the improvement of our profession, and preventing the talents and energies of its members from being directed to other remedial sources, from which we may yet obtain means more effectual than those we now possess, for eradicating this formidable disease.

Glasgow, 1st May, 1838.

REMARKS ON MAMMARY ABSCESS.

[Concluded from p. 389.]

Bleeding is a remedy, the virtues of which are variously estimated by practitioners in mammary abscess. In robust females I would trust fully more to bleeding from the arm, followed up by local bleeding, than to any other remedies. In subjects where it may not be advisable to bleed generally, we have to trust mainly to leeching, followed by cold or tepid saturnine applications. If these means, along with tartar emetic, do not succeed in checking the inflammatory action, I see no harm in applying a linseed-meal poultice for a day or two, to promote the opening of the abscess, when the patient objects to have the abscess opened by the lancet, or when this may not be deemed advisable by the surgeon. But I fearlessly affirm that a vast amount of mischief is annually produced by the indiscriminate and reckless employment of poultices. Some inexperienced or bigoted practitioners may think me a little Quixotic in thus sweepingly denouncing poultices. But I hope to be able to shew that I speak advisedly, influenced only by the result of my own unbiassed observation. I hope, moreover, that these remarks will not prove to have been written without producing any good practical effect. The subject, I am aware, is trite and commonplace; but in making poultices, *inter alia*, the subject of a few observations, I have a respectable precedent. No less a personage than the late eminent Mr. Abernethy delivered, before the Royal College of Surgeons, London, a lecture on the best methods of making poultices, and on the advantages resulting from their use. I feel somewhat diffident in censuring where so great a master eulogized; but, convinced of the correctness of my views, I feel called upon to speak freely on a subject like this, where error involves suffering, and injurious treatment brings discredit upon our art.

Simple, soothing, and innocuous, as poultices appear to the majority both of professional and non-professional people, I am firmly convinced, from what I have observed in practice, that they do prove frequently very mischievous, and that not only by preventing practitioners from using more appropriate local applications, but also by a deleterious, relaxing, or debilitating effect which they themselves exercise upon the parts affected. Although I have more immediately in view at present the bad effects of poultices in mammary abscess, yet as repects several other surgical diseases my remarks will hold good. The abuse of poultices is wide spread; and it would prove an interesting and instructive, but tedious task to expose this abuse fully, and with judgment and ability. Perhaps some of our hospital surgeons, or surgeons in extensive practice, may ere long favour the profession with such a desirable *exposé*. If my remarks should succeed in eliciting such a document, I shall consider myself well rewarded for the trouble of throwing them together. I would not like to state the number of limbs I have seen destroyed—at least, brought into such a state, mainly by poultices, as to sink the powers of life, or to render amputation necessary, for injuries which, under a more scientific and less routine system of practice, would have done well. In phlegmonous erysipelas, in severe contusions, in peri-

ostitis, in compound fractures, I content myself with saying that I have seen much mischief result from a reckless system of poulticing, which too many surgeons are in the habit of pursuing. These surgeons, with a *Sangradoism* truly pitiable and deplorable, poultice—poultice—poultice—without ever once suspecting that by so doing they may occasionally be doing harm. But let me confine myself to mammary abscess.

The proper time for using poultices in this disease is very brief. In those acute cases where matter forms rapidly in spite of all our efforts, its exit, I believe, is favoured by the relaxing effects of an emollient poultice. For this object chiefly would I recommend the application of a poultice. As soon as the abscess has fairly given way, I conceive that poultices are no longer of use. Such I am well aware is not the general opinion of the profession. But there are some who never can see harm resulting from any of their own plans of treatment—who, in their routine system of practising, never dream of the possibility of their remedies proving, under certain circumstances, hurtful in place of being salutary. It is almost a hopeless task exposing to such individuals the impropriety or injurious tendency of generally adopted plans of treatment; but to less bigoted and more observant I do not despair of shewing the mischief which results from long-continued poulticing in mammary abscess. In this complaint, after one or more abscesses have formed and given way, we generally find the poultices continued by those practitioners who are not aware of their injurious tendency. The consequence of this mode of treatment is, the discharge from the abscesses continues undiminished. The whole breast not unfrequently becomes greatly enlarged, of a stony hardness, of a dull red colour, especially around the apertures of the abscesses. I have seen many breasts brought into this formidable-like state by long-continued poulticing. I have seen a breast swollen to three or four times its natural size, of great hardness, with the orifices of several abscesses discharging a little thick pus, dispersed over its surface, which was poulticed, and continued getting worse, till at length it was thought expedient to submit to have a bandage applied to the part, so as to give it firm support, whereby the hardness and swelling progressively diminished, and in no long time entirely disappeared, leaving the breast perfectly sound. In this case the poultices were continued, from the relief which the patient said she experienced from them. But the mischief they occasioned shews how apt the feelings of patients are to lead us astray, if other circumstances are not at the same time taken into consideration. Many females with mammary abscess express themselves tolerably easy under a frequent change of poultices; but I have again and again made myself sure that this supposed relief was merely deceptive, for the affected parts went on from bad to worse, and when the poultices were interdicted and a bandage applied, the relief experienced was very striking, and the patient's gratitude was generally expressed in glowing terms. I may here adduce a case in point. It is the more notable from its being the first which opened my eyes to the bad effects of long-continued poulticing, and to the advantages to be derived from proper bandaging of the breast.

Mrs. H——, shortly after the delivery of her second child, was seized with pain of right breast, accompanied by febrile excitement. Pain in the breast continued very severe, and ended, in the course of a week, in two different abscesses, notwithstanding the free application of leeches, purgatives, and tartar emetic. The poultices which had been applied before the abscesses gave way were continued, the patient saying that she experienced relief from a frequent change of them; but the breast, instead of improving, soon became more painful, and two or three new abscesses formed. The breast was very hard, and swollen to at least three times its natural size. The poultices having thus been continued for at least five weeks, with the occasional application of a few leeches to the affected parts and some internal remedies, and no good effected, I began to think that the poultices were retarding, in the place of conducing to, the cure of my patient. This idea having struck me, I told her that I thought a bandage, properly applied, might prove serviceable. To this application she, of course, readily consented. I therefore applied a roller, from eight to ten yards long, round her chest, in such a way as to give firm and equable support to the whole of the

affected breast, taking care to leave the orifices of the abscesses free. The night after the bandages was applied, she slept for four or five hours together—a thing she had not done since the commencement of her complaint. In little more than twelve days scarcely a vestige of her complaint remained; the swelling and hardness having rapidly subsided, and the abscesses dried up, under daily bandaging for the above period.

This case proved to me a very interesting and instructive one, and is just such a one as is to met with every day. One abscess formed after another, in tedious succession, in spite of all my efforts; nothing proving of any decided benefit till I bethought myself of bandaging. Had this plan of treatment not suggested itself to my mind, there is no saying how long the patient might have been harassed with her disease, and myself annoyed by reiterated failure. The probability is, that without it several sinuses would have formed, and these would have continued to annoy her till the usual but very severe remedy was resorted to, viz. laying them open with the knife.

In mammary abscess the hardness and swelling, consisting of an œdematous and lymphatic infiltration of the cellular tissue of the mammary gland, is caused in a great measure by poulticing. I have adopted this opinion from actual observation of many cases, and I trust practical men will pay some attention to my remarks, and observe for themselves whether I am borne out or not by the every-day occurrences of practice.

In place of swelling out this communication by a detail of cases (knowing how sadly abused things cases often are now-a-days, being much a-kin to the distorted facts of reckless theorists), I prefer stating summarily, as the result of my observation, that long-continued poulticing is especially injurious in mammary abscess, and the sooner it is given up the better; and that much benefit may be derived from bandaging, especially in those cases where there is a copious discharge of matter, with much swelling and hardness of the affected breast, without very acute pain.

It is well known that sinuses are frequent sequelæ of mammary abscesses, and I need scarcely say, if these also could be cured by bandaging, it would be a vast improvement on ordinary practice. It is a terrible operation to cut up deeply and extensively burrowing sinuses—an operation which would be willingly relinquished, were an equally successful and milder means of cure proposed. I anticipate considerable success in the cure of sinuses of the breast from bandaging; but my experience of this practice in this complaint is as yet very limited. One case, however, very lately was rapidly cured by this treatment. Let it be borne in mind, however, that bandages, if not applied with judgment and care, cannot be expected to prove so beneficial as we have found them. In mammary abscess, each and every part of the affected breast should be firmly and equally pressed, while an opening or openings are made in the bandage over the orifices of the abscesses. In mammary sinuses, again, the whole breast should be compressed, and we should endeavour to have the pressure applied so that the tract of the sinus shall be equally pressed upon, and due provision made for the egress of matter from it, by the bandage being removed from its orifice, or by an opening being made in the bandage at this part.

I am not aware that views similar to the above have yet been laid before the profession. I respectfully submit them to my brethren for approval or refutation. If correct, let them at once be adopted and acted upon; if erroneous, let them sink into merited neglect. Unless I am shewn to be mistaken on the topics discussed above, I may at some future period, Mr. Editor, solicit your indulgence for the insertion of another communication on this subject.

OBSERVATIONS
UPON
VENTILATION AND WARMTH;
COMPRISING STRICTURES ON DR. ARNOTT'S WORK UPON THESE SUBJECTS.

By Julius Jeffreys, Esq.

[Concluded from page 283.]

The reader is already informed of the causes which gave rise to the papers on ventilation, of which it is my purpose to make this the concluding one. Commenced as strictures upon the work of an influential writer on very important subjects, the task, for its proper com-

pletion, ought to comprise a critical examination of every question or plan therein contained, the right decision upon which could be of moment. I cannot so far desert my undertaking as to leave unnoticed the many such remaining points; but a protracted examination of their correctness I have been readily persuaded to drop. I do so with grateful relief to my own feelings; yielding to which, it were less painful to witness the progress of opinions and plans directly opposed to my strongest convictions, than by a lengthened discussion of them, to afford any ground upon which could be rested a suspicion in the minds of any readers, that unworthy feelings had a part in this criticism.

A reader whose attention should have been directed to the thermometer stove alone, might well consider that it could not afford matter for a long disquisition. The examination of *it*, however, formed but a small part of the duty of these papers. A large body of facts, opinions, and plans, connected with ventilation and warmth, has been long afloat. It was to be desired, for the public good, that these should be collected into a well-digested treatise by a qualified person. If such a work was much needed, equally necessary was the establishment of truth upon all important points therein discussed; and this in proportion to the talent and influence of the author of such a work.

Thinking and right-minded persons, who shall have afforded them an attentive perusal, cannot discover in these, my papers, any other than a sincere desire to establish that which is strictly true, upon questions of importance to every one. Bearing their importance in mind, and observing the manifest spread of certain opinions connected with them, they cannot look upon an opposition to these opinions as uncalled for, provided that opposition is well founded.

At the same time, where a commentator has to differ, on almost every essential point, from his author, the frequent renewal of strictural remarks may, perhaps, be as unpleasant to the feelings of the reader as it is to my own. Upon one point, the ventilation of rooms by keeping the windows open a little at top, it is necessary to remark at some length, in completion of one section of my task. The other chief points upon which my own observation does not conform with Dr. Arnott's, I shall do little more than enumerate.

In my last paper it was demonstrated, to the satisfaction, I trust, of every reader, that an apartment having the doors and windows shut as is usual and proper, cannot ever be in a wholesome state as to ventilation, in which, instead of an open fire and chimney, there is a close stove and closed chimney; that the air which enters such a room cannot exceed a small fraction of what the open chimney insures, and vigilantly insures, without any attention on the part of the inmates being needed. In practice, therefore, it becomes absolutely necessary, in the case of the close stove and closed chimney, to open the windows a little at top; and although Dr. Arnott would appear, from Art. 90 of his work, to consider this unnecessary in a cold winter day (requiring thereby the inmates to accustom themselves to a most scanty supply of air), in Art. 91 he does himself recommend, when the weather is not so cold, or when the company is more numerous, that the windows should be drawn down a little at top. Here the reader can hardly fail to notice the remarkable fact, that Dr. Arnott thus becomes the only writer in favour of windows kept open at all, in any but summer weather, and is therefore himself the only person to whom his remarks, denouncing open windows, in Art. 82, can apply; so that any one who should go about our cities during the winter months, looking up at all houses to discover who were the imprudent persons with open windows against which he declaims, would find them only at those houses which were warmed and ventilated according to his own directions.

While a sufficient ventilation is at all times vigilantly enforced by an open fire, when the chimney is closed, it can only be effected by watchful attention on the part of the inmates of an apartment. Granting to them such a perception of every degree of impurity in the air as would be required, but which no one possesses, and allowing to them the vigilance necessary, neither the acute perception of their senses, nor their vigilance, would avail them, while their ventilating channel, which is in fact that portion of the room above the stove, was made to open at the side of the

house—that is, to have vent at the tops of the windows, drawn open for the purpose.

I have already remarked how admirable is the plan of causing our chimneys, considered only as ventilating channels, to open outwards above all walls; but here we have a very feeble current venturing itself out sideways, although a breeze that would move a feather, setting on that side of a house, would not only stop all air from coming out, but would blow cold air in, in quantities defeating all the intentions of the inmates; and in the case of a sleeping-room, requiring one person at least to keep awake, lest a change of wind to that side of the house on which the air was going out at the top of the windows should, by blowing air in, reverse the current of ventilation. That which was a very moderate, nay scanty, ventilation in the evening, when the wind was upon the other side of the house, would be converted into a dangerous influx of wind, and perhaps drift even of snow, upon the very beds of the inmates. Let no one suppose this to be an overstrained, or even improbable case. Any opening of windows, left wide enough to maintain ventilation at all sufficient, when the wind was from the other side of the house, would, upon a change of wind, let in gusts and drifts dangerous in the extreme: and that such a room would be in a very unwholesome state without such opening, that is, with the windows closed, the reader, whose opinion has accorded with the arguments contained in my former papers, will himself affirm. There are few persons, indeed, who, from their own feelings, do not object to the closing up of a bed-room chimney by a chimney board. We have, in these cases, a deficiency actually become manifest to the senses. Now there is no fact connected with the animal economy more true, and none which more deserves to be an aphorism in medicine, than that *ventilation grows far short of a salubrious abundance long before the deficiency is troublesome to, or even felt, by the occupiers of an apartment.* The practice of closing up the chimney is by no means wholesome nor usual, even in a cold room; but with a close stove and the windows shut, it would be highly deleterious, however gradual and insidious its effects; for, as already demonstrated, it is beyond all question true, that without an open chimney such a stove has no power of effecting ventilation through the usual crevices worthy of any comparison with that which real salubrity demands. The persons, therefore, who employ a close stove of any kind, with a closed chimney in their bed-rooms, must either incur great risk from leaving their doors or windows more or less open, or they must accustom themselves to a ventilation imperfect in a very injurious degree.

I proceed now to enumerate the chief remaining points upon which I am compelled to differ from the author of the treatise before us, and which I consider of too much importance to be left unnoticed.

Economy of fuel. Every close stove is, if properly managed, necessarily less consumptive of fuel than an open fire; but if the chimney be closed, no small part of the economy will be of the character of parsimony; for such a stove has not a tenth, often not a twentieth part, as much fresh air to heat as has the open fire. Hot water, although Dr. Arnott supposes the contrary, may be so employed as to effect as great, or even a greater economy of fuel, than any air stove; and the water may be heated in a few minutes.

It is questionable if any self-acting regulator is superior to a hand regulator of the draught. Unless the warmth of the room itself, and not of the stove, is made to regulate the fire, adjustments by the hand must be and are made to maintain a steady temperature in the room. A solid metallic regulator may be shown to be superior in every way to one of air, if properly constructed.

The principle which causes the thermometer stove itself to differ from the more usual kinds, will not at all prevent the over-heating of the air. This very stove was employed by Messrs. Strutt, of Derby, many years ago, and subsequently about London; but, owing to inattention to the draught, was found to overheat the air. The stove, however, with the commanding of the draught, as judiciously attended to by Dr. Arnott, is well adapted for halls and open courts, but not, I think, for any dwelling apartments.

Just in proportion as any stove is well fitted for domestic purposes, so is it ill-fitted for almost every purpose in the arts. With such purposes experience has given me some acquaintance, and

enables me to affirm, that for none of them, as supposed by Dr. Arnott, is the thermometer stove at all adapted.

Count Rumford ingeniously proposed to recover waste heat from the smoke of chimneys by one or more pipes in them, in which fresh air was brought down from the summit by a counter current. This was exactly the double current process of Dr. Arnott; and with equal judgment have other persons employed this principle. Dr. Arnott proposes, in several parts of his Treatise, to recover the warmth of impure air passing out of rooms by a similar apparatus. The proposal is ingenious, but would be utterly irreducible to any useful effect. So far from costing little, any apparatus to be effective, where the difference of temperature of the passing currents at any one point ought not to be five degrees, must be very elaborate and expensive, and the heat saved by it not worthy of any consideration.

Of blowing machines for ventilating purposes I have used several. Having set aside the box-pump long in use, and recommended by Dr. Arnott for one in which the piston was a pendulum, with practically no friction, and in which the valvular apertures admitted of being very free, I found the effect of the latter to be in every respect superior to the pump, while its action was as silent as that of any valvular apparatus can be made. But after an abundant comparative trial of this instrument with ventilating fan-wheels, candour compelled me to give the preference to the latter, when properly constructed. Dr. Arnott's general condemnation of them would not, I think, have been made, had he seen this ingenious invention of Desaguillier's properly employed. As to obliquity of action, there is no necessary loss of effect from it; of which the common vertical windmill is a remarkable proof, surpassing, as it does in a prodigious degree, any in which the more direct action of the wind is employed. My own observation and experience have led me to consider the fan-wheel to be much the best instrument for ventilating purposes where the issuing current of air is unobstructed; but where it is obstructed, as in a small nozzle for blowing a furnace, the usual cylindrical blast-pump is doubtless a more proper instrument, the operations to be performed in the two cases being very different.

The modification of the common open fire, in which the fresh fuel was forced up at the bottom of the fire instead of being thrown on at the top, was an ingenious contrivance of Mr. Cutler's. Its object was to insure the entire combustion of the fuel, by causing the bituminous and smoky current to pass up through the bright cinders, and thus be consumed. But no such open fire will burn well without a current from below, nor will the ashes be discharged readily without the usual grating below the fire. For these and other reasons this fire-grate has ceased almost to be known. Cutler provided a quick horizontal draught through the fire, to make up for the absence of the usual current from below; but in the "coal-torch," or modification of Cutler's fire-grate, proposed by Dr. Arnott, no coals would burn, nor any but the most inflammable matter; and so far from being smokeless, such a fire would throw off the densest fumes, and would radiate very little heat. Encumbered in an especial degree with all the faults chargeable to common open fires, it would present to the beholder a less attractive and cheering aspect than any of them. A barrel of pitch even, inflammable as it is, if set a-light at top, gives out with the flame volumes of smoke, unless a current of wind blows powerfully against it; and it is well known that a box of coal would not burn at all lighted at top. This, indeed, is a quality so providentially given to our fuel, as to be well worthy of notice in a "Bridgwater Treatise," otherwise dangerous fires in the coal of cellars, wharfs, and ships, would be daily breaking out; whereas, so nicely is its combustibility determined, that we lay it up daily in large heaps about our cities in full security, and are unmindful even of the beneficent provision in its structure, which guards us against the fear of its thus burning at the surface.

Finally, with regard to open fires. Not only is the radiation of warmth from them a source of such comfort that they never will be laid aside in England, but in our climate, where we have so much less of unclouded solar light than in most others, it is highly probable that there is an instinctive desire in the people of this country for an open fire, since its light, though proved by experimental researches to differ in some respects from that of the sun, may exercise all the same beneficial influence over the ani-

mal system which has been long proved of solar light; the benefit of the one differing from that of the other in degree only. Hence how indescribably cheering is the winter fire, radiating health-giving light, as well as warmth!

In conclusion, having proved, by arguments founded upon established principles in medical and general physics, that the chimneys in our dwelling-rooms cannot be closed without a very injurious exclusion of fresh air, or an introduction of it in a manner which never could be safely practised, it remains for me to remark upon the fact, that close stoves, some of them even discharging the gas of the combustion into the room, are so employed in this country, while they are common abroad*; and that persons so employing them are often unobservant of their injurious effects. While in one part of Europe close stoves and chimneyless rooms are usual, and in another part, charcoal fires voiding their products into the house, in England, favoured with an abundance of fuel, we had happily long followed a course, defective indeed, but greatly more salubrious; a point settled by frequent discussion, and by the experience of the whole nation for ages past. Lately, however, both the former have been brought before the notice of the public, with every argument to recommend their adoption. Though the one is decidedly much more deleterious than than the other, it is earnestly to be hoped that the people of England will fall in with no plans which diminish their supply of fresh air, or which deprive them altogether of open fires. Each of those plans has, indeed, many a person to speak in favour of it from trial; but against any specious evidence of this kind well-disciplined minds will be on their guard, particularly those of medical philosophers, not under any bias of fashion or of prejudice. They well know that by precisely similar arguments are upheld nearly all the injurious practices by which human health is undermined, and that the most destructive of them being oftentimes slow and insidious in their action, have always advocates with a fair show of cases in their favour. In well-ordered minds, deductions from general experience, supported by sound principles in science, will never be allowed to give place to partial experience, opposed, as in the present cases, to the most substantial truths in natural and medical philosophy*.

* Their being found to answer in colder climates, and not in our own, has been explained in a former paper.

THE EPIDEMIC FEVER.

To the Editor of the Medical Gazette.

Sir,

The following remarks upon the fever now prevailing in this metropolis, are respectfully offered to your notice, with the hope of drawing the attention of your more experienced correspondents to the subject.

In general the invasion is sudden, being marked by rigors and extreme prostration of strength, and attended with severe and distressing headache, particularly about the forehead and temples. After the lapse of a few hours, delirium supervenes, greatly resembling that of delirium tremens, the patients muttering some incoherency, generally applicable to their usual employments, but from which they can always be roused by drawing their attention to surrounding objects. The sleep is much disturbed and unrefreshing; there is a disposition, even in the early stages of the disease, to pick the bed-clothes, and every muscular effort is accompanied by a tremulous motion of the limbs. The tongue remains clean throughout the complaint, and is generally moist on its edges, and white in the centre. The thirst is not distressing, the anorexia complete, the skin is moist and perspirable; the countenance anxious and febrile. Pulse soft, frequent; becoming, very early in the disease, weak and compressible.

The symptoms which seem to me worthy of especial notice in this epidemic are those which have the appearance of being strikingly incongruous in the history, viz., the state of the tongue and skin, and the nature of the delirium.

The severity of the headache, the prostration of strength, the expression of the countenance, at once decide the practitioner to assume that he is treating

* It is necessary to notice particularly an erratum in my last paper, the press not having been corrected by myself, Medical Gazette, May 12, p. 280, line 7, for "the atmosphere we know to be ever active in endeavouring to press upwards a body of air *heavier* than itself," read "a body of air *lighter* than itself."

a fever of the typhoid kind; but the tongue, instead of being parched and loaded with sordes, remains clean and moist; nor has the skin, in any of the cases which I have witnessed, possessed that dry pungent heat which characterizes typhus, but, on the contrary, has continued moist throughout the disease.

The delirium also differs very materially from that which commonly attends typhus, or indeed continued fever of any description; and even where the disease has terminated fatally, I have not observed the patient sink into a completely comatose state, but he can be roused to answer questions coherently until within a short period of his death.

Notwithstanding the comparative mildness of some of the symptoms, this fever cannot but be regarded as a most formidable disease; but as it was my only intention to remark upon its peculiarities, I shall merely say of the treatment, that the patients do not bear depletion, and I can recollect no recovery where this treatment was energetically employed.

In the progress of this epidemic, I have more than once had reason to regret the influence which preconceived opinions as to the theory of fever have had on the conduct of the treatment. The notion that the headache was the consequence of cerebral inflammation neeessarily suggested the employment of bleeding and mercurials, where the opposite plan of treatment was, I think, the only one to be safely pursued.

When we see the headache continue during the progress of the disease, unmitigated by the employment of depleting remedies, whilst it gradually disappears with the other symptoms, leaving no trace of eerebral mischief, in cases where depletion had not been resorted to, are we not justified in concluding that the eerebral affection was uneonneeted with inflammation, but with functional disorder merely, which, whatever may be its real pathology, does not give rise (at least immediately) to any disorganizing process?

I am, sir,
Your obedient servant,
LONDINENSIS.

London, 21st May, 1838.

[Our own experienee would not have led us to describe the tongue as "clean," though certainly it is much more "moist" than in the form of fever usually prevalent in London. But an important circumstance, in addition to those enumerated by Londinensis, is the appearance, in a large proportion of cases, of a rubeolous rash, totally different from the petechial eruption so common in low typhoid fevers. We believe the most successful treatment to consist in the guarded use of stimulants.—ED. GAZ.]

MEDICAL GAZETTE.

Saturday, June 2, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

THE NEW FRENCH PHARMACOPŒIA.

THE French Codex, like our own Pharmacopœia, has not escaped criticism; and whether we suppose the Aristarchs on both sides of the water to have been actuated by mere zeal for the public welfare, or in some eases to have been egged on by less laudable motives, still the community has been the gainer. In some few cases, the angry emendators have been in the right; in others, the advantage consists in the attention of practitioners having been directed to some remarkable formula, or some considerable alteration of strength in a well-known medicine.

M. Béral has pointed out what certainly looks, at first sight, like an inconsistency, in the receipt for the *Potion gazeuse*, or effervescing draught. The framers of the Codex first order the potion to be composed of an ounce of syrup of lemon-juice, half an ounce of lemon-juice, three ounces of water, and half a drachm of bicarbonate of potash. These ingredients are to be mixed in a phial, which is to be instantly corked. So far, so good. They then add, that it is often useful to make the patient take the alkali and the acid separately, so that the effervescence may take place in

the stomach. The alkaline part is then to consist of half a drachm of bicarbonate of potash, and half an ounce of syrup of lemon-peel; the acid mixture, of half an ounce of lemon-juice, an ounce of syrup of lemon-juice, and two ounces of common water. The patient is now to take equal parts of each of the mixtures; and consequently, as M. Béral observes, if the patient takes the first mixture, and an equal quantity of the second, he will have swallowed the whole of the potash, and only one-sixth of the acid required to saturate it.

In the Codex of 1818, it is directed that when the alkali and acid are taken separately, the alkaline mixture is to be swallowed in two portions, and two drachms of lemon-juice after each. Thus there will still be a deficiency of acid by the omission of the syrup of lemon-juice (one-third of which is juice), though far less than in the present Codex. Very possibly the authors found, that when the acid was nearly sufficient for the saturation of the alkali, the effervescence in the stomach was too violent; and that in cases where this kind of effervescence was desirable, a large excess of alkali was useful.

M. Béral also attacks the *Apozème vermifuge*, or decoction of pomegranate-root bark. He thinks that people have a right to be surprised that the Codex has passed over the observations of M. Dublanc the younger, from which it appears that much more extract is obtained from this bark by successive infusions with cold water than by decoction. In the London Pharmacopœia not only is boiling ordered, in defiance of the experiments of M. Dublanc the younger, but, to the great scandal of many well-meaning critics, the bark of the fruit is used instead of the bark of the root. For our own parts, we should be glad to know whether the former is not nearly as good as the latter, and whether the roots are procurable in sufficient quantities to drive out one-fiftieth part of the tape-worms of Europe.

A reviewer in the *Gazette des Hôpitaux*, after reporting these remarks of M. Béral, sets forth some still more delicate doubts of his own. He wants to know whether it is not absurd to prescribe certain remedies, such as digitalis, in the form of an alcoholic tincture, not reflecting, he says, that alcohol entirely destroys its contra-stimulating power. *Distinguendum est*, as the logicians say; alcohol, no doubt, will lessen or destroy the lowering power of foxglove, when exhibited in the form of two or three stiff beakers of punch; but twenty drops of spirit will not balance the quantity of digitalis which they contain. The French tincture, too, contains twice as much digitalis as the London one, so that twenty drops would be a pretty large dose*; and therefore the answer to this objection is, *de minimis non curat Prætor*—practical men do not weigh hairs in cobweb scales. The reviewer makes a similar objection to the tincture of cantharides. He affirms that the blister-fly, though an irritant to the skin and the urinary passages, depresses the system and lowers the pulse like digitalis; so that when given in the form of tincture, its action is in part neutralized. If this account of the action of cantharides is correct, we must reply as before, *de minimis*, &c.; the quantity of spirit is infinitely too small to counteract the sedative operation of the remedy. Surely these objections belong to the very pedantry of pharmacy!

Let us now take another comparative view of the Parisian and London Pharmacopœiæ; and let us suppose an English practitioner to look into the French Codex for preparations of opium and

* There is another tincture made with equal quantities of the fresh leaves and of alcohol. This is to be dispensed only when specially prescribed.

mercury, what will he find? Of the former there is an extract, another extract deprived of narcotine, and a third made with wine; an acetic tincture, a tincture of the extract, an ammoniated tincture, two wines, powdered opium, Dover's powder, a pill, two syrups, an opiated collyrium, an opiated liniment, together with the Theriaca and the Diascordium, each containing a grain of crude opium, or half a grain of extract of opium, in a drachm*. Then of the salts of opium we have morphia, with its acetate, muriate, and sulphate, and codeine; as well as syrups of the acetate and the sulphate, each containing a quarter of a grain of the salt in an ounce of syrup. The extract of opium deprived of its narcotine is supposed to be sedative without being previously stimulant. Magendie says he has used it with advantage, particularly in the case of a young Greek physician, of high promise, with whom the common aqueous extract did not agree†.

The tincture of opium is considerably stronger than the one in the London Pharmacopœia, as it is made with one pound of the *extract* to twelve pounds of alcohol.

Of the two wines, one is called *Vin d'Opium composé*, or *Laudanum liquide de Sydenham*, and contains saffron, cinnamon, and cloves, in addition to the opium, the menstruum used being Malaga. Twenty drops are equal to a grain of the gummy extract of opium. This represents our *Vinum Opii*, but is stronger. The other wine, called *Vin d'Opium obtenu par la fermentation*, or *Opium ou Laudanum de Rousseau*, is obtained by mixing opium, honey, water, and yeast, and allowing them to ferment for a month, or more. The liquid or wine resulting from this process is very viscid, so that the drops are large, and twenty of them are equal to about gr. iiss. of extract of opium.

The *pilules de Cynoglosse*, or *pilulæ cum Cynoglosso*, are composed of the bark of the root of Cynoglossum officinale (houndstongue), hyoscyamus seeds, the aqueous extract of opium, myrrh, olibanum, saffron, castor, and syrup of opium. These pills contain 1-8th of their weight of extract of opium. The collyrium contains a grain of extract of opium to an ounce of rose-water. The *Linimentum narcoticum* is a compound of the *Baume tranquille* and Sydenham's laudanum, in the proportion of eight parts of the former to one of the latter; and the tranquillizing balsam itself is a most elaborate compound of olive oil and eighteen plants, of which six are narcotic, and twelve aromatic or odoriferous. If to these we add the various preparations of poppies, it must be acknowledged that there is no lack of opiates.

Nor is the catalogue of mercurials a scanty one. We find the red oxide, or peroxide, the black sulphuret, the cyanuret, the sub-deuto-sulphate, the proto- and the deuto-ioduret, the proto-nitrate, the ammoniacal proto-nitrate, a solution of the nitrate, the proto-acetate, the proto-tartrate, calomel prepared in three different ways, corrosive sublimate, and a solution of it, a soluble hydrochlorate of mercury and ammonia, the white precipitate, powdered calomel, powdered corrosive sublimate, two mercurial ointments, two others containing corrosive sublimate, and another made with the red oxide of mercury and acetate of lead, the citrine ointment *(pommade citrine)* made with the nitrate of mercury; two other ointments, one called *digestif mercuriel*, the other *onguent brun;* two plasters, and two *trochisques escharotiques.*

The stronger mercurial ointment is made with equal quantities of mercury

* There is a preparation called *Electuaire de quinquina, opiat febrifuge*, or *Opiata febrifuga*, which does not contain any opium.

† Formulaire, 8e Edit. p. 76.

and lard, and is therefore of the same strength as the London ointment; but the *pommade mercurielle simple*, or *onguent gris*, is weaker than our Ung. hydrargyri mitius in the proportion of 5 to 8, being made with four ounces of the strong ointment to twelve of lard.

Of the two ointments containing corrosive sublimate, the *pommade de Cirillo* is made with a drachm of the salt to an ounce of lard; the other, called the *pommade de Desault*, has only twelve grains to an ounce of *pommade rosat*, but contains the red oxide, and other ingredients.

The solution of corrosive sublimate *(liqueur de Van Swieten)* is made with eighteen grains of the salt to twenty-nine ounces of water and three of alcohol; hence, say the authors, this solution contains $\frac{1}{1000}$th part of its weight of the sublimate, while the receipts in other Pharmacopœiæ contain only $\frac{1}{1152}$; and therefore the French solution is stronger by about one-tenth.

In our last article on this subject, we directed the attention of our readers to the copiousness of the Codex in the matter of syrups, as indicating the luxury of our neighbours; the list of carbonated waters is another proof of the same kind. While our Pharmacopœia contains but two, the Codex offers eleven to the patient who wishes to get well agreeably; the *eau gazeuse simple*, *eau de Seltz artificielle*, *eau alkaline gazeuse*, *eau de soude carbonatée (soda water)*, *eau de Vichy artificielle*, *eau de Mont-Dore artificielle*, *eau de Bourbonne*, *eau magnésienne*, *eau magnésienne gazeuse*, *eau de Sedlitz artificielle*, and *eau de Spa artificielle*—all invite his attention, and promise relief to the most queasy stomach.

The first on the list contains nothing but carbonic acid gas; but the framers of the work observe that by putting two ounces of syrup of lemon-juice into each bottle before it is filled with the carbonated water, a very agreeable beverage is obtained, known under the name of effervescing lemonade *(limonade gazeuse)*; and that by varying the syrup, a great number of acidulous and sweet drinks may be thus prepared.

In spite, however, of the advance in luxury, which eleven carbonated waters would seem to indicate, we are inclined to think that the consumption of soda-water alone in England far exceeds that of all the carbonated waters put together in France; and this suspicion is strengthened by the direction to pour the *eau gazeuse simple* into twenty-ounce bottles—a dose too large to be often taken.

Our gastritic readers will thank us for mentioning that the *eau de Seltz artificielle* is frequently to be met with in continental hotels; and though we would not pledge ourselves that it always contains, as the Codex requires, chloride of calcium, chloride of magnesium, chloride of sodium, carbonate of soda, phosphate of soda, and sulphate of soda; yet we can state from our own experience, that it is always most plentifully charged with carbonic acid. The real Seltzer water contains much less. Whether the immense demand for carbonated beverages in this country depends on the growth of luxury, or on the increase of diseased stomachs, we are unable to determine; but certain it is, that every class of society enjoys these sparkling fluids; and from patrician champagne down to plebeian ginger-beer, they are prepared to suit every taste and every pocket. Thanks, too, to the industry of manufacturers, champagne, or, at least, its name, is no longer confined to the opulent, and wines may be seen, in Anacreontic language, to *sparkle* on almost every board. Dr. Granville, in his amusing and instructive account of the Spas of Germany, has told us how

this branch of industry is carried on in the district of Franzensbad, in Bohemia, under the auspices of Mr. Hecht. A large quantity of light Hungarian wine, both pink and white, "stale, flat, and unprofitable," is to be had at Egra for asking.

"The *Kalte Sprudel*, supplying out of its spring five thousand four hundred and seventy-two cubic inches of pure and free carbonic acid gas per minute, lies close at hand. The wine, with the addition of a little sugar, being put into a twelve-gallon cask, placed on the machine invented by Mr. Hecht; and the gas being conducted through pipes from the said Sprudel to a forcing pump connected with that machine,—five atmospheres of the gas are forced into the juice of the Hungarian grape, and the turbulent mixture is let out into bottles, without contact with the atmosphere, and corked. They are wired at one and the same time; and afterwards deposited in another part of the premises, there to wait for the finishing stroke—the putting on of the *silvery cap*—which, with so many quaffers of champagne, is the *sine quâ non* mark of its being a genuine wine *!"

If it be a test of civilization, as we are sometimes told, that it makes good things cheap, we are surely advancing very fast, when champagne, formerly the envied privilege of the wealthy few, now makes its way into half the houses in London—thanks to the fertile genius of Mr. Hecht!

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

May 22, 1838.

Dr. Merriman in the Chair.

On a successful Plan of arresting the Destruction of the Transparent Cornea from Acute Purulent Inflammation of the Conjunctiva. By Frederick Tyrrell, Surgeon to St. Thomas's Hospital, and to the Royal London Ophthalmic Hospital.

The author having often had occasion to witness the insufficiency of the means commonly resorted to in the treatment of acute purulent inflammation of the conjunctiva to arrest the sloughing process in the transparent cornea, was induced to study the disease very attentively in the hope that a knowledge of the mode in which the morbid change takes place might suggest some adequate means of controlling it. In this hope he was not disappointed, having devised a remedy, the success of which has been sufficient, in his opinion, to warrant him in offering it to the profession. The cornea being, in the author's opinion, almost altogether dependent for its supply of blood upon the conjunctival membrane extended over it, he conceives it to be demonstrated that, in the high degree of chemosis attending upon acute purulent inflammation of the conjunctiva, its supply of blood must be cut off by the mechanical strangulation of of its vessels; from which condition, sloughing of the whole or of a part of the cornea (according to the degree of strangulation) must necessarily result. The plan of treatment, therefore, recommended by the author, consists in dividing the fold of conjunctival membrane, which, by its reflection, constitutes the chemosis, in order, by relieving the distension of its vessels to diminish the degree of chemosis. The novelty of the plan consists not merely in the division of the conjunctiva, which has been long practised by others without the least benefit, but by dividing it in a *radiated* manner, from the centre of the cornea towards the sclerotic margin in the intervals between the insertions of the recti muscles, whereby the large trunks of the vessels supplying the conjunctiva are avoided. The method hitherto adopted by many surgeons of dividing in a circular direction, parallel to the margin of the cornea, not only produced no advantage but was even prejudicial by cutting off more perfectly than before the vascular supply from the cornea. Several cases are related in which this practice was adopted in persons presented to the author, some in a very advanced stage of inflammation, and after sloughing of the cornea had actually commenced, in which the morbid processes were immediately arrested by its adoption; and the paper concludes by claiming for it the attention of the profession on the following grounds:—That it is safe and easy in its performance; that it is more efficacious than any other plan hitherto proposed; and that it prevents the necessity of active depletion, or the adoption of any severe general or local measures likely to injure the general health, or to produce severe suffering.

Read also, *Observations on Nævi, their Structure and Treatment.* By T. B. Curling,

* On the Spas of Germany, vol. ii. p. 174.

Esq., Assistant-Surgeon to the London Hospital, &c.

The object of the author in this paper was to point out the advantage of the practice of treating nævi by passing setons through their substance, in the manner first proposed by Mr. Fawdington, and afterwards recommended by Mr. Macilwain, in a case published in the 18th volume of the Society's Transactions. He relates several cases in which the practice had been pursued by him, with the effect of entirely removing the disease, without leaving any scar or other trace behind. The setons are passed with a common sewing needle, carrying a doubled silk, and are retained in the tumor until sufficient inflammation shall have been excited to cause obliteration of the vessels, after which the tumor soon shrinks and disappears. The practical part of the paper is preceded by much interesting speculation on the structure of nævi, which our limits do not permit us to enter into at length.

After the conclusion of the business the chairman reminded the Fellows, that the present meeting was the last of the session, and that the Society would hold its next meeting on the 13th November.

ST. ANDREW'S DEGREES.

To the Editor of the Medical Gazette.

Sir,

I think you will oblige an old correspondent by inserting the following remarks on a communication in a late number, on the reputed character of Erlangen degrees. I shall not occupy your valuable pages by entering into the value of the observation, that degrees obtained without examination are more honourable to the possessors, than those which require that proof of professional competency. As to the statement that a candidate being able to write a Latin thesis is a proof of medical proficiency, nothing can be more absurd. Still, however, as the degrees of the German universities are possessed by so many excellent members of our profession, it would be erroneous and unjust to object to them on the score of the want of proficiency of those bearing them. Your correspondent seems to treat Scottish degrees very slightingly, which is far from being justifiable, when we recollect that the majority of those British physicians who have most distinguished themselves in their profession and science generally, have been Scottish graduates. Speaking of St. Andrew's, your correspondent states that this University has not yet recovered from its "reckless" plan of granting degrees. In justice, therefore, sir, to the medical graduates of that university (of which I am an unworthy member), I beg to send you the following extract from the last curriculum issued by that body:—

"The candidate, if not possessing the degree of A.M., must produce certificates of his having had a liberal and classical education, and be ready to undergo an examination as to his proficiency in the Latin language. He must produce certificates that he has regularly attended lectures delivered by Professors in some University, or by resident Fellows of the Colleges of Physicians or Surgeons of London, Edinburgh, Glasgow, Aberdeen, or Dublin, for at least four *complete* sessions during four years, on anatomy, practical anatomy, chemistry, pathology, materia medica, practice of medicine, surgery—each of these courses being of six months' duration. Practical chemistry and midwifery, of which three months' courses are admitted; attendance at a public hospital for at least twelve months, and six months' attendance on clinical medicine. The examination is not limited to medicine; it includes all the branches of professional education, but especially anatomy, chemistry, medicine, *surgery*, and midwifery; and is conducted before the Court of Examiners, who are selected from among the most distinguished physicians and surgeons in Scotland, at two periods in every year."

On no pretext, sir, are medical degrees awarded at St. Andrew's, except in the manner above mentioned, even when (as in my case) the candidate previously possessed the diploma of the College of Surgeons or Apothecaries' Hall. These remarks, sir, will, I trust, be sufficient to remove the stigma which the observations of your correspondent would tend to cast on those who have graduated at St. Andrew's since 1833 (the date of the last curriculum); and when among the examiners we find the names of Lizars, the celebrated anatomist, and Dr. A. Buchanan, well known by his papers lately published in the Medical Gazette, no one need blush for the respectability of a degree obtained after such an examination—an examination, I venture to assert, inferior to none in extent, and superior to many in Great Britain.—I remain, sir,

Your obedient servant
and old correspondent,
M. D. Andreapolis.

May 21, 1838.

CASE OF TRIPLETS AND OF LOCKED HEADS.

A CASE of this kind occurred in the practice of Dr. Joseph A. Eve, of Augusta, on the 24th of September, 1837.

The woman was a delicate negress, aged about 35 or 40 years. Her health had been bad during the whole period of gestation, and particularly about the time of parturition.

The first birth was very easy and rapid; the child having passed before the doctor's arrival. He found the woman on her knees on the floor, leaning upon a chair, and the child suspended by the cord. As soon as he had made the ligature on and cut the cord, she was put to bed, and he found, upon examination, the feet of another child presenting. The labour progressed with the second child in this presentation until the body had passed as far as the arm-pits, when, in consequence of the pains becoming weak, and the fear of strangulation of the cord, the ergot was administered, with the effect of increasing the force of the pains. The next phenomenon worthy of remark was the indication of undue pressure on the brain of the second child, by convulsive contractions of its legs. At the same time the woman complained of severe pain and numbness in her right leg—the same side at which the head of the upper child presented. A further examination was then instituted, to discover the cause of compression, and of the arrest; for the pelvis was unusually large, and the child rather small, though not much below the average size. On this examination the doctor discovered the head of a third child below the superior strait; whilst the head of the second, whose body was delivered, was still above *the same strait*, constituting a case of locked heads. His first attempt was to dislodge the head of the third child; but this was soon found impracticable; for it was immoveably fixed below the superior strait. Not approving the plan, adopted by some, of delivering the upper child by the forceps, before delivering the head of the lower, he determined to await the delivery of both together, as long as he might think it safe to the mother, and, if necessary, ultimately to decapitate the lower child, press the head up from the superior strait, and thus allow the upper one to pass, or assist it with forceps, as circumstances might demand. Whilst awaiting the issue of this plan, he requested a consultation; but before the arrival of another physician, and within a little more than an hour after the discovery of the true nature of the difficulty, both heads passed. The superior child made some spasmodic movements after birth, but could not be resuscitated. Both heads were very much indented by the pressure of the other.

Except the injury inflicted by the accident, the children were all well formed, and very little below ordinary size. Two of them were boys. The mother passed her accouchement as well as could be expected under the circumstance of her previous wretched health.

Many cases of difficulty and perplexity in child-bearing arise from the small dimensions of the pelvis; but this was one which may be fairly attributed to too large a pelvis; for had this been of ordinary capacity, the head of the third child could not, with the good developements of both, have engaged the superior strait, with the neck of the previous child engaged in it, and the head at or near the superior plane. —*Amer. Journal*, Feb. 1838.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, May 24, 1838.

Francis Bancombe Pring, Flintshire.—John Francis Woody, Tamworth.—John Cooper, Reading.—James John Louis Donnet, Gibraltar.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *May* 29, 1838.

Abscess	3	Hernia	1
Age and Debility	30	Hooping Cough	4
Apoplexy	7	Inflammation	10
Asthma	10	Bowels & Stomach	1
Childbirth	7	Brain	4
Consumption	37	Lungs and Pleura	7
Convulsions	20	Insanity	3
Croup	1	Liver, diseased	2
Dropsy	8	Paralysis	1
Dropsy in the Brain	3	Small-pox	18
Erysipelas	1	Tumor	1
Fever	10	Unknown Causes	61
Fever, Scarlet	6		
Fever, Typhus	5	Casualties	8
Heart, diseased	2		

Increase of Burials, as compared with the preceding week . . . 3

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

May.	THERMOMETER.		BAROMETER.	
Thursday . 24	from 45	to 57	29·81	to 29·90
Friday. . . 25	44	63	29·94	Stat.
Saturday . 26	43	60	29 99	30·01
Sunday . . 27	35	59	29·95	29·75
Monday . . 28	43	63	29·67	29·55
Tuesday . 29	40	66	29·59	29 65
Wednesday 30	49	68	29·71	29·77

Prevailing wind N.E.

Generally clear, except the 24th and 28th, when rain fell.

Rain fallen, ·225 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

FINAL AND TOTAL DISPROOF OF THE CREDIBILITY OF DR. ALCOCK'S ASSERTIONS.

By Dr. Charles Orpen.

To the Editor of the London Medical Gazette.

Sir,

The abusive parts of Dr. A.'s last (p. 297*—305*), as to myself or others, I shall certainly not condescend to answer equivalently, nor even waste time in noticing by quotation. Almost all his oft-repeated misassertions, too, have been already sufficiently disproved, by previous decisive refutations. I shall, therefore, now only readvert to a few, replying finally and conclusively, thus:—

He reiterates, in a new form, his inventive assertion, that "the Licentiates have actually protested against the measure" of the College and its Committee, as to the Pharmacy question.

The "Licentiates of the Royal College of Surgeons of Ireland," "residing in Dublin and its vicinity," "at a General Meeting," "on Monday, the 23d day of April, 1838," "pursuant to summons," "(which meeting also was the first since that of 6th January last)," have publicly, by their Secretary, denied the truth of his statement, thus:—

"Resolved,—That this meeting consider it due to themselves to state, that they have NOT protested against the views of the College, or against the plan proposed by the Pharmacy Committee. The difference between the Members and the Licentiates upon the point at issue—namely, the Apothecary question—being, that the Licentiates propose, as the first object, a plan which appears to the Members to be impracticable; who have, therefore, selected a course which the Licentiates have expressed their willingness to adopt, if their own view should fail of success."

This Resolution, too, was read, by their desire, at the College, on "the 7th of May," in Dr. A.'s presence; yet, on that very day, in his last letter, dated "7th May, Dublin," he still, for the meridian of England, re-asserted, what he and all here knew had been declared by them untrue, in his own hearing.

He asserts, over and over again, that the acts, proceedings, and petition of the College and its Committee in this matter, are only "the proceedings of the Pharmacy party within the College" (p. 298*).

If you look in *Saunders's Dublin News-Letter* of "15th May, 1838," you will see, that at a General Meeting of the College, by summons to all Members, on the "14th," when 63 members actually attended, of whom, for a time, he was one, they again "unanimously resolved," among other things, that "we unanimously and fully concur in the principle and prayer of that Petition" to parliament about the Pharmacy laws. If unanimous meetings of 51, 53, and 63 members, on summonses, be a party, what is the College?

He asserts, that on the "17th of February, 1834," he alone "opposed the plan of Dispensing and Selling medicines, and opposes it now" (p. 299*). The resolution, that he says he then opposed in a minority of 1 to 29, did not state one word about selling medicines, nor relate to the Members or Licentiates of the College resident (like him and us) in Dublin at all, but to the Members and Licentiates resident in the country, as follows:—"That it appears expedient, that all Members and Licentiates of the College, who are engaged in practice at a distance beyond ten miles from Dublin, shall have the privilege of dispensing medicines to their own patients." Therefore, what this one Member selfishly opposed then was the obtaining legislative permission for all Country Members and Licentiates to supply their own patients with medicines (without liability to prosecution, under the "22d section" of the "Irish Apothecaries' Act of 1791"); while he still remained tied up, as all Members and Licentiates, within Dublin and ten miles round, were and are by our present charter, and by the above-named Act. All other members present were willing, forgetting themselves, to seek this advantage for others. However, Dr. A. still says he is consistent all through, though he, a Member resident in Dublin,

then opposed even the gratuitous supply of medicines by the Country Members and Licentiates. And although he says now (p. 932), he is "an advocate for the possession, by every physician and surgeon, of a right to give medicine if they please," yet he asks (p. 298*), "Do I now oppose what I before advocated?" Am I "guilty of inconsistency?" To which the answer will be, "Yes, surely; for you say you now advocate what, it is proved, you before opposed." You are consistent in nothing, but in stating uniformly, and yet contradictorily, what can be equally and wholly disproved by documents truly quoted, and facts fairly stated.

Dr. A. states (p. 945*), that "the Apothecaries do not even require attendance upon a surgical lecture." "The Report," "published by order of the Society of Apothecaries," just before that letter of his, states (p. 1, section 2), that "the Apothecaries' Company requires the student to furnish evidence of having studied" "surgery" "in addition to his peculiar department." They say (section 3), "surgery does not change its nature, although taught at Apothecaries' Hall." They speak (p. 2, section 5) of the "surgical classes" and "the Apothecaries' Hall certificates," of having "attended" on the lectures as given from "the chair of surgery" and "of anatomy," "at Apothecaries' Hall," now held by "Messrs. Ellis and Alcock." And even Dr. A. himself forgets his own previous misstatements; for (p. 302*) he asserts, that the College opposition has existed "ever since the Apothecaries had the hardihood to establish a School, where Anatomy, Surgery, Pharmacy, and Midwifery, are to be taught."

Dr. A. says (p. 299*), "It is recorded in the College minutes" of the "9th Nov. 1837," "though Dr. C. O. has omitted to mention it," "that there were but 38 members in the room when the latter question came on," namely, the appointment of the Pharmacy Committee. Had I "mentioned" it, I should have wilfully stated a deliberate falsehood; for no such thing, nor any thing like it, is recorded in the minutes; but it is "recorded," that on another quite different previous question, viz. the first that came on that day (and which did not concern the interests of the College at all, as the Pharmacy one does, but merely related to one of its professors), 38 had voted; and it is also equally "recorded," that the total number present that day was 51; and that the Committee was also appointed without a dissentient voice.

Dr. A. says (p. 302*), that "the College at present refuses, at the instigation of the School party, to recognize the Professors of the School of Physic." He knows, as well as all the Members, that the whole College has refused this recognition for many years, and solely because the School of Physic had previously determined not to recognize its School; and he knows that the present Anatomy Professor in the Physic School was himself the most decided advocate for refusing his predecessor's certificates, as a just retaliation, to force, if possible, a recognition of his own certificates, from the similar chair in the College, by the School of Physic, he not then foreseeing his own transfer to it.

His and his colleague's erroneous statement (on p. 301*), as to the dates of the "Pharmacy Committee's Report" and the "Licentiates' Report," was previously contradicted, by the true statement of the whole matter (on p. 148*); so that further explanation is needless.

Having thus shown that in four matters, and even as to his own School and his employers' requirements, Dr. A. has stated, as already proved in a dozen other matters, the direct reverse of what he and all here knew to be the facts, it is quite needless to go into detail, as to the other secondary matters, to prove that all his subsidiary fabrications, either as to myself or others—as to the College's or its Committee's minutes, documents, or proceedings—as to its motives or objects, or those of myself or others, present or prospective—are, in no instance, either now or hereafter, to be relied on in the least degree. I shall therefore, if possible, never waste time again in replying to any more of his asseverations.

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11, North Great George's Street,
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LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton-Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXI.

Diseases of the Parenchyma of the Lung (continued).—Phthisis Pulmonalis.—Anatomical History.—Granulations.—Diffused Induration.—Yellow Tubercle.—Tuberculous Infiltration.—Vomicæ and Cavities.—Pathological History.—Nature of the Indurations; Views of Laennec, Andral, and Carswell; Explanation of their Production and Changes.—Nature of Yellow Tubercle; Views of Authors.—Explanation of its Production and Changes.—Causes of the Development of Phthisical Lesions.—Seat of Tuberculous Deposits.—Contraction and Obliteration of Tuberculous Cavities.—Cretaceous Tubercles.

The last class of diseases of the lungs which I shall consider, are those comprehended under the head *phthisis pulmonalis*, or *pulmonary consumption*. By this name, and more popularly by that of *decline*, is implied a wasting of the body from the effect of a disorganizing process going on in the lungs. It is, unfortunately, too familiar to us all, to need any further definition or description; and in order to preserve that rational connexion which these lectures are designed to exhibit, between the physical changes and pathological lesions, and the signs and symptoms which they produce, it will be well to give you, in the first instance, a brief sketch of the anatomical characters of consumptive disease of the lungs; we shall then be able to trace it in its progress through its various stages and complications, and to comprehend the changes of physical properties which it may induce in the organs of the chest.

When we examine the lungs of persons who have died of consumption, we find them greatly changed from their natural condition: they are more or less consolidated in irregular masses, and on cutting into them they are generally also excavated in parts into hollows of various sizes, which are either empty or contain a thick liquid matter. On closer examination, and after attentively observing the lungs of many consumptive persons, we are enabled to classify the morbid conditions which they present under the following heads:—

1. On pressing the softer parts of the lung between the fingers, we feel in it a number of hard little bodies, and on cutting into it we see them roundish granules, of a light semitransparent reddish drab or skin-colour, sometimes more grey or ash-coloured, more rarely devoid of colour, and quite transparent, of sizes varying from a pin's head to a hemp-seed. Their hardness is considerable, sometimes almost equalling that of cartilage; these are the *miliary granulations* or *miliary tubercles* of Laennec and other writers. They are sometimes found singly, studding a tissue otherwise healthy; but more commonly they are in groups of several together, and then they are either clustered in bunches, like berries, or they form a considerable mass, with the interstitial tissue consolidated and indurated between them. They are most commonly distinct in the inferior lobes; in the upper parts, and near the root of the lung, they are usually conglomerated in masses. In the upper

parts, too, it is common to find in them opaque specks of a yellowish white colour, which are generally in the centres of the granules, sometimes at their margins. In the distinct granulations the opaque part is little more than a speck; but in those which form a conglomerated mass, the opacity is sometimes seen extending from granule to granule; and in others it constitutes a mass of considerable size within the mass of granulations. [These and the following descriptions were illustrated by drawings.]

2. In the next place we find consolidation of another kind. It is diffused through some extent of the pulmonary tissue, of no particular shape, except that sometimes it seems to be limited to single lobules. In consistence it varies, but often it is nearly as hard as the miliary granulations, and in parts it has somewhat of their semi-transparency and colour, but generally it has a darker hue, from the colour of the blood and of the black pulmonary matter in it. The consolidation is pretty complete, and the pulmonary texture cannot be distinguished in it, except here and there the coats of a large blood-vessel, bronchus, or an interlobular septum, which are often thickened, and partake of the induration. In other cases the consolidation is less perfect, there being still some air in the tissue, and the adjoining tissue being often emphysematous. In these indurated masses are often to be seen, here and there, more opaque lighter-coloured spots, which are sometimes quite distinct and of a dead yellowish white, like those seen in the miliary granulations; but they are here less regular in their shape and size, being sometimes in streaks, curves, and angles, and mottling the dark consolidated texture with spots and patches of a lighter and opaque hue. In the lightest and most opaque spots we recognize what we must describe as the third morbid appearance to be met with in phthisical lungs, namely:—

3. Opaque yellowish-white masses of various form and size, generally somewhat rounded. Some of them are nearly as solid as the dark or semi-transparent indurations, but they are much less tough; others have more or less of a cheesy consistence, and some are found in parts approaching to a state of grumous fluidity, still retaining their light colour and opacity. These opaque masses are commonly found within the indurations from which they appear to be formed, and they are just of the same character as the specks before described as occurring in some of the single or aggregated miliary granulations. In fact, as these specks are seen in some in greater number and extent, and preceded by an intermediate state of opacity, in parts to pervade the whole mass, it may be fairly concluded that the clusters and nodules of granulations are also converted into this same opaque, friable, yellowish-white matter. This matter, which is indisputably entitled to be distinguished as *tuberculous*, is occasionally found also in other situations, unaccompanied by any induration; such as in the interior of dilated vesicles and bronchial tubes, in masses under the pulmonary pleura, and in the bronchial glands. In these instances it is commonly of a friable or cheesy consistence, and has not the hardness which it seems to retain for a while when it has originated in the indurated tissue. But this yellow tuberculous matter, however tough and hard it may be in the first instance, tends to soften, either partially or wholly; and thus the masses are sometimes found consisting of loose clots, in a purilaginous fluid, or wholly reduced into a curdy kind of puriform matter. The tuberculous matter is also not unfrequently found diffused through a considerable extent of the pulmonary texture, constituting the *infiltrated tubercle* of Laennec. In its earlier condition, the lung, in this state, closely resembles the last stage of hepatization, when the opacity, which precedes suppuration, shews itself. It is very much mottled or marbled, for besides the yellowish white opacity which is seen in different degrees in its different parts, there is the black pulmonary matter giving it a grey or greenish colour; there are the whiter coats of vessels and interlobular septa, and spots of red tissue less affected. When the lung in this state is cut or torn, which it commonly may be with facility, its interior presents a granular surface, like that of hepatization, and except that its colour is more varied, and it has generally more of the light opacity of tuberculous matter, it resembles a hepatized lung more nearly than any thing else. But in it are seen what are rarely met with in hepatized lungs, circumscribed abscesses or cavities, containing a fluid matter. To this softened and fluid state, then, all the conditions that I have been describing, tend to pass; and they constitute *vomicæ*: when so softened, the matter is evacuated into the bronchial tubes, leaving behind the lesions, which I have next to describe.

4. Lastly, we find cavities or excavations very various in number and form, and of sizes from that of a cherry-stone upwards, to the extent of a whole lobe. Sometimes they contain more or less of the remains of the softened tubercle, or a more liquid pus, or a mixed serous or mucous fluid tinged with blood, or they may be empty. They communicate with

each other and with the bronchial tubes, the process of softening and ulceration having destroyed the terminations of these tubes; but blood-vessels and interlobular septa are often spared by the destructive process, and form cords or bands across the cavities. The blood-vessels are, however, almost always impervious in these cases, and the septa are thickened by depositions of lymph. Their walls are composed of the consolidated tissue of the lung, rough, and occasionally sloughy, or of an irregular coat of lymph, or in old cavities of a sort of adventitious membrane, which is sometimes thin, like a mucous membrane, and in some more rigid, and of a fibro-cartilaginous character. When these cavities approach to the pleural surface of the lung there is often a coating of lymph or false membrane on the pleura at the part, which either thickens it, or unites it by adhesions to the costal pleura. Sometimes, however, there is no such deposit or adhesion; and it occasionally happens that the pleura is also ulcerated, gives way, and being perforated, allows the contents of the cavity, and the air from the bronchi, to pass into the pleural sac, constituting pneumothorax and pleuritic inflammation. There is this remarkable in the position and size of the cavities, that they are almost always largest and most numerous near the summits of the lungs, there being often one or more cavities there, when in the inferior lobes there are only scattered indurations. In fact, it may be generally observed of all those lesions connected with phthisis, that they affect the upper and posterior more than the lower and anterior lobes; and that they are also more advanced in the former. This is, however, most remarkable with the circumscribed indurations and tubercles; for with the diffused consolidations, especially of the light opaque kind (tuberculous infiltration,) the middle and inferior lobes are often also affected, and cavities are more commonly found in every part.

Besides these chief and more essential changes of the lungs, in phthisis, many others are often found of a more accidental character, such as hæmorrhagic effusion and consolidation, inflammatory congestion, and hepatization of the lung—products of inflammation in the pleura; inflammation, ulceration, thickening, and dilatation of the bronchial tubes; irregular dilatation of the air-cells, sometimes with increased flaccidity, sometimes with rigidity; enlargement and induration of the bronchial glands, with yellow tuberculous matter in its different states in them.

Let us now generalize a little on these changes, which anatomy has discovered to us in the lungs of the consumptive. They may, for the most part, be reduced to two. 1. Consolidation, generally of an indurated kind, and either almost colourless and transparent, or of pearly grey or reddish drab, or of a dark red or more dingy colour. 2. An opaque yellowish white, parsnip-coloured, friable matter, of various degrees of consistency, being first hard, and afterwards becoming soft, and forming vomicæ: this lighter opaque matter, which is properly called tuberculous, is produced commonly within the consolidations just named, but sometimes elsewhere.

What, then, are these morbid conditions of the lung? What pathological view are we to take of them; that is, what are we to think of their nature and origin? Let us see first what the most eminent modern pathologists say respecting them. Laennec considered them "accidental productions, that is, real foreign bodies, which spring up in the substance of the lungs, and may be developed in any other texture of the body." We owe great respect to the name of Laennec, for, as you know, we have largely profited by his labours; but I must in candour confess, that his view of the origin and nature of tubercle has never been satisfactory or even intelligible to me. The only way in which I can interpret it is, that tubercles are parasitical bodies, originating in an unknown way, possessing a life and structure of their own, growing by attracting matter to them, and tending, by their own inherent properties, to go through a certain series of changes. The transparent miliary granulations, the grey miliary tubercles, the grey diffused induration, and a gelatinous infiltration, he looked on all as varieties of these bodies in their first stage, and as all tending *per se* first to become opaque and yellow, then crude tubercle, which is still hard, and ultimately to soften into a cheesy or pasty liquid, which is the mature tubercle.

Now this view involves several assumptions little supported by analogy; for instance, that bodies so different in physical character and texture are the same, and that the stages through which they pass are produced by their inherent properties, and not by the modified properties of the tissue of the organ; and it assumes what has been disproved by observation, that the opaque yellow tubercle is always preceded by the grey or semi-transparent, and that the grey induration must always in time become yellow tubercle. Whilst, then, we admit the accuracy of Laennec's observation, that the grey and semi-transparent indurations tend generally to become yellow tubercle, we

must consider his view of the change to be too unsupported and hypothetical to be received as satisfactory.

The view of M. Andral is far more simple, and involves fewer assumptions. He considers tubercles generally to be the result of a modified nutrition of the texture, and that they are produced, and go through their changes, through the agency of the vessels of the part, and the blood which circulates in them. Although he admits that the miliary and diffused indurations precede the production of yellow tuberculous matter, he supposes them to be, not an early stage of this matter, but the result of chronic inflammation affecting the individual vesicles, or the general texture. The chief peculiarity of this view is, the explanation of the regular form and size of miliary tubercles, by locating them in the individual air-vesicles, just as the same author first accounted for the granulations of a hepatized lung. That the diffused induration, called by Laennec the first stage of tubercle, is the result of chronic inflammation, has been admitted by Chomel and Louis, who otherwise rather incline to Laennec's opinions.

The most recent writer on this subject is Dr. Carswell, with whose admirable "Illustrations of the Elementary Forms of Disease," I dare say you are familiar. He neither adopts the opinion that the indurations are an early stage of yellow tubercle, nor does he admit that they are more than accidentally connected with it. He supposes yellow tubercle to be a peculiar secretion, which takes place especially from mucous membranes, but that it may accompany other secretions, such as that of inspissated mucus in the air-vesicles, or of dense false membranes on the pleura or peritoneum; and thus he accounts for the grey miliary bodies with specks of yellow tuberculous matter, and the similar admixture of this matter with deposits on serous membranes. But if you examine the miliary bodies minutely, you will be soon satisfied that they contain no inspissated mucus that will account for their increased hardness; the induration is obviously in the texture itself, and not merely contained within the cells: and Dr. Carswell's view throws no light on the manner in which yellow tubercle is produced in the grey or dark indurations, whether miliary or diffused; yet this is a point as well established as any in the pathology of phthisis. I must not, however, take up your time by special pleading against particular opinions: let us examine the subject of the pathological changes rationally, in connexion with what we have learnt of the pathology of other textures, and we may come to a more satisfactory view than any which we have noticed. I must start by telling you, that I owe my notions on the matter, in a great measure, to Professor Alison; and although I do not know that our views are now quite alike, yet if I have fallen into the right track, it was his researches that directed me to it in the first instance.

If we examine the induration which commonly precedes the production of yellow tubercle, we find that it differs from the healthy structure certainly in these respects; that it contains a greatly increased quantity of matter, and that this matter is generally harder than the healthy tissue. Now this increase of substance implies either increased secretion or diminished absorption. That absorption is not diminished in the tissue, is plain from the fact that portions of the healthy tissue are at the same time removed by this process; and that increased secretion is present, is proved by the fact, that the indurated texture presents new characters, and is not a simple accumulation of the matter of the natural tissue. Now, to produce an increase in the nutritive secretion, there must, according to a well-established pathological law, be an increased determination of blood to the part. Does this amount to inflammation? Let us see whether indubitable inflammation ever produces an effect on a simple membrane, like that in question. In treating of pleurisy we found that acute inflammation of the pleura causes an overflow of the nutritive secretion in the form of coagulable lymph, which may soon become highly organized into a soft cellular membrane; but when the inflammation is of a lower and more chronic character, the effused matter is slower in the process of organization, and forms a harder texture of lower vitality—a kind of fibrous or fibro-cartilaginous structure. Will not the same observation apply to the parenchyma of the lung? The overflow of the nutritive secretion caused by acute pneumonia, we have found to constitute red hepatization, whether granular or diffused; but when we came to examine the effect of lower and more prolonged inflammation on the tissue of the lung, we had to describe a dark consolidation with increased density, in no essential particular differing from some forms of the indurations which we are now considering. Thus the hard, compact, granular consolidation occurring around excavations, gangrenous as well as tuberculous, and admitted even by Laennec to be the result of chronic inflammation, has occasionally the colour and consistence of the indurations which precede the formation of yellow tubercle; and as I have shown that

there is a non-granular form of acute hepatization, so it is reasonable to expect that there may be a diffused or uniform kind of consolidation resulting from chronic inflammation, affecting the interstitial more than the vesicular texture. To such a condition the grey diffused induration, called by Laennec the first stage of tubercle, so exactly answers, that Andral, Chomel, Louis, and Carswell, all concur in considering it to be a chronic form of hepatization. When it is the sequel of the acute disease, or of long continued pulmonary congestion, there is often much redness in the induration; but where the irritation has been of long continuance, and unattended with the more sthenic degrees of vascular action, or a very congested state of the lung, the texture is more semitransparent, dense, and grey, or variously modified by the black pulmonary matter in it. The more uniform colourless masses occasionally present, may be traced to be the interlobular septa, or cellular tissue around the vessels, in a state of indurated hypertrophy. In these bloodless and almost cartilaginous portions, we see the exact characters of the matter of which the miliary granulations, or grey miliary tubercles, are minute samples; and if we adopt the view of Andral, that the regular size of these depends on the chronic induration being located in the coats of individual vesicles, we shall see a sufficient reason for their isolated or clustered characters. Moreover, as we have traced the diffused consolidations of the lung through various gradations, from acute, soft, red hepatization, down to grey induration, so M. Andral has found the miliary bodies presenting the same series of gradation; being sometimes soft and red, in other cases livid and harder, whilst the same lung may contain also the granulations similar in size, but pale or grey, and of various degrees of induration. With regard to the more rare transparent miliary granulations of Bayle, described by Laennec also as an early stage of tubercle, I can state that I have twice, at least, found them on the pleura and on the peritoneum, when other parts of these membranes were coated with dense false membranes, and when there was no trace of yellow tubercle in the body. As to the commoner pale granular deposits on the serous membranes, they are the acknowledged products of chronic inflammation, and their numbers and isolated circumscribed form constitute another point of resemblance to the miliary indurations which, in the lung, pass into the state of yellow tubercle. Without, then, going so far as to assert that the miliary indurations of the pulmonary tissue are always dependent on chronic inflammation, I think we may fairly say that both they and the diffused induration are more akin to the products of this process than to any other that we are acquainted with. The condition of the blood we found to be a material element in determining the products of inflammation in the case of pleurisy; so doubtless it is likewise concerned in the modified nutritive secretions of other textures. The more vital and organizable products are furnished by blood rich in fibrine; and they are easily reabsorbed, or, if organized, are mobile, and sufficiently like the tissues of the part, not to incommode or irritate them. But if the blood be poor in nutrient matter, the deposit from it may be susceptible of only a low degree of organization, and will consequently be not only more difficult of absorption, but also less assimilable to the texture of the part, and more calculated to irritate it as a foreign body. It may thus be seen, that although the lowest degrees of inflammation may be alone capable of producing the chronic indurations, when the blood is healthy, yet, when it is diseased, various degrees of inflammation, nay, even the ordinary nutrient process without inflammation, may be accompanied by the deposition of a lymph of a degraded character, and organizable only into a dense semi-cartilaginous tissue. I shall speak of this again presently.

The semi-transparent grey or dark induration is not always converted into crude yellow tubercle. Sometimes it is the seat of vomicæ, which contain a dirty or bloody pus; and although even in this the curdy matter of tubercle is sometimes seen, it is plain that these vomicæ result from a more direct and speedy process of ulceration or irregular suppuration; another analogous result of continued irritation in the condensed tissue.

And now let us pursue the same mode of inquiry with regard to the opaque pale-yellow tuberculous matter which characterizes the second class of phthisical lesions. Laennec calls this the second stage of tubercle, but neither for its formation nor for its subsequent softening does he assign any other cause than an assumed and unintelligible "inherent property." Dr. Carswell is much more explicit on this point; in fact, all his descriptions of tubercle apply only to this kind of matter. He considers it to be a secretion, *sui generis*, totally destitute of organization—an effete matter, continually separated from blood in an unhealthy state, thrown out chiefly on the free surface of mucous membranes, and producing bad consequences only in proportion as it accumulates in organs, impedes their functions, and acts on them as foreign matter. This opinion, so far as it regards the nature of tuberculous

matter, does not differ materially from that long since published by M. Andral, who regards tuberculous matter as a peculiar modification of secretion, more analogous to pus than to any other matter; but materially differing from it. And truly, if we survey the general characters of tuberculous matter, consisting of pale, opaque, albuminous particles, generally deposited in a tissue previously consolidated, and the manner in which it tends to become soft and liquid, forming circumscribed collections like abscesses, or infiltrated through the texture, from which it is expelled like foreign matter, we cannot fail to see some general resemblances to the process of suppuration. We have found that the consolidating lymph of a hepatized lung becomes opaque and light coloured before it softens into pus; but the changes here are too rapid to admit of their being fully watched. But when an analogous process goes on more slowly, and in a simple structure, as in the pleura, we can better trace the resemblance. Thus in the latent and more protracted forms of pleurisy, we have had occasion to remark that the lymph first effused forms a dense tissue of low vitality, and resembling fibro-cartilage in hardness and colour. If the irritation still continues, this new structure throws out a lymph of still lower vitality, in friable shreds, and, in some instances, in form of a curdy matter, totally incapable of organization, which, mixing with the effused serum, constitutes one kind of empyema. Now such a process in the pulmonary tissue would produce all the changes which we have been describing, in the production successively of grey induration, crude tubercle, and softened tubercle. Thus a portion of this tissue (whether a single vesicle or part of a lobe), generally under the influence of chronic inflammation or local congestion, becomes indurated by the effusion of lymph susceptible of a low degree of organization. The original irritation continuing, or the very induration itself determining an increased flow of blood to the part, the new structure evolves in the looser parts of its substance a still less organic form of albuminous matter, a pale opaque curdy substance; but as this cannot be (like that from the pleura) thrown off, it presses on its indurated matrix, and causing its absorption, accumulates in its place: thus is effected the conversion of the grey induration into crude tubercle. This entirely unorganized substance acting as a foreign body on the adjoining tissues and the remains of the living texture within it, causes irritation, ulceration, and the effusion of serum and pus, which, as my friend, M. Lombard, of Geneva, first explained, softens and breaks up the crude tubercle into the curdy grumous matter of the matured tubercle. The same irritation and ulceration gives vent to the matter through one or more bronchial tubes; and thus are formed the tuberculous cavities.

But we have seen that yellow tuberculous matter is produced not only in the grey indurations, whether granular or diffused, but also in softer consolidations, like that of hepatization. It is sometimes seen in rounded circumscribed masses in a hepatized lung; in other instances it pervades with its opaque yellowish grey or mottled colour a whole consolidated lobe. In this, the infiltrated tubercle of Laennec, the grey hepatization of Andral, there are often here and there cavities containing a curdy pus, and communicating with the ulcerated bronchi. There are also occasionally found in it circumscribed indurations and tubercles of older date; but in other instances no other form of chronic lesion is present, and the lung has the appearance of inflammatory engorgement in some parts, of common red hepatization in others, whilst other portions of the same consistence have the opaque yellowish colour of tubercle; and these conditions pass by such imperceptible gradations into each other, that it is impossible to avoid the conclusion that they are parts of the same process. And can we wonder that vessels of the inflamed parenchyma of the lung should at the same time pour out in some parts organizable lymph, in others tuberculous matter, which is the same as albumen, only not organizable—can we wonder, I say, when we see a similar variety produced upon the inflamed pleura, where some portions are covered with good lymph, others with a curdy matter like tubercle, whilst many albuminous particles, also in an unorganizable state, are thrown off with the liquid into the sac? So also in the very masses of coagulable lymph that an inflamed pleura or peritoneum throws out, there have been sometimes found distinct purulent and tuberculous deposits: and why should we wonder at this either? Lymph, pus, and tubercle, are the same albuminous matter, and differ from each other in mechanical condition and susceptibility of organization, rather than in their chemical nature. According to the microscopic researches of Gendrin, part of which I have myself been able to confirm, lymph is composed of regular globules, which by a vital attraction string themselves into fibres and films, which may become organized, and form membranes. Pus consists of larger and less regular globules suspended in serum; but these globules have no vital attraction for each other, and remain loose, and consequently insusceptible of further organization. Tu-

berculous matter is wholly devoid of organic form, its particles even not being globular, but irregular, like those of mere dirt or clay, and it must remain where formed, a dead inert mass, until decomposed by chemical agency, or changed by the operation of the surrounding tissues. You can readily perceive that these different properties, although possessed by matter chemically the same, and from the same source, must lead to all that variety of results which we know to follow organizable, purulent, and tuberculous deposits. But you are not to suppose that the characters of these matters are always distinct; that lymph is always equally organizable, or perfectly free from the greenish colour and disintegrating globularity of pus, or even from the lifeless curdy particles of tubercles; nor that tuberculous matter shall not often contain flakes or films of imperfect lymph. The diffused tuberculation or infiltration of the lung from inflammation, that we have just been speaking of, generally presents a matter in this transition state. It is neither good organizable lymph, nor is it wholly unorganized tubercle; and the albuminous effusions on serous and mucous surfaces not unfrequently present such an intermediate state, that it is difficult to determine to which class they most belong. I have again strongly to repeat what I long since told you with regard to the inflammatory secretions of mucous and serous membranes—*that lymph, pus, and tubercle, pass by imperceptible gradations into each other.* The history of the intermediate products has yet to be more fully studied; and it is a subject of immense importance, for they probably constitute those forms of phthisical lesions which it is most within the power of medicine to control.

But lastly, we have found that tuberculous matter is sometimes deposited in tissues and on surfaces bearing no marks of inflammation or other disease. The structures thus affected are commonly those either very vascular naturally, or peculiarly subject to congestion of blood, such as the bronchial glands, the lungs of children, the spleen of monkeys, &c.; and viewing tuberculous matter as a deposit of unhealthy fibrin from the blood, we see a reason, as Dr. Carswell observes, why it is most likely to be found in those organs where the blood accumulates or passes slowly. Whatever may be the cause which determines the deposition of tuberculous matter in these cases, we know that pus also is sometimes secreted in parts unaffected with inflammation, as in the purulent deposits in the viscera after great surgical operations; and in the profuse discharges of matter from the bronchial membrane, which is found after death to be paler than usual: nay, pus has been occasionally found in the centre of fibrinous coagula in the heart, when no purulent matter could be discovered elsewhere in the body. So, likewise, tuberculous matter has been met with in the blood itself, within coagula in the spleen and in the heart, and in fibrinous concretions within the blood-vessels. This circumstance tends to shew that the fibrinous portions of the blood are liable to be converted into tubercle, independently of any action of the vessels; they lose their vitality, and may in proportion be merely deposited in tissues, or on surfaces, without the presence of any irritation. We are thus again, as in case of the grey indurations, led to trace to the condition of the blood the cause of consumptive disease of the lung; and it is probably a diseased state of this fluid that constitutes what is called the tuberculous or scrofulous diathesis, in which there is a tendency by vessels in different degrees of activity to deposit tubercle instead of lymph; and when this diseased state exists to a great extent, the tuberculous matter is excreted from the blood without any increased vascular action, but merely as an accompaniment of the natural secretion of a membrane, or instead of the ordinary nutrient deposit of a tissue; and thus it may be either an external or an interstitial deposit. Whatever in such cases determines the first deposition of tubercle in a tissue, will with greater facility effect its growth by the addition of similar matter to a ready formed nucleus. The tendency to the deposit of yellow tubercle, independently of irritation, implies a condition of the blood even more depraved than that which leads, under the same circumstance, to the formation of the chronic indurations; it is an ulterior degradation of the fibrinous nutriment of the tissues, replacing them by a matter not merely inapt in texture and of lower vitality, but wholly destitute of life, and of the principle of organization. When, therefore, tuberculous matter is found in an uninflamed tissue, it may be looked on as a sign of a most decided constitutional taint. In such conditions of the system tuberculous depositions may take place with great rapidity; and as they are already almost ripe for elimination, the ulceration and destruction of the lung will soon follow; but nothing can give development to tuberculous disease with such fearful speed, as the occurrence of acute inflammation in the pulmonary tissue. It is from this process in a highly tuberculous constitution, I believe, that the general tuberculous consolidation, called infiltrated tubercle, takes place. The matter deposited is often rather a mixture, or inter-

mediate state, of lymph and tubercle, one product predominating in one part, the other in another; but it is altogether beyond the reach of the sorbefacient remedies which avail in pneumonia to promote the absorption of simple lymph; and if it do not destroy life by its solid interference with the function of the lung, it speedily runs in many points into softening and suppuration, and the patient is carried off by a *galloping* consumption. In this case the lungs are found more or less solidified, and on incision incipient cavities are seen almost in every part; but there is no induration; the most solid parts have scarcely more substance than a hepatized lung, and they even more readily break down under the fingers.

The development of the indurations is a work of more time, and their structure makes the destructive process which they induce more tardy; nay, the diffused indurations that form the walls of softened tubercles and vomicæ seem to be a provision of nature towards the limitation of the consuming disease; but under certain circumstances even these are formed to such an extent, and so soon, that the patient is destroyed by their first development. In other cases the first formation of indurations is not sufficient to prove fatal, but as some of these are becoming converted into tubercles and cavities, another eruption or crop of them (as Laennec calls it) takes place, and causes suffocation. I do not conceive, however, that in either of these cases the miliary granulations alone are sufficient to destroy life; but there is commonly with them an effusion of serum or of mucus, which completes the fatal obstruction. We have not time to inquire into all the circumstances which may lead to the development of these indurations; but I will mention to you two cases which I have lately seen, and they are not the only ones of the kind, which illustrate well some of the more important constitutional and local causes of these formidable lesions.

A young woman was attacked with continued fever, which, after several weeks of considerable danger, left her in a state of extreme weakness, so that she could only lie on her back, in which posture she remained altogether for at least six weeks. As the fever left her she began for the first time to cough, her breath became shorter, her pulse did not lose its frequency, and her faculties remained in an obtuse lethargic state. She died about a month after the first commencement of the cough. On dissection the whole anterior parts of both lungs were quite healthy to the depth of from one and a half to two inches. The whole of the posterior portions were highly congested with fluid blood, those quite behind scarcely containing any air. But the remarkable point was, that *every part of these congested portions was thickly studded with miliary granulations, whilst not one was to be found in the anterior parts.*

A lad of about 16 years of age became the subject of an attack of pleurisy of the right side, having previously enjoyed good health. It was inefficiently treated, and had become chronic when I first saw him. He considered himself, however, cured of the pleurisy, and complained only of weakness, although there was dulness on percussion, no respiration, and slight enlargement of the inferior two-thirds of the right chest. The sound and respiration above were clear but bronchial, and that of the left side was puerile and without rhonchus. An attack of bronchitis subsequently came on, and from that time till the fatal termination, a month after, there were mucous and subcrepitant rhonchi on both sides, and the expectoration at length became muco-purulent. In the lower half of the right side there were between two and three pints of purulent serum containing many flocculi. The lower lobes of the lung were quite compressed, and adhered to the mediastinum, and by a pillar to the diaphragm. The upper lobe was not compressed, and a dense fibro-cartilaginous membrane bound it to the chest, and limited the liquid effusion. The left lung was throughout studded with miliary tubercles, and there were a few in the upper part of the right lung, but *none in its lower compressed portions.*

In both these cases great constitutional debility preceded the development of the indurations; in both they were formed only in those parts of the lungs to which the blood had freest access, and were absent in those which, under the influence of gravitation acting on a weak circulation in one case, and of pressure in the other, received an unusually scanty supply of blood.

It will be hardly necessary, after what you have heard, to discuss the question of the seat of the hard grey and the yellow tuberculous deposition. If tubercle be, as we suppose, a degraded condition of the fibrine or nutrient principle of the blood, we may expect it to be deposited wherever the nutritive or the secreting process is carried on—wherever lymph or pus is occasionally found — wherever, in short, blood-vessels run. Instead of being, as supposed by Dr. Carswell, most commonly deposited on free mucous surfaces, its appearance on them is less usual, and bears the small proportion of the liability of these surfaces to the plastic inflammations. Lymph is sometimes, but rarely, secreted by mucous membranes; and it is about as common to find tuberculous matter, or a

degenerated fibrin akin to it, in the air-tubes or cells, when the coats of these are entire. It is more common on the serous membranes; but it is much more usually (but not, as Lombard maintains, exclusively) deposited in the interstitial cellular texture of organs, especially those which are highly vascular, and in which blood is apt to stagnate or accumulate. I cannot at all assent to that most mechanical notion of Dr. Carswell, that tuberculous matter is, in the early stage of the disease, secreted in equal abundance from all parts of the mucous membrane, and that the only reason why it accumulates sooner and more in the upper lobes, is that their smaller extent of motion prevents its perfect elimination from those parts. Were this true, how easy would the diagnosis of consumption in its earliest stage be! for every atom of tubercle retained in the upper lobes there would be masses expectorated from the lower; yet we find that it is very rare to see any expectoration in the earliest stage, except that of a thin transparent phlegm. More probable is the opinion of Broussais, that the upper lobes are the first and most extensive seat of tuberculous change, because the bronchial tubes there are shorter, and inflammation more readily passes along them to the cells. But I apprehend the real reason of their peculiar liability is in the greater abundance of interstitial tissue in them than in the lower lobes. The bronchi, instead of being lengthened out into mere membranous tubes before they terminate in cells, divide more immediately and abruptly into short branches and cells, and the delicate vesicular structure is thus mixed up with a good deal of the interstitial cellular tissue which every where surrounds the earlier bronchial divisions. The smaller capacity of motion possessed by the upper lobes of the lungs may, too, have a share in disposing them to become the seat of tubercular deposit, not by permitting it to accumulate, but by causing bronchial obstructions to the respiration, favouring those congestions of blood, &c., which we have seen are circumstances peculiarly favourable to the formation of tubercles. It is not the yellow tubercle only that predominates particularly in the upper parts of the lung; I should say that it is rather the grey indurations, which become afterwards converted into yellow tubercle. Primary tuberculous deposits are nearly as common in other parts of the lung as at the apex. But if we regard the induration as an organized tissue, formed of albuminous matter of low vitality, but still above that of tubercle, we render its production referrible to the same causes as those which engender tubercle, and sufficiently explain the affinity of one lesion to the other.

I have described the ordinary changes of tubercle, from its primary deposition to its softening and evacuation, and the formation of an ulcerous cavern. These caverns become, if life lasts, lined with a deposit of a mixed nature, but with an albuminous lymph for its basis; and this is commonly mixed with tuberculous and purulent matter. Hence it seldom adheres long, but is broken up, detached, and expectorated. When the constitutional powers are stronger, and the lung less extensively diseased, the coating of the cavity is susceptible of organization, and in time forms a fibrous, or fibro-cartilaginous membrane, which pretty smoothly lines the cavity. If the cavity communicate freely with the bronchi, it will be kept by the pressure of the air from any considerable contraction, to which it naturally tends; and in some instances we have reason to believe that the tubes do become obstructed, and that the contraction of the membrane ensues, and tends to obliterate the cavity. Such contracted cavities are now and then met with, but scarcely ever quite empty; they contain more or less of a pale-coloured plaster-like matter, which consists chiefly of carbonate and phosphate of lime, and sometimes contains earthy concretions. The contraction is evident from the puckering of the pulmonary tissue visible on the pleural surface near the cavity, and the adjoining vesicles are generally dilated to fill up the space. The cretaceous matter is probably secreted by the fibrous false membrane; but it may have been originally more of the character of tubercle or pus, and being unable to escape, the animal part has been absorbed away, and the earthy insoluble salts are left behind, and accumulate from successive depositions. I have seen a similar matter in the remains of old pleuritic effusions; and perhaps we may associate with it the osseous plates that are deposited in adventitious fibro-cartilaginous membranes, on the pleura, pericardium, and great vessels. This earthy deposit is, however, sometimes connected with an earlier stage of the tubercular formation. I have more than once, in lungs not extensively diseased, met with pale yellowish tubercles, composed of concentric layers of almost cartilaginous hardness: in another part of the same lung these layers are broken or loosened by a plaster-like gritty matter of a calcareous nature; and in another part a whole tubercle may consist of this matter, sometimes containing concretions, and having only a few flakes of albuminous matter in it. This is still more distinctly a specimen of what is commonly called the atheromatous structure, which especially invades the coats of the arteries, and the fibrous parts of the valves of the heart,

where it often proceeds to ossification; and I think that it is to be classed with the grey and yellow tubercle, in so far as it is another variety of matter very low in the scale of organization, and formed of lymph of defective vitality. In these latter cases there is no puckering or trace of contraction about the tubercle until it has evacuated its contents, which it is very slow to do, since it has not the tendency to soften and cause ulceration that makes common scrofulous tubercle so destructive. Hence I have several times seen a few of these tubercles in lungs otherwise healthy, the individuals having died of some other disease.

Our time does not permit me to enter into many other particulars respecting the pathology of tuberculous disease, which would further illustrate the views which we have taken, and enlarge your knowledge of the facts connected with the subject. I refer you especially to the papers of Professor Alison, in the first volume of the Transactions of the Medico-Chirurgical Society of Edinburgh; to Andral's Pathological Anatomy; Louis' Treatise on Phthisis; and Lombard's Essay on Tubercles*. You will also find much valuable observation in the articles *Tubercle* and *Tubercular Phthisis*, in the Cyclopædia, by Drs. Carswell and Clark; but I doubt that you will remain satisfied with the view which the former takes of the pathology of pulmonary tubercles, however ingenious it may appear to be at first sight. In referring you to these different writings, where you will see other opinions stated, and other explanations given, I would ask you to bear in mind the general view that I have given you of the nature of tuberculous deposits, whether grey, dark-coloured, or yellow; that they are formed of lymph of defective vitality, and are either very low in the scale of organization, or altogether beneath it; that whether arising from secretion or textural nutrition locally disturbed by chronic inflammation, or from these processes having to act, in their natural state, or excited by vascular action, on a blood defective in vitalized fibrin, it is this want of vitality in the deposits which determines their character and course, and leads to the formidable lesions which are induced by them.

* To these may be now added a very important paper on the morbid anatomy of 100 cases of phthisis, in the number, of the Edinburgh Medical and Surgical Journal for last January, by the late Dr. Home, jun., whose premature death has deprived the profession of a member of whose talents for observation this first contribution gave the highest promise.

THE CROONIAN LECTURES,

For 1838.

By John Clendinning, M.D., F.R.C.P.

EXPERIMENTS AND OBSERVATIONS RELATING TO THE PATHOLOGY AND PATHOLOGICAL RELATIONS OF THE HEART.

Introductory Remarks.—In physiology, healthy and morbid, there may be distinguished two great classes of functions, which differ very much from each other in the laws which govern them, and the elements and agencies by which their ends are effected. I mean, first, those that are constituted by powers which are dynamic, or *vital simply*, such as those peculiar to the sensorium, to the muscular fibre, to the blood, and the analogous powers and actions in the vegetable physiology—functions whose peculiar conditions and phenomena admit of no mechanical, chemical, or other physical explanation. And secondly, those functions that are of a mixed nature, and to the effectuation of which, organic powers and purely vital properties contribute but in part, often but indirectly, and in which material elements, as carbon, oxygen, &c., and physical forces, chemical or mechanical, are the principal agents. Of the latter, or mixed class, are the visceral functions, with the exception of those of the encephalon and spinal brain; the functions also of the secreting surfaces; and generally all those involuntary actions, organic and humoral, that contribute to the work of *nutrition*, whether in a principal and direct manner, as those of the heart, stomach, and lungs,—or merely secondarily, and as subsidiaries, as appears to be the duty of the liver, spleen, pancreas, kidneys, skin, and lower intestines. Of these two classes of functions, the former, or the purely dynamic and vital powers and functions, possess in themselves a high and peculiar interest. They are most variously and amply developed in the higher animals, and stand out most prominently in that department of organic nature in which alone man can justly claim for himself pre eminence above other animals; I mean the Moral and Intellectual. They have likewise peculiarly mysterious laws, energies, and modes of operation; and at the same time have very extensive relations to, and exercise vast disturbing, and otherwise modifying powers over, the material elements and forces, amid which, and by the support of which, they exist in the organisms. Owing to these and other causes, the constitution and government

of the nervous organs have of late been very laboriously investigated; and considering the imperfection of the methods used by many inquirers, have been investigated with much success: and from those researches unquestionably some advantage has been derived in clinical practice, and more may in future be expected.

Paramount importance of the nutrient functions in grave diseases.—But it does not seem to be from that quarter that the largest accessions to curative power may be looked for, but rather to researches calculated to elucidate the laws of functions less ethereal and noble than those that refer to the external world, and the continuation of the species; those functions, namely, whose appropriate end is the preservation of the individual, and which collectively, may, in a large way of speaking, be called the *nutrient functions*. The functions of relation, both the sensorial and the sexual, have paramount interest and importance for the moralist, the naturalist, and the philosopher; indeed of every colour and character of mind. But to the practitioner of medicine, the paramount function is that of *nutrition* broadly understood, so as to include all the viscera of the chest and abdomen, with their dependent tubes and appropriate stimuli.

The nutritive forces are those that operate the growth and decay of the structures, and determine the character, and regulate the amount of those organic and humoral powers and capacities which are the material causes of life. Almost all grave diseases, whether acute or chronic, are complicated with, or even dependent on, organic changes of condition, implying *abnormal nutrition;* for example, inflammations acute and chronic, common and specific; and in fact all the varieties of the phlogistic process; also, in popular terms, all dropsies, consumptions, asthmas, and palsies, which are at present the great outlets of life, and are known to depend on various modifications of nutrient action in the brain, heart, and lungs. And as the suffering and mortality of such diseases are results mainly of irregular efforts of the *nisus formativus* or *nutritivus*, which seem to me to be in effect the same thing, so the cure of them must consist mainly either in repressing that force, or stimulating it, or altering its direction.

And the relative importance of the nutrient function as a subject for professional study, and medicinal regulation, in the treatment of grave diseases, is, it is conceived, still more important in the present than in former ages; and for this reason. Of late years disease has apparently become more simple as a whole, by the abstraction of a large part of the humoral depravities, that seem to have been main causes of the mortality of the plagues and epidemics in general of past ages. The less favourable crasis of the humors alluded to, appears to have originated from various causes in various combinations, especially in frequent scarcities and famines; in habitual excess in salted food, and deficiency of wholesome vegetable food during the winter and spring seasons; and in defective domestic and personal cleanliness; and defective atmospherical purity at all times. To the preceding may be added, frightful abuse of alcohol by all classes; and very unskilful use of authority in what was intended for wholesome regulation of industry by legislators and magistrates; and as a consequence of all the circumstances referred to, extreme mental depression, interrupted only by occasional convulsive excitement in the dark and miserable masses. To all these popular conditions as remote causes, I would attribute those abnormal states of the vital powers in solids and fluids, which sprang more immediately, of course, out of extremely defective composition, distribution, and depuration of blood; and which, however caused, gave to the endemic and epidemic diseases of those times a tendency to maculæ, hæmorrhagic exudations, fœtid and otherwise foul and anomalous excretions, with asthenic delirium or typhomania, &c. &c.—in a word, to intense malignity of disease, humoral and nervous, such as we now rarely witness; and which, should it re-appear, would now, as then, unquestionably produce frightful mortalities and devastations. Now such causes of humoral vitiation and anomalous vital conditions, and in general of inferior tenacity of life, no longer existing in their former intensity to complicate morbid states and mask diseases, it results that disease consists at present more in simple defect or excess, material or dynamic, and less in perversion or qualitative changes; and that therefore the business of treatment is more simple, and owing to its greater simplicity in great part, more efficient. Also, that from the mitigation of many causes of malignant disease, and the apparently superior average tenacity of life, the chronic class of disorders preponderates over the pestilential and acute; or in other words, those diseases recede in which the frailty of the subject, and the intensity of the morbific agencies, leave little room for curative skill, from the rapidity of their march, or the mysterious complexity of their causes and symptoms; while those other diseases advance in relative frequency and importance, whose slow progress and protracted duration

give time and opportunity for the safe and efficient employment of the resources of a Therapeia, as yet very limited in its means, to a great extent merely tentative in its nature; and in its daily exercise restricted by various considerations and circumstances, moral, physical, and prudential.

Special occasion and objects of the inquiry.—Under such impressions, whether erroneous or not, it has long seemed advisable, that the especial attention of such pathologists as are practitioners, and have opportunity, should be fixed on the vast and complicated apparatus whose end is *nutrition*; or the due preparation, purification, and distribution of nutrient fluids; and that to increase the probabilities of success, it would always be advisable to use, where practicable, *instrumental aids* to observation, and to adopt simple but accurate methods of recording the results; and thus to depend as little as possible on *conjecture* in observing, or on *memory* in recording. And so far as opportunity permitted, I have myself acted on those views, more especially as regards the great organ of nutrimentary distribution—the heart; and although I have by no means completed the limited inquiries, which, as not beyond my powers or opportunities, I have conceived myself warranted in attempting, yet I have, I imagine, already arrived at some results, not so far as I know, generally known, and yet of some interest—results which I propose to make the subject of the present lectures.

Importance of accurate knowledge of heart disease.—I have already stated that the especial object of my inquiries has been the pathology and pathological relations of the heart. My attention has not, indeed, been limited to that organ; on the contrary, it has been endeavoured to embrace in the inquiry all the viscera; but the heart has been the especial object of observation, and that for various reasons, but principally this, that I found disease of the heart, simple enlargement with thickening usually, in a large proportion of fatal cases of adults, whatever might be the name or supposed proximate causes of the diseases preceding death; so that, in male adults, *morbus cordis* seemed, in official practice at least, to occur more frequently than phthisis, which I had previously believed to be, at all ages above puberty, the principal fatal disease of these climates. And further, this contributed to fix my attention on the heart, viz., that in almost all the cases of asthmatic and chronic catarrhal or bronchitic disease that I had met with in persons of mature years, and especially in males, in eight and even nine cases out of every ten, I thought I met with hypertrophy of at least the left ventricle of the heart. Such perpetually recurring disease of the heart, either as a direct cause of death or as a complication of fatal diseases of other organs, seemed to warrant the opinion that to the heart might be attributable a large share of the fatality of many diseases and cases of disease in which no suspicion had been entertained of any defect in that capital organ. I had also had several opportunities of feeling the want of some standard or measure of health more accurate than the *closed fist* of the subject, which seems to have satisfied the illustrious Laennec; having on several occasions known hearts to prove decidedly hypertrophous by instrumental measurement, which had by able anatomists and highly-informed pathologists been previously pronounced normal, more especially in cases where death had supervened quickly on acute complaints, and where no important lesion was observable of the valves, pericardium, or aorta.

Definition of health in the heart by means of instrumental measurement.—The facts just alluded to, and more especially those last referred to, suggested the propriety of endeavouring to define, as precisely as might be, the limits of health and disease with respect to that organ, and especially to fix as accurately as I could the limits of healthy development and nutrition, as opposed to the hypertrophous state on the one hand, and the atrophous on the other. The object just stated I selected for inquiry before others, for several reasons, especially these two—that it was *easier*, as admitting more fully of instrumental assistance; and that it was *more necessary*, as being peculiarly useful in the study of hypertrophy, which I soon convinced myself was the principal element in importance, because the most frequent of occurrence, and the most extensively mischievous element in diseases of the heart. For the purpose just stated, then, measurement by bulk and linear measurement, as well as measurement by weight, presented themselves for adoption. But I found linear measurement too difficult and uncertain, from variability and irregularity of shape, from organic flaccidity, and other mechanical circumstances; and measurement by bulk, in a fluid contained in a graduated vessel, I found to be but partially applicable, owing especially to the wide distance between the extremes—between enlarged adult livers, emphysematous lungs, &c., on the one hand, and infant hearts, cerebella, spleens, &c., on the other; so that I found measurement by weight in air the only method generally applicable and convenient, and the only mode at all quite obviously by which the ratio of any viscus of

the whole person could with any approach to precision be attempted to be ascertained. This mode I likewise found abundantly *accurate*, as well as easy in practice. I accordingly extended to the heart the practice of measurement by weight previously applied to the encephalon by several observers, and on the largest scale and with most success by my friend Dr. Sims, as evidenced in his instructive paper published in the 19th volume of the Medico-Chirurgical Transactions; applied to some extent also in France to the heart itself by Bouillaud, as previously by Senac, and contemporaneously by Lobstein, Cruveilhier, &c.; also by Meckel, in Germany, and some others.

This method of measurement by weight, for reasons hereafter to be stated, was subsequently extended to all the viscera where practicable, and as often as possible was made to include the person of the subject likewise. To measurement by weight were added, in many instances, measurements also by bulk, with the view of determining the density of the viscera in various conditions; and these observations have been now long enough in progress to admit of inferences, founded in several instances on facts, occurring in several hundred subjects.

Early last year I had the honour of laying before the Royal Medical and Chirurgical Society a paper communicating some of the conclusions first arrived at, and founded on observations embracing between two and three hundred subjects. Since that time, owing to the kindness of several friends in great part, I have been able to enlarge my facts by new accessions to double the number at my disposal last year, and am thus in a position to repeat, with additional evidence, certain statements of that paper, and to mention other additional results not known to me, or (for reasons not necessary to mention here) not stated on that occasion.

The first step then appeared to be, to determine, with as much precision as my materials would admit, the normal dimensions of the heart, its normal bulk, weight, and density.

From want of precise data on this point, it appeared that mistakes had been made from time to time by pathologists and practitioners, which might otherwise have been avoided. Diseases, and symptoms properly referrible to disease of the heart, have often been ascribed to pulmonic affections, or to chronic disorders of the liver, or spleen, or other abdominal viscus; and in cases of complicated disease in not a few instances, disease of the heart, not including valvular defect or pericarditis, has been entirely overlooked, and the whole of the morbid actions referred to other organs, themselves probably in many cases not principals in the morbid causation, but merely secondarily affected, and in consequence of the disease at the centre of circulation. Of this there will hereafter be proof offered, consisting of facts and inferences that have convinced my own mind at least that much as has been written on diseases of the heart, it still remains to be determined with precision what constitutes disease of the heart—what are the anatomical and physical evidences of the most important species of disease of the heart—or, in other words, what limits divide hypertrophy from normal nutrition of that organ.

Normal absolute weight and volume of the heart.—With a view, then, to determine the normal dimensions, &c. of the heart, nearly four hundred hearts were examined and weighed, and distributed into groups in tabular forms, distinguishing age and sex, and placing all cases of *morbus cordis* and of *phthisis* apart from the other cases, for reasons which, if not obvious, will presently appear. It is proper to mention that the diseases of almost all the subjects of observation are known, and that in most cases pretty full notes exist of the morbid appearances, and are in my own possession.

The first part of the table contains the weights of between eighty and ninety hearts of males above puberty, who died of various diseases, not morbus cordis or phthisis, and distributed into groups or columns, according to age, viz.—

15 to 30 years of age.
30 to 50 years of age.
50 to 70 years of age.
70 to 100 years of age.

The second part of the table contains like particulars regarding more than one hundred female subjects, dead of other diseases than pulmonary consumption, or disease of the heart, and distributed in similar groups or columns, according to age.

Now, according to the first part of Table 1, containing normal hearts of males, we have as our average for the years 15 to 30 (68 oz. 10 dr. ÷ 8 =) $8\frac{1}{2}$ oz., which is nearly the average assumed in the paper above alluded to for the whole of life. For the ages 30 to 50 we have for the average weight (228 ÷ =) $9\frac{1}{2}$ oz.; and if we exclude from the total of which the quotient is $9\frac{1}{2}$ such hearts as exceeded 11 oz. in weight, which is an ounce above the ordinary limit of health assigned by Senac, Lobstein, Meckel, and Bouillaud, and much more above the normal standard according to Cruveilhier, we shall then have (228 − 93 ÷ 16 =) 8·85 or $8\frac{7}{8}$ oz.

Then in the next interval of age we

TABLE I.

Table of *Hearts* divided into four sections according to age, namely, 15 to 30 years of age, 30 to 50, 50 to 70, and 70 to 100, distinguishing the sexes, and giving total number of observations, total weights, and average weight for each interval of age and each sex, and distinguishing phthisis and morbus cordis from all other diseases and cases; and adding for each class the average for life above puberty.

		15 to 30.			30 to 50.			50 to 70.			70 to 100.				
		No. of Obs.	Total Weight.	Average.	No. of Obs.	Total Weight.	Average.	No. of Obs.	Total Weight.	Average.	No. of Obs.	Total Weight.	Average.	Total of Obs.	Average for Life above 15.
Varia exclusive of phthisis, &c.	* *Gross* Totals and averages	8	oz. dr. 68 10	$8\frac{1}{2}$	24	oz. dr. 228 2	$9\frac{1}{2}$	33	oz. dr. 336 2	$10\frac{1}{6}$	15	oz. dr. $156\frac{1}{4}$	$10\frac{1}{2}$	80	9·45
Varia exclusive as before	† *Net* Totals and averages	8	68 10	$8\frac{1}{2}$	16	135	$8\frac{1}{2}$	23	215	$9\frac{1}{3}$	11	107	$9\frac{3}{4}$	58	9·00
Phthisis, averages, &c.	Males.	11	$86\frac{3}{4}$	8	19	$181\frac{3}{4}$	$9\frac{1}{2}$	17	$160\frac{3}{4}$	$9\frac{2}{5}$	3	$28\frac{1}{4}$	$9\frac{1}{3}$	50	9·06
Morb. chron. cordis	Males.	5	$72\frac{3}{4}$	$14\frac{1}{2}$	24	$409\frac{1}{4}$	17	34	$513\frac{3}{4}$	15	13	195	15	76	15·33
Varia exclusive of phthisis, &c.	* *Gross* Totals and averages	20	163	$8\frac{1}{7}$	31	$266\frac{3}{4}$	$8\frac{3}{5}$	30	245	$8\frac{1}{6}$	24	$196\frac{1}{4}$	$8\frac{1}{6}$	105	8·26
Varia exclusive as before	† *Net* Totals and averages	20	163	$8\frac{1}{7}$	28	233	$8\frac{1}{3}$	27	212	8	23	184	8	98	8·12
Phthisis	Females.	8	$56\frac{1}{4}$	7	8	70	$8\frac{3}{4}$	9	69 10	$7\frac{2}{3}$	2	14 10	$7\frac{1}{3}$	27	7·47
Morb. chron. cordis	Females.	5	$61\frac{3}{4}$	$12\frac{1}{5}$	8	99	$12\frac{3}{8}$	16	234 14	$14\frac{1}{2}$	14	175	$12\frac{1}{2}$	43	12·75

* Gross totals include *all cases* not known to be diseased during life, and not *obviously* altered in structure.

† Net totals include *no hearts* amounting to 11 oz. avoirdupois, or upwards.

Note.—All the weights avoirdupois.

Grand Total of Observations ..... { Males ... 206 / Females 175 } = 381

N.B.—The observations made all within the same period, and without selection of cases.

have 33 hearts giving an average (336 ÷ 33 =) 10·17 oz. or $10\frac{1}{6}$ oz.; and if we exclude from the division 10 hearts of 11 oz. and upwards, as in the last case, we have a net average of 9·33 or $9\frac{1}{3}$ oz.

In the fourth interval of age, viz. 70 and upwards, there are 15 hearts giving a gross average of $10\frac{1}{2}$ oz.; and excluding 4 of 11 oz. and upwards each, we have a net average of 9·75 or $9\frac{3}{4}$ oz., and for the whole four columns we have a gross average, of 9·45 and a net average of 9 oz. for the life of males above 15 years of age.

In like manner in the second part of the table, we have for females the following averages, viz.:—

15 to 30 yrs. of age gives 8·14 or $8\frac{1}{7}$ oz.
30 to 50 ——— gives 8·33 or $8\frac{1}{3}$ oz.
50 to 70 ——— gives 8 oz. nearly.
70 to 100 ——— gives 8 oz.

And the whole four columns give an average for life of 8·26 or $8\frac{1}{4}$ gross average, and 8·12 or $8\frac{1}{8}$ oz. net average, from puberty to extreme age.

Comparison of estimates.—Now, with respect to those averages, I may remark, before proceeding farther, that they agree very well with the estimates of Senac, Meckel, Lobstein, and Bouillaud, which were deduced from much smaller numbers apparently, and which are:—Senac, 8 to 10 oz.; for Lobstein, 9 or 10 oz.; for Meckel, about 10 oz.; and for Bouillaud, for both sexes, $8\frac{1}{2}$ oz. nearly avoirdupois: and there is much reason to believe, that if they had attended to distinction of age and sex sufficiently, which it is clear none of them has done, their estimates would still more closely correspond with those of the present tables. Cruveilhier's estimate, I must, however, observe, agrees very indifferently with mine; but I may add, that it does not, so far as I know, appear that he drew his averages from a total sufficiently large, and there is good reason to think that he too paid inadequate attention to the distinctions of sex and age. His estimate for the healthy adult heart is 6 to 7 ounces, an estimate too low according to my observation for any age above 10 or 12 years, or for any class of patients except females dead of cancer.

Advantages of the mode of tabulation adopted.—I may be permitted to remark, as to the tables, in explanation of their construction, that I have had a two-fold end in view in arranging the facts in groups, according to age and sex. One reason was this: supposing the sum total of cases, without distinction of sex or age, to be large enough to yield an average pretty nearly approximating to the truth, it might, on arithmetical grounds, be expected that the general average would pretty nearly agree with averages obtained from any large sections into which the general total might be divided, and that any excess or defect in any sectional average as compared with the general average, would be found to be compensated for by an opposite error in some other section. And the division into sections, without diminishing the facility of obtaining a general average, promised this important additional advantage; that it would enable me to detect any marked deviation from the general average that might be produced by differences of age or sex. Accordingly both these advantages have been obtained by the mode of tabulation adopted.

Our absolute average weight then, exclusive of phthisis and morbus cordis, for the adult is, for the male 9 oz., and for the female nearly an ounce less, by this first and simplest method of attempting to define the limits of health, viz. totting up the separate measurements, and dividing by the number of hearts measured, and assuming the quotient as the normal average weight or measure.

Normal relative weight of the heart.—But it seemed desirable to obtain not only the absolute average weight of the heart, but also the ratio of its weight to those of other organs, and to that of the person. This has been attempted by several naturalists for the brain, and quite recently again by an illustrious German physiologist, and with some success; and though the peculiar dignity of the cerebral functions would give importance to any even partially successful attempt to elucidate their laws and causation, yet it appeared that the example of Tiedemann might, with advantage, be followed with respect to the heart also; and the attempt has accordingly been made.

Ratio of heart to lung.—Amongst the other viscera the lungs have been selected as the most intimately connected with the heart in function, and at the same time is, if the heart be not an exception, the most important instrument of the organic life. However, of all the organs, the spleen not excepted, the lungs are most variable in volume and density, and of course in weight, so that for want, unquestionably, of sufficient repetition, the success of this method of defining the limits of health in the heart has not yet been such as I should wish.

However, the results are as follows, dividing the cases into three classes as before, viz.—1. Varia, or various diseases, exclusive of phthisis and morbus cordis; 2, phthisis; and 3, morbus cordis; we have, as average ratios for the whole

of life above fifteen years, the following: For the first class we have,

For the male the ratio of ..... } :: 1 : 5·28, and
For the female .... :: 1 : 4.18

For the second class we have,

For the male the ratio of..... } :: 1 : 7·19, and
For the female ..... :: 1 : 4·60

For the third class, or morbus cordis, we have,

For the males ..... :: 1 : 3·79, and
For the females ..... :: 1 : 3·06

And for all the three classes, viz. 1, varia, or various diseases, excluding consumption and disease of the heart; 2, phthisis; and 3, morbus cordis, we have the general average,

For the male ..... :: 1 : 5· 42, and
For the female ..... :: 1 : 3· 95

Now if the normal weight of the lungs were known, it would seem that the knowledge of such ratios, supposing them correct, would help us in practice to determine the normality or abnormality of the heart's nutrition or dimensions; and if the ratios between the organs under consideration were deduced from a sufficiently large number of instances, it would certainly be available for that purpose. But for that end a very large number of observations indeed would be required, much larger than are at present at my disposal.

Normal ratio of the heart to the whole person after death.—To proceed, then, to the other mode of fixing the normal dimensions of the heart by *relative* measurement: the necessity of this method, in addition to ascertaining the *absolute* weight, is obvious from several considerations, but especially from this, that the heart varies in size, not strictly proportionally to, but still varies with the temperament, sex, stature, and conformation. The stature, especially, is well known to affect the dimensions of the heart, so that excluding mere fat, a large person always implies a large heart, and *vice versâ* a slender person, *cæteris paribus*, a small heart; and to remove this source of doubt there was no remedy but ascertaining the weights of both heart and person in a large number of cases of persons dead of known diseases. This attempt has been made, and the ratio ascertained as accurately as nearly two hundred observations would furnish the necessary data. These results are contained in the third table, or table exhibiting the ratio between the heart and person in one hundred and ninety-four observations, distinguishing sex and age, and distinguishing other diseases from phthisis and morbus cordis, and these latter also from each other.

TABLE II.

Table of Ratios of Hearts and Persons in 194 cases, viz. 108 males, and 86 females, giving number of observations, and distinguishing sexes, ages, and diseases, as in Table I.

	15 to 30.		30 to 50.		50 to 70.		70 to 100.		Totals of Obs.	Averages for Life.
	No. of Obs.	Ratio of Heart to Person.	No. of Obs.	Ratio.	No. of Obs.	Ratio.	No. of Obs.	Ratio.		
Male.		Heart. Person.		Heart. Person.		Heart. Person.		Heart. Person.		
Varia	9	as 1 to 164	8	as 1 to 150	18	as 1 to 161	7	as 1 to 155	42	as 1 to 158
Phthisis	6	1 160	11	1 146	8	1 154	3	1 136	28	1 149
Morb. cordis	3	1 115	11	1 121	20	1 119	4	1 124	38	1 120
Female.										
Varia	17	1 169	13	1 161	22	1 147	6	1 121	58	1 149
Phthisis	6	1 162	2	1 199	4	1 150			12	1 170
Morbus cordis	3	1 100	3	1 107	7	1 134	3	1 115	16	1 114

Averages for life for all diseases, and both sexes, and all ages { Males·· as 1 to 142 / Females as 1 to 144

According to this table, the ratio for all diseases, exclusive of phthisis and morbus cordis is, for males,

From 15 to 30 years of age : : 1 : 164
„ 30 to 50 ..... : : 1 : 150
„ 50 to 70 ..... : : 1 : 161
„ 70, and upwards .. : : 1 : 155

For females in like manner, the ratios of healthy hearts to their persons are,

From 15 to 30 years of age : : 1 : 169
„ 30 to 50 ..... : : 1 : 161
„ 50 to 70 ..... : : 1 : 147
„ 70, and upwards .. : : 1 . 121

And the general averages for each sex of this class, or healthy hearts, are, for life, above 15 years, for males : : 1 : 158, and for females ..... : : 1 : 149.

Now, the accuracy of these approximations to the true ratios has been tested amongst other methods, by comparing them with the ratios deduced from two other classes of cases ; one remarkable for the nearly uniform increase of weight on the side of the heart; and the other equally remarkable, in general opinion, for invariable loss of substance and diminution of weight and bulk in the person. I mean, of course, morbus cordis and phthisis; and the discrepancies between the results are just such, I think, as I should expect, assuming the ratios found for the normal heart to be nearly correct. They are as follows:—In every one of the four intervals of age, on the male side, we find the heart bear a smaller proportion to the person in the class *Varia*, or that of normal hearts, than in either of the others; while in the class *Morbus Cordis* we find, as might be anticipated, the ratio of the heart to the person greater than in any of the others; and the class *Phthisis* occupies, in this respect, a middle place, the heart in that class being larger with respect to the person that in the class *Varia*, and less very much than in the class *Morbus Cordis*. In the class Varia, the male heart constituted 1-158th of the person; in the class Phthisis, 1-149th, and in the class Morbus Cordis, 1-120th. In the female division, again, the ratio is most favourable to the heart in morbus cordis, and least favourable in the class Varia; while at least in the first column which contains the largest number of observations, the class Phthisis occupies, as with the males, the middle place in the ratio of the heart to the person.

Before concluding the subject of the ratio of the heart to the person, let me add the following fact in confirmation of the approximative correctness of the ratios stated as obtained in the case of adults. I have had opportunity of ascertaining the absolute and relative weights of the heart and person in 23 cases of persons under puberty; the majority under 5 years of age, and nearly equally divided between the sexes, and I find the average for the males to be ..... : : 1 : 146, and for the females..... : : 1 : 153, and the common average ratios : : 1 : 149. Now, if we consider the greater activity of the heart in early life, we shall see at once a final cause or reason why the heart should bear a higher ratio to the person than in adult life; accordingly in infancy the organ is relatively weightier than afterwards, until decrepitude: and for two especial causes, viz. first, that it is rather thicker, comparatively, in its walls, especially on the right side; and, secondly, that it is all muscle, and contains no fat whatever. The ratio of 1-150th part of the person may be considered, therefore, as an agreement with the post-mortem ratio for adults of ordinary stature and corpulency, as close as could reasonably be looked for, and a confirmation of no small weight of the approximative accuracy of that ratio.

Let me add, that this ratio differs materially from that given in Meckel's Handbuch, which is, : : 1 : 200 for the adult, and.....: : 1 : 120 for infants and children; but Meckel has not given particular observations, nor, indeed, particulars of any kind, except those just quoted, and probably had not at his disposal a sufficient number of measurements; and further, very possibly paid insufficient attention to differences of sex, age, disease, &c.

Normal bulk of the heart.—Then as to the results of measurement by bulk. My observations under this head amount to somewhere about fifty, including all ages and diseases, and both sexes; and are only so few from finding the difference between the measurements in air (weight) and water (bulk), at all ages, in all diseases, and in both sexes, so inconsiderable that, even in the largest hearts, the former scarcely ever fell more than an ounce short of the latter; so that in every case the weight in air could be inferred from the bulk in ounces of water, and the bulk from the weight, to within a few drachms at the most, with much confidence.

And density.—With respect to the density, likewise, it may be anticipated, from what I have just said, that though I have found considerable differences between individuals, yet there has been no constant difference between any two classes. The density was examined by two different methods; the one easier, but rather less accurate, perhaps—viz. by comparing the weight in air with the bulk in minims of water, by means of a graduated vessel; the other by the usual method of obtaining specific weights. The results obtained by those two methods agree in the general conclu-

sions to which they lead; for though they differ from each other in every case, yet the excess is always on the same side, and of the same proportional amount, viz. about 2 per cent. By the former method, the density varied from 1050 to 1062; by the latter, the limits of variation were 1030 and 1040·5; and in neither case was any striking difference detected in connexion with morbus cordis, as compared with other diseases. From both sets of observations made by different methods, and on different subjects, and agreeing in placing the density of the normal heart rather above than below that of the diseased heart, it would seem to follow that the predominant tendency in excessive nutrition of the heart is to what may be called eccentric development; that its tissues grow in bulk rather than in density; and that the diseased heart is ordinarily more voluminous for its weight, and less solid for its consistency, than the normal heart. But additional observations are still necessary to warrant a confident opinion on the subject: of this I am fully aware, and it is my intention to repeat my trials as opportunity shall offer; but I am disposed to think that they are not extremely wide of the true proportions.

Summary.—The normal heart, then, to sum up the whole of my observations together, may be assumed to average, for the whole of life above puberty, about 9 oz. in absolute weight, and 8½ in bulk, for the male, and 8 oz. or a little more in weight, and 7½ oz. or a little more in bulk, for the female; and to bear after death to the person, for the male, the rates of about 1 to 160; and for the female, of about 1 to 150. So that a male adult heart, considerably exceeding 9 oz. in weight, but not considerably exceeding the 160th part of the whole subject, might, for a person of the working classes, and of ordinary fatness, but of much muscularity, be held to be normal; while a heart of like absolute dimensions, but occurring in a subject of average stature and muscularity, would justly be considered hypertrophous, although, owing to general obesity or œdema, it should not exceed, or should even fall short of, the 160th part of the weight of the person. And this observation applies, *mutatis mutandis*, to the female; in whom, I think, the extremes of obesity and leanness more frequently occur than in the male; with respect to whom, therefore, this qualification of our rule of measurement is even more necessary than with regard to the other sex. The female stature and muscular developments vary considerably less than the male, and the limits of normal nutrition and volume of the heart are consequently narrower; so that any female heart greatly exceeding 8 oz. in weight or volume, may *ipso facto* be suspected of hypertrophy, whatever may be its apparent anatomical condition, and but few instances indeed, if any, will be met with, I apprehend, of female hearts exceeding 9 oz. in volume, or 9½ in weight, in the persons of individuals of ordinary stature and conformation, that had been quite free from pectoral disease during their lives.

Influence of age on the development of the heart.—In the course of some observations upon the mode of construction used in the table of the heart, one advantage expected from the plan adopted was stated to be, that it would render evident, without special search, any difference in the nutrition and development of the organ that might depend on age or sex. In fact, in examining my papers, in preparing the communication above alluded to, as read before the Royal Medical and Chirurgical Society last year, I found reason to conclude that the heart increased in weight and volume with increase of years; and this conclusion was stated to the meeting before which the paper was read. From the first volume of the Transactions of the Medical Society of Observation of Paris, it appeared that Dr. Bizot, then of Paris, had previously arrived at that conclusion by a method quite different from mine, viz. by linear measurement alone. Dr. Bizot's conclusions were founded on a total of observations of 150, or more, and mine on a total of rather less; but the agreement in result, of two inquirers proceeding by methods so different, was striking, and their common conclusion resting on two different and independent lines of evidence and sets of observations, laid claim to a high degree of probability. However, in the numerical method, at least, if not in all methods of interrogating nature, multiplication of instances is every thing; and when preparing these lectures, I of course endeavoured to turn to account the additional observations then at my disposal, for the confirmation of that conclusion if true, or its correction if erroneous. The result is, I think, decidedly in favour of Dr. Bizot's conclusion and my own, of which my hearers will now be able to judge.

The force of nutrition in the heart increases as life advances, proved by increasing average weight.—It has been already stated, in the account of the first table, that the average for the years 15 to 30, in the male was 8½ oz. in weight; for the years 30 to 50, the net average was still 8½ oz.; from 50 to 70, the net average rose to 9½ oz.; and above 70, to 9¾ oz. For the female the following averages were obtained:—

15 to 30 years	8$\frac{1}{7}$ oz.
30 to 50 „	8$\frac{1}{3}$
50 to 70 „ .....(nearly)	8
70 and upwards	8

Now in both sexes there is an increase, though not of equal amount. The absolute increase of weight in the male heart, from puberty to extreme age, is near 13 per cent., or about 1-8th. In the female there would appear to be, after 50 years, no absolute increase, but rather a diminution; not amounting, however, to more than about 4 per cent.: so that the tendency to increase is not apparent, judging by the absolute weight, except in the male sex. But if we take the table of hearts and persons, and examine the ratios at different ages, we shall find the female heart at least relatively increasing, from the first interval of age up to the end of life. The ratio of the heart to the person, is, in the female between 15 and 30 years of age, :: 1 : 169; in the next interval, it has changed to :: 1 : 161; in the third interval, it has advanced to :: 1 : 147; and in the fourth interval of extreme age, it has attained to the ratio of : : 1 : 121. Whereas, in the male, in whom, judging by the absolute weight, the increase with years was obvious, there is, in the table of hearts and persons, a much less striking and regular gradation and increase. The ratio for the first interval is, for the male, : : 1 : 164; the ratio of the second interval is, : : 1 : 150; for the third we have : : 1 : 161; and in extreme age, the ratio : : 1 : 155. For the female, therefore, there is on the whole of life above 15 years, an increase of the ratio borne by the heart of nearly 29 per cent.: so that on the whole, the tendency in the nutrient force to sustain itself in the heart of the female, amid general wasting, and to increase its efforts in that of the male with advance of years, seems clearly evinced by the facts stated, and the conclusion at which Dr. Bizot arrived by the method of linear measurement, is again confirmed by the results I have obtained by weighing the heart.

Proved likewise by increased frequency and hypertrophy.—Before passing on to the next topic, I would call attention to another fact already cursorily alluded to, and which, when properly understood, strengthens materially, I conceive, the opinion that in the male heart, at least, the nutrient forces are in no degree enfeebled by age, but are rather augmented. It is this. I mentioned two averages derived from different totals and divisors, when speaking of the table of absolute weight of the heart; the one table containing all hearts not obviously morbidly massive and weighty, and not known, or on strong grounds not believed to have exhibited, during life, sufficient signs of hypertrophy; the other containing no heart that materially exceeded those limits within which I have constantly found the healthy heart to fall. Having never, that I know of, met with an instance of a heart that had been functionally quite healthy, amounting in weight to 11 oz. even in males of ample size and stature, I have accordingly omitted, in the second or net table, all hearts reaching as high as 11 oz. Acting, then, on this rule, I have been obliged to omit, in the second or net table, altogether 22 male hearts. Now whether those larger hearts thus omitted in the net table as probably morbidly hypertrophous, and which had been included, I believe, in the gross total of healthy hearts only from imperfect knowledge of the health of the subjects, and whose excessive nutrition would probably, had opportunity offered, been made clear to me before death;—whether those hearts be considered morbid or not, their excess above the normal average evinces the tendency to augmented nutrition in the male of advancing years. The energy of any power can only be measured by its effects, and I can conceive no better method of measuring the energy of the *nisus nutritivus* in the heart, or any other organ, than by carefully measuring by weight the quantity of assimilated matter appropriated by, and embodied in, its structures. Assuming the correctness of this view, as it appears to me I may reasonably do, and reminding my hearers that hypertrophy is, physiologically speaking, but a modification of healthy nutrition, I think I am warranted in saying that the superior frequency of excessive nutrition in the heart, as compared with other organs, and more especially in advanced life, can only arise from a greater energy of nutrient action than is habitual in, and proper to, that organ; and that from such superior frequency of hypertrophy alone, might with much probability have been inferred, *à priori*, what Bizot's researches and my own seem to have proved experimentally—viz. the existence in the heart of a nutrient force peculiarly energetic, not declining through age, as in other organs, but sustained throughout life as in the female, and even growing as in the male with years, contrary to the law of nutrition in every other organ, and in the whole person.

And the modification of the law of nutrition just pointed out, I may remark, before passing to the next topic, suggests an additional restriction of the rule by which the presence and amount of hypertrophy should be determined, and requires that in the male, at least, some allowance must be made for the physiological influence of age over the development and mass of the heart; a restriction that may require attention in judging of cases in which the excess of volume or of weight, absolute or relative, is moderate or disputed.

REMARKS ON ANIMAL MAGNETISM.

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The subject which forms the heading to this article is, at the present moment, one of absorbing and increasing interest, and unusually calculated to receive from mankind an undue share both of neglect and attention.

It is highly adapted to enlist the credulous, and to ensnare the lovers of the marvellous, while the sceptic's hostility is at once roused by the singularity and inscrutable nature of the phenomena it presents to our notice. On the other hand, but few minds are capable of approaching the subject with that state of calm and dispassionate feeling which such researches more particularly demand; and the consequence is, that truth is with difficulty extricated from the mass of erroneous materials which quickly accumulate.

We are much mistaken if the history of animal magnetism will not hereafter be often cited, as a striking proof of the tendency of the human mind to hasty ill-digested induction, and many names deservedly high in reputation, will, we believe, in this instance at least, give proof of their fallibility, and of the lurking tendency to the failing to which we are alluding. Indeed, the history of science proves, that men of high attainment have not only run into extremes upon subjects of mere speculative interest, but have been equally opposed when opinion was susceptible of physical demonstration; and in proportion to the obscurities of the subject, to the difficulty of arriving at positive and incontrovertible facts, and of connecting these facts clearly and distinctly with their real cause, ought we to be cautious in arriving at a decision, and unwilling to bow to the authority of a name.

The real difficulties of accurate observation are too frequently underrated, and we are in consequence deluged with an ocean of ill-recorded and often erroneous materials, from the examination of which nothing solid or satisfactory can ever be constructed. We are too apt to suppose that every man may become an observer, while, on the contrary, but few are either physically or mentally adapted for the post, and the labours of but few have ever survived the trying scrutiny of time. Perhaps we are more perplexed by false facts than by false theories, because, were the former what they pretend to be, and in sufficient numbers, the latter could scarcely have originated; our mental constitution being such, that with materials properly selected, and sufficiently copious, but little difference of opinion would be found to exist. The truth is, that in almost every instance a preconceived opinion is the ground of our observing, and we are insensibly led to adjust our facts for its support and elucidation, admitting with reluctance what is either adverse or neutral. The hope, also, of discovery, and of adding to, rather than of additionally confirming, what we before were acquainted with, exerts a constant tendency to divert the attention from the more ordinary channels of explanation, and urges the investigator, either from this or from some less worthy motive, unfairly to insist on the speciality of the phenomena, to isolate them from other and better known facts, thus destroying their analogy, and reducing them to a form more in accordance with his theoretical prepossessions. We must never forget that the most visionary and hypothetical of writers is perpetually referring us to his facts; and the credibility of an author, especially in reference to a subject such as we are now considering, does not depend on his founding his opinions upon the result of positive experiment, but upon his powers of correct and unbiassed observation.

These remarks are peculiarly applicable to the now current question of Animal Magnetism, which, like phrenology, has become a subject of popular interest and discussion, and like this very partially understood science, has met with many false friends, many too credulous disciples, and many ignorant traducers.

The state of mind best fitted for the reception and investigation of truth, is, indeed, one of difficult and rare attainment. To have that deep and abiding conviction of our profound ignorance; of our being but on the threshold of knowledge; to feel that our faith should have no limits; that "impossibility" is a word which, in all subjects fairly open to investigation, it is presumption to employ, while at the same time we with-

hold our assent until the judgment is fully satisfied, requires long habits of mental and moral cultivation, which can only become national when ages of chastening experience shall have rolled over the heads of a people.

The progress of truth, as we have already remarked, is not only retarded by blind credulity and ignorant scepticism, but also by the timidly conscientious, who are scared by anticipated consequences—by the self-conceited, whose mental vision does not expand beyond the circle of their own puny acquirements—and by those who are perpetually urging on the advocates of new principles to their personal application.

The simple consideration of how many things, a few years since catalogued in our list of impossibilities, which are now actually accomplished, and so familiar that they too often fail to elicit the admiration they so well deserve; and the comparison of the little we really know with the immensity yet unknown, should indeed induce the mind to coincide with the candid and illustrious Andral, when he remarks, that "a fact may not be less true because it is not included in the category of circumstances, which the state of human science, or the extent of the limits of the human intellect, enables us to comprehend, and compels us to recognise."

It is with these feelings that we would desire to approach the subject of Animal Magnetism, asking for it from others no other favour than that of a calm and dispassionate investigation.

No one who has interested himself in the literature and practice of this singular science, can for a moment doubt that it has been the means of producing many extraordinary modifications of our bodily functions, under circumstances at once both novel and peculiar, and highly calculated to excite curiosity and stimulate inquiry. In a theoretical point of view the great question to decide is, to what principle are we to refer these results; what is their proximate cause? Are they the direct effect of a special influence which one human being is capable of transmitting to another? are they, in short, the consequence of a direct emanation?—or, lastly, are they to be referred simply to the novelty of the circumstances by which the magnetised is surrounded, and thus tend to establish, not the agency of a previously unknown fluid, but merely the singular susceptibility of the nervous system, under particular external conditions, to become irregular in its manifestations?

We are not, it must be observed, contesting the facts of magnetism, or attempting to explain them; but we shall endeavour in the few remarks we are about to make, to contemplate them apart from what all magnetizers have directly or indirectly implied, viz., the existence of a special influence or fluid, by which one individual, by the exercise of his volition, is capable, not only of modifying, but of actually imparting or exciting new and previously unknown functions in another.

If we can render improbable the fact of any thing being actually imparted, we divest animal magnetism of its more theoretical aspect; we prove the inapplicability of the name; we render superfluous a host of speculations and surmises; and reduce its phenomena within the limits of other facts, equally inscrutable perhaps, but with the existence of which we were previously familiar. By so doing, we are aware that to many minds the subject would lose its most attractive features; but the real interest of the investigation remains unimpaired, and from its cautious prosecution we may reasonably expect that some increase to our present knowledge will ultimately result.

The amount of evidence requisite for establishing particular conclusions varies with the nature of the latter; and the harmony and simplicity of nature's operations are such, that we ought always to be most unwilling to make new facts the stepping-stone to new principles; until, at least, long and patient examination has proved that on no other supposition could they be classified or explained.

What then, let us inquire, are the arguments which tend to invalidate the idea that there is a magnetic fluid; and that it can in any way be controlled by our volition, and be infused into the system of another?

And here again there are two points for our consideration; for we may either assume that we transmit a part of ourselves, that is, a part of our own conscious principle, be it what it may, and thus perform a partial transmigration into the system of another; or else that

we set in motion in the desired direction the active agent, but possess no control over its effects.

In opposition to the first supposition we would observe, that were it correct, the effects should in all instances correspond, both in kind and degree, to the will of the magnetizer, which no one will assert to be the case. It is indeed true, that the phenomena once induced, can be frequently renewed by a repetition of the same process, but their *first* production is wholly beyond our control; we can neither calculate their nature or their intensity, nor can we modify them according to our will and fancy. Each fresh patient is a new field for observation, and we are wholly ignorant until we try, of what may be the effects of the means we employ; a fact not reconcileable with the hypothesis, that the magnetizer's will is any way essential to the process. It is true that in certain cases we can calculate with some degree of probability on the occurrence of particular effects, as sleep for instance, but this arises wholly from experience, and not at all because we will these phenomena rather than others.

It should also be remembered that the particular state induced is often one which the magnetizer is incapable of experiencing in his own person, and the powers he is supposed to impart are powers which he does not possess; it is therefore manifestly absurd to attribute to the effect of the magnetizer's will what he can neither voluntarily experience himself, nor voluntarily excite in another.

We cannot dissever the effects from the cause, and to suppose that our will is the cause of phenomena of which we have not the slightest idea until after they are produced, and of the nature of which, when produced, we are wholly ignorant, is indeed to become the victims of theory, and to set aside all true principles of reasoning.

Were we acquainted with the mechanism of the nervous system, one might imagine it possible to influence its functions in a definite manner, supposing ourselves capable by an effort of the will to influence the system of another at all; but it is difficult to imagine how, in a state of entire ignorance as to how the nervous matter executes its mysterious functions, we can thus regulate its movements, and direct its operations, by the magic influence of our will.

If will could any where act with energy, it would be upon our own functions; and yet we find no self-magnetizing professors; while the individuals who are most susceptible of the magnetic influence are those whose nervous system is the most shattered—the least capable of healthy and vigorous action. Should, however, the individual become capable of spontaneously manifesting the phenomena which previously had only been produced by magnetic manipulation, it would only prove that the tendency of the system to irregular action was unusually great, and that the morbid symptoms were, in the first place, only accidentally excited. It may be objected, that we are equally ignorant of the machinery of our own nervous system, though its functions within certain limits are under our direct control; yet we again say, that to influence the system of another by any power of volition emanating from ourselves, does imply a knowledge of the instrument on which we act—an idea at once negatived by the unexpected nature of the results, and rendered additionally improbable by the consideration, that if we were in possession of such a power, why is it that we can only excite a series of morbid phenomena? Surely it would be far easier to stimulate powers habitually active than to impart new and unheard-of faculties—far easier to transfuse into another what we feel and understand, than to excite actions which we can neither imitate nor comprehend.

Another argument we might urge is, that the effects of mesmerism have constantly varied with the processes employed; and it is difficult to discover in the facts detailed by the great apostle of the science more than a very distant analogy with the description of magnetic phenomena by modern observers. Now if these had been constantly dependent upon a particular influence under the control of the magnetiser's will, such differences in the results could not have taken place. We therefore conclude that there are no grounds for supposing that the so-called magnetic effects are in any degree directly dependent on the exercise of our volition.

Let us now glance at the objections which render highly improbable the

existence of any fluid or magnetic influence whatever, capable of being transferred from one individual to another.

In the first place, a power which in certain cases is capable of inducing such wonderful effects, ought in no instance to be wholly inert; at least such is not the *modus operandi* of any known power, as electricity, galvanism, and mineral magnetism,—all of which act upon our frame in despite of any volition we may exert against them, and notwithstanding any constitutional peculiarities we may possess. Animal magnetism, on the contrary, is very nice and fastidious in the selection of its victims, and is, with very few exceptions, incapable of exciting even the most insignificant sensation in those who possess "a sound mind in a sound body."

We have been magnetised by Baron Du Potet himself, and two professed magnetisers, in succession, and though most desirous of being affected, and most predisposed at that period to believe that it was in their power to do so, we were yet unable to detect even the slightest sensation consequent upon their varied and persevering manipulations. Now had these individuals really been the sources of a power such as they in all sincerity supposed, is it consonant, we would ask, with all known analogy, that it could have been forcibly directed upon the writer's nervous system for nearly one hour and a half, and not have produced even a consciousness of its existence? Whatever constitutional peculiarities may characterize particular individuals, it is giving them far too wide a latitude to suppose them, in the great majority of instances, capable of rendering wholly inert an influence so powerful as animal magnetism is supposed to be. Such extremes of strength and feebleness nullify all analogy derived from the imponderable fluids, and in a process of reasoning we can only proceed from the known to the unknown.

The magnetiser himself ought always very sensibly to feel the loss of so important a fluid; and though this is asserted to be the case, yet we have never observed any effect beyond that of pure fatigue, necessarily attendant upon a very tiresome and protracted effort.

It has also been constantly observed that the magnetic effects are increased when a number of persons are acted upon at once—a fact adverse to the idea of a particular power residing in the magnetism (which could only be weakened by diffusion), but quite consistent with another, and, we believe, more rational explanation. A similar objection might be founded upon the fact, that an individual is affected in proportion to the frequency of the process, while the system in general becomes less and less sensitive to all other external influences.

The peculiar effects attributed to mesmerism have manifested themselves when the patient was led to suppose that the magnetizer was acting upon him; and they have not taken place when the process was really exerted, but the patient in complete ignorance of the fact. In proof of the former assertion, we refer to some of the cases reported by the French commissioners, and, among others, to Deslon's attempt to magnetize a tree in the garden of the celebrated Benjamin Franklin; while the total absence of magnetic phenomena so long as the patient was ignorant of his being acted upon, as well as their rapid appearance when he became aware of the fact, are amply confirmed by the same observers, and may be happily illustrated by the History of the Metallic Tractors. Towards the close of the last century, these simple cones of metal were invested both by the inventor and the public voice with, if possible, more than magnetic power; but Dr. Haygarth, of Bath, put their real virtue to the test in a manner which soon dissipated the illusion. He caused a variety of false tractors, but exactly resembling the true ones, to be secretly made, and found that the effects were wholly unconnected with the nature of the material employed, but entirely depended on the patients' impressions with regard to them; his false apparatus answering all the purposes of the real, or proving wholly inert if the individual became aware of the deception. The records of homœopathy furnish us with a second illustration of our subject. In the Hospital St. Antoine, where all possible precautions were employed to prevent deception, the effects from infinitessimal doses of perfectly inert substances were similar in all their essential features to

those following equal minute quantities of the most powerful drugs. Hysteria, natural somnambulism, extasis, catalepsy, epilepsy, and other peculiar though less easily named conditions of the nervous system, have at times spontaneously manifested all the more important and credible magnetic phenomena; and since, under these circumstances, no magnetic virtue can be appealed to as the cause, why should we have recourse to it for an explanation of similar facts, though under novel and unexpected circumstances?

We know that a whole ward has become hysterical from witnessing the paroxysms of one of its inmates, and yet we do not suppose the magnetic influence to have been the cause. The threat of a severe remedy has soon caused the disturbance to subside; but we do not attribute this quieting effect to any mysterious emanation from the medical attendant by whom the threat is pronounced. And, on the other hand, if it should be clearly proved that the practice of magnetism elicited phenomena different from any that had ever been observed in connexion with disease or any other known circumstances, we should still be far from having demonstrated the speciality of the influence, because, having varied the conditions surrounding an individual, we might naturally and *à priori* expect some novelty in the results.

The system responds in a special and inscrutable manner to external impressions, and the more the latter are diversified and peculiar, the more varied and peculiar are the phenomena induced; and no one can witness or execute the process of magnetism, without being convinced that it is highly calculated to produce new and singular sensations in the nervous system of the magnetized, and that it is well adapted to excite those latent tendencies to irregular functional manifestations to which certain individuls are particularly predisposed.

We cannot, even in many instances, rationally refer the effects to the often-mentioned but little understood causes of imagination and imitation, for we cannot imagine what we are wholly ignorant of, nor can we imitate what we have never seen; and the patient is not only often unconscious of the approaching changes in his functions, but does not even remember them after they have taken place.

The records of superstition furnish us with innumerable instances of the remarkable bodily effects consequent upon powerful and long-continued mental excitement, and we cannot limit the influence of similar causes upon the frame. Some have died from the opposite emotions of joy and grief; fear paralyzes one, while it wings another's flight; the heart's action is impeded by one impression, and excited by another; and to refer the phenomena of magnetism to an influence different from any that we acknowledge as present in the instances to which we are briefly alluding, is, we think, both unnecessary and unphilosophical.

It is evident, as we have already remarked, that the nervous system of particular persons is spontaneously capable of manifesting the very same phenomena which by many are regarded as the direct effect of magnetic influence; and since the power of the latter is almost entirely limited to those in whom the tendency to irregular action obtains, is it not far more consonant with sound observation to suppose that their morbid idiosyncrasies are elicited by the simple external conditions which the practice of magnetism involves, in the same way as they are occasionally excited by other circumstances and impressions, when no special fluid or emanation can for a moment be suspected?

Another objection may be founded upon the different means which the operator employs to dissipate the magnetic effects. They are, indeed, wholly irreconcileable with an emanation hypothesis; for what has once transmigrated from our own system, to play its pranks in that of another, unless, indeed, it be what we have already shewn it cannot be, a part of our own conscious principle, can no longer be under our control; and the idea of our willing it back again, and inducing its exit by means such as those we are alluding to, is an extortion upon our faith with which, we hope, few feel disposed to comply.

We are quite aware, that to all this it may be replied, that "arguments are of no value; facts are every thing." Let it, however, be remembered, we are at the present moment simply attacking the reasonableness of the popular theory, while we freely admit the reality and great interest of many of the phenomena; and that it is by no means a matter of indifference what

theory we embrace, because false facts are almost necessarily the result of a false theory. Animal magnetism did not originate in observation; facts were warped to supply hypothetical exigencies, or made to promote some still more sordid purposes, until such a mass of extravagant absurdity was boldly advanced in support of the science, that we can scarcely attempt to retrace its history with other feelings than those of pity and distrust. Scarcely any of its vaunted powers have ever stood the test of rigid and impartial scrutiny, and many of its most imposing narrations are indebted, for all that renders them marvellous, to the easy credulity and ardent zeal of the magnetiser, or have been the result of deep and well-sustained deception on the part of the magnetised. In other instances, we believe and hope that both parties were the victims of mutual delusion; and from what we have both seen and effected, we feel satisfied that there is nothing easier than to fancy ourselves the cause as well as the spectator of extraordinary effects.

As the result of our personal experience, we can only state, that the more we saw attempted by others, the less we believed in any such science as the term animal magnetism implies; and the more we practised the art, the less confidence did we feel that the singular effects produced resulted from any power inherent in ourselves.

Notwithstanding, however, all that we have advanced against many of the magnetic facts, and the whole of the magnetic theory, we still believe that the investigation is one of great interest, and that if pursued by cool-judging and impartial observers, it will ultimately extend our knowledge of the functions of the nervous system, and perhaps lead to some valuable therapeutical applications.

In the recent numbers of your journal are some valuable papers from the pen of Mr. Mayo. The facts which this distinguished observer recites are full of interest, but at present obscure in their nature: his deductions, however, though expressed as inferential, and advanced under shelter of an apparently cautious philosophizing, are strongly infected with the evil of hasty generalization; and we regret that they have not been held back until the evidence was far more voluminous and conclusive. By adopting the same plan, we should soon have as many laws as phenomena; and in the midst of such numerous half-supported inferences, it will, indeed, be most difficult to act the part of an unprejudiced observer.

Our remarks have far exceeded the limits we originally intended; perhaps they will not be considered as wholly irrelevant at the present period of metropolitan magnetic excitement.

Reading, May 19, 1838.

OBSERVATIONS

ARISING OUT OF THE

RESULTS OF AMPUTATION IN DIFFERENT COUNTRIES.

By Benjamin Phillips, Esq. F.R.S.

Surgeon to the St. Marylebone Infirmary.

To the Editor of the Medical Gazette.

Sir,

The accompanying paper was read before the Royal Medical and Chirurgical Society, at a meeting held Nov. 14th, 1837.

When the question of publication arose, some doubts were expressed as to the correctness of the returns upon which my statements were founded, and I was solicited to proceed further in the inquiry. Satisfied that, in so far as I was concerned, no error existed, and believing in the accuracy of the returns which I possessed, I still was willing to reconsider the subject; I therefore determined to test the correctness of my data by a renewed inquiry, and it is a comfort to me to feel that the evil was not exaggerated either in the returns or the conclusions.

The first means of satisfying myself, to which I resorted, was to make a tolerably diligent search through the pages of the Edinburgh Medical and Surgical Journal, the Medical Gazette, and Lancet;—the earlier years of the latter contain pretty ample London Hospital reports. From these sources I extracted 308 cases: the result of some of these might be stated as doubtful, but I have satisfied myself that the deaths amount to 76, or as nearly as may be $24\frac{3}{4}$ per cent.

I have also succeeded in procuring from certain hospital surgeons of London, private notes of the result of their practice during the last four years;

the cases amount to 107, the deaths to 28, or $26\frac{18}{107}$ per cent. For London, therefore, as might be expected, the average number of deaths, after amputation, is greater than for the provinces. I am, however, disposed to think, although I have not data complete enough to make the statement confidently, that the mortality has been increased in the metropolis during the last three or four years by at least 3 per cent, by accidents which have occurred on the Birmingham and Great Western Railway.

If those persons, who doubted the accuracy of the former returns, should still be sceptical, I am happy to say that I have permission to submit most of them for their inspection; but, for reasons already stated in the paper, it would be inconvenient to publish them.

I am, sir,
Your obedient servant,
B. PHILLIPS.

17, Wimpole Street,
May 18, 1838.

The object of the paper which I have the honour to lay before the Society this evening is, in the first place, to prove that the opinion commonly entertained, with respect to the mortality succeeding to amputation, is incorrect; in the second place, to exhibit the results of inquiries undertaken in France, Germany, America, and Great Britain, as to the mortality consequent upon the operation; and to compare these results with each other, for the purpose of ascertaining to what extent is justified the belief in the greater success of amputation in our own than in other countries: in the third place, to adduce evidence in proof, that the treatment commonly employed in certain cases of old standing disease, is less favourable in its results, and less in consonance with pathological principles, than that to which I shall allude in the progress of this inquiry. Before I proceed to the consideration of the several points to which I request your attention, I beg to offer my warmest thanks to those of my professional friends in this and other countries, who have, with so much readiness and kindness, furnished me with the greater part of the information upon which this paper is founded; at the same time I am bound to express my regret that the riches of our great hospitals are rendered so little available for inquiries like the present—that those noble institutions, which should be the storehouses of exact observation, made on a large scale, and from which accurate ideas should be disseminated throughout the land, are almost completely without the means of fulfilling this very important object.

Some persons are disposed to express doubts of the value of investigations like the present; I trust, however, that no one will, on that account, be deterred from pursuing them; I confess myself unable to comprehend the force of such objections. Had I required any additional evidence of their importance, it would have been amply afforded me in the course of the investigations necessary for the production of the present paper. It has happened, on several occasions, that the medical men to whom I have applied for the results of their individual experience, have at once said, "I very rarely lose a case after amputation;" and when they have referred to their own notes, or to the hospital records, where such a thing was practicable, they have been astonished at the extent of the mortality. These circumstances alone would be sufficient to show the fallacy of resting our belief on vague impressions, and the necessity of some more exact evidence upon which to found opinions. There are few persons who are not ready to admit the necessity of a considerable number of facts to enable us to come to a proper knowledge of disease, or of the effect of remedies:—now, is it better that this knowledge should be based upon such impressions as those I have described, than upon exact data? In estimating such matters men commonly say—so and so is the experience of ages; but the experience of ages can be of little value, unless it be reduced to some more definite form than is conveyed by such general expressions. Centuries pass away, and really scarcely any accession is made to our experience: the same class of words continue still to shew vague ideas where precision is so desirable. In fact, is it better that the species of vague statistical knowledge which composes most men's experience, be preserved — that, for instance, which enables a man to say this or that operation is unsuccessful—or that he should be enabled to say that two out of five, or any other proportion, are the victims of this disease, or that operation?

At the outset I had intended that a greater number of points should be embraced in this inquiry, but this intention has been defeated by the very incomplete returns which I have acquired; defective, however, as they proved to be, they are now so purged and arranged as to afford a firm basis for the conclusions which will be set forth.

It may be thought that there are many sources of fallacy in investigations like the present; that certain hospitals are placed under peculiar circumstances, and that the results furnished by them would not be a fair representation of the whole. It is true that there are many hospitals so situated, either permanently, or in particular periods, as to render it probable that the results of amputation would be very unfavourable. In certain manufacturing towns a large number of accidents are produced by machinery; in such cases the injury may not be limited to the limb which it may be thought prudent to amputate, and the death of the patient may be a consequence of the accident, and not of the amputation. Other hospitals may be peculiarly the receptacles of chronic disease: an improved system of treating such diseases may be employed there, and the number of amputations may be small; but from that very circumstance, from the operation being performed as a really last resource, the result of such amputations might be unfavourable.

I have, therefore, made such a selection of hospitals as would produce something like an antagonism in this respect, and would render the results shewn a fair representation of what actually occurs.

In certain of the hospitals included in our reports, the mortality after amputation has exceeded fifty-three per cent.; in several it has not exceeded twelve per cent.; in one of the number, out of twenty amputations, there has been only one fatal result. Against this variety, as producing a chance of error in the general result, I have, I believe, sufficiently provided in the selection of hospitals; but the result which I have mentioned has determined me to withhold the names of the several hospitals included in the returns, and this because the extremes may have been brought about by a variety of modifying circumstances, the force of which might with difficulty be estimated in an inquiry like the present; but these extremes are of course absolutely necessary, to acquire a mean. On a former occasion, when treating of the results of the ligature of arteries, I endeavoured to shew that the opinion commonly entertained by medical men with regard to the mortality after, or consequent upon, this operation, is decidedly incorrect. At this time I propose to shew that the mortality after amputation is very great —much greater than is usually believed; and I trust, on some future occasion, to be able to shew that there are very few operations so free from danger as to justify us in losing sight of that circumstance in coming to a decision upon the propriety of performing them; and in a large class of operations, in which all that is hoped for is a certain alleviation of suffering, this circumstance should constitute an important element in the decision of the question.

The amputations included in this inquiry are those of the arm and the forearm, the thigh and the leg. The whole of them have been performed within the last four years in civil hospitals and in the private practice of hospital surgeons. The gross number of cases is 640, and this number embraces all cases, acute, chronic, and the results of violence which have occurred in the practice of the persons by whom the returns have been furnished within the period I have named. Of these cases 490 are reported "cured," and 150 died, either in consequence of the operation, or the progress of the disease, to rescue the patient from which, recourse was had to the operation.

I apprehend that a large number of our professional brethren are unprepared for such a result; I have only met with very few who were at all sensible of the extent of the mortality which occurs.

Compared with lithotomy, amputation and the ligature of arteries are often, perhaps commonly, held to be unimportant operations; and yet the results shew a very great balance in favour of the success of lithotomy.

It is necessary, however, in connexion with this circumstance, that a fact should be borne in mind, by which these results will be modified. A large number of amputations are performed

under very unfavourable circumtances. Lithotomy, on the contrary, commonly admits of our choosing a time when the patient is best prepared for it. In other words, the mortality consequent upon lithotomy is often directly caused by the operation; in amputation, by the disease or injury.

I have now shewn that the mortality succeeding to amputation is very great —23 per cent. I shall therefore proceed to analyze the gross number, and exhibit the proportion furnished by the different countries implicated in the inquiry. They are as follow:—

	Cases.	Deaths.		Per cent.
France	203	.. 47	or	$23\frac{31}{203}$
Germany ..	109	.. 26		$23\frac{93}{109}$
America	95	.. 24		$25\frac{5}{19}$
Great Britain	233	.. 53		$22\frac{174}{233}$
	640	150		$23\frac{7}{16}$

Here is an average number of deaths, amounting to, as near as may be, $23\frac{1}{2}$ per cent. If the several countries be taken separately, we find that France is a fraction below this average; that Germany differs only to the amount of a fraction from France; that America only exceeds the average by a little more than 2 per cent.; and that Great Britain is a fraction below the average.

That these returns shew us to be a favoured nation, is true; but I apprehend not to the extent that we have been accustomed to believe. Whether we owe this pre-eminence, slight though it be, over the nations we have named, to our climate, the constitution of our people, or to our system of treatment, is a matter not easy to determine; at present we may state our belief, founded upon the results of great operations performed in France and Germany, that in as far as those countries are concerned, we are not justified in assuming that climate, mode of life, temperament, or other natural modifying agents, place us in a more privileged position, in as far as ability to resist disease or to bear up against the effects of operation is concerned, than the natives of those countries; but we are disposed to refer those beneficial results to the superiority of our after treatment.

There was no doubt a period in the annals of French surgery, and that not very distant, when the returns would have exhibited a result very different from the present. In the practice of the older French surgeons the habit of stuffing with "charpie" was carried to such an extent, that great constitutional disturbance was the consequence, and the patient too frequently sunk under that or the abscesses, the purulent absorption or phlebitis, which followed: but that system has been almost totally abandoned, and that of procuring immediate union, as practised by ourselves, is at present extensively adopted.

I am, however, bound to say that the returns shew that consecutive union judiciously obtained—the stuffing being banished, and a strip of simple dressing only interposed, as was the practice of Dupuytren—is in its results just as successful as union by first intention. It has still this drawback—the average time of healing the wound is greater, and the inconvenience of dressing more considerable, than when immediate union is practised; but it has also certain advantages in cases of hæmorrhage, which may go far to lessen the force of the circumstances I have named.

We now proceed to inquire, whether the treatment commonly, and in this country almost universally, employed after amputation in diseases, attended by proper suppuration, be fortunate in its results, and whether another method of treatment might not as a rule be advantageously adopted.

I have classed the observations which I possess, so as to shew on the one hand the cases in which amputation has been performed in acute diseases, or in consequence of injuries; and on the other, the cases in which it has been employed as a remedy for chronic diseases; but as chronic disease is a very general term, I will point out the diseases which I have included in it: they are diseased articulations, with considerable purulent discharges, necrosis, caries, and extensive old-standing suppurating surfaces.

In our own country, for a considerable number of years, the method of immediate union has been almost exclusively employed. No matter what may have been the disease, it has commonly been attempted.

The inconveniences of secondary union were doubtless considerable; so that the system of immediate union once adopted, should have been exclusively employed, was a result very much in conformity with the revolutions of all systems.

The idea of lessening pain by diminishing the number of dressings—of avoiding profuse suppuration—of rapidly healing a large wound, were powerful elements for the overthrow of the old method. At the same time, its brilliant successes in the practice of military surgeons, came so strongly in support of the new system, that the abandonment of the system of consecutive union in our own country, was, and still is, almost complete. In other countries, the merits of the new system were much less readily appreciated, and the prejudice which was raised against it was considerable. Men clung with pertinacity to the old system, disastrous as were its effects, and even up to the present moment many eminent surgeons employ it almost exclusively; modified, however, it is true, and a very large number employ it much more extensively than we do.

Indeed, from his Thesis sustained in 1824 (No. 109), Gensoul, the eminent surgeon of Lyons, did not appear to be aware that any difference of opinion existed on the subject, for he says, "that all practitioners are agreed to proscribe immediate union when amputation has been practised for the removal of an ulcer which has long furnished an abundant suppuration, and that all men of experience followed the precept, that it was necessary to preserve a suppuration which has long existed, that it may serve as an emunctory, until insensibly the economy may be weaned from the habit it has contracted."

Gensoul was, however, very much in error in believing that, even in his own country, this practice was universal; and certainly in other countries the converse of this position is the fact. In Germany the theory has been of late advocated, but in so general a way as to be of little use; and in practice the precept has certainly been little followed.

In the Encyclopädisches Wörterbuch, Band 2, art. Amputation, are some general remarks upon the subject, by Dr. Grossheim, but they are too vague for application: they reduce themselves simply to this dictum—that in scrofulous and rachitic cases, and old suppuration, we should not quickly heal the wound after amputation.

I might have reiterated these statements, and adduced cogent reasons to prove that these opinions were probably correct; but I have preferred a different and more conclusive course. And I beg attention to the results which I shall now proceed to lay before the Society, in proof of the opinion that there is a large and well-defined class of diseases in which, after amputation, union by the first intention cannot, as at present practised, be so successfully employed as that in which it is consecutively obtained.

This class of diseases I have already particularized; and of the 640 constituting the gross number on our tables, 213 cases are of this kind.

Of these cases, immediate union was attempted in 117; consecutive union in 96.

Of the 117 cases 88 only succeeded; the deaths amounted to 29.

Of the 96 cases, in which the treatment was by consecutive union, 76 succeeded; the deaths were 20.

Of these cases, Great Britain furnished 86; the other countries included in the observations, 127.

Of the 86 cases immediate union was attempted in 60; consecutive in 26, and with the following result:—Of the 60 cases there were 15 deaths; of the 26 there were 5 deaths.

Of the 127 cases, immediate union was attempted in 57 cases; consecutive in 70.

Of the 57 cases, 14 were unsuccessful, the patients died, and 43 succeeded. Of the 70 cases where consecutive union was employed, there were 15 deaths. The results, therefore, attendant upon the practice of immediate union, are a mortality amounting to 25 per cent.; upon consecutive union, of nearly 21 per cent.

And there is a singular uniformity attendant upon the results of these two modes of practice, as shewn by the returns furnished by our own and the other countries; and all are strongly confirmatory of the prudence of avoiding immediate union in this large and well-defined class of diseases.

As I trust this has been made sufficiently evident, it may now be asked, in what way is this increased fatality, which is attendant upon immediate union, to be explained? With respect to a portion of the cases the explanation is easy. They have been produced by phlebitis and purulent absorption; pus being found in the lungs, liver, and other situations.

The reason why, in such cases, the tendency to this termination is more frequent than in ordinary cases, is, I apprehend, because the disposition to purulent secretion in the diseased organ still continues; and because immediate union might prevent its evacuation, and so cause constitutional disturbance and absorption.

In a certain number of cases, "visceral congestion" is occasioned by the suppression of the accustomed secretion; diarrhœa supervenes, and the patient dies.

My information is not sufficiently precise to enable me to state the precise proportions of deaths which have been brought about by these several means: under the terms "visceral congestion, abdominal disturbance, and diarrhœa," I have nineteen. Neither am I able to state, with any thing like accuracy, what is the organ in which such disturbance is commonly manifested; but it is evident that the mucous membrane of the intestines, in such cases, is very frequently affected.

Having indicated the result, and being strongly impressed with its correctness—having adduced evidence which militates strongly against the practice of immediate union in the class of cases to which I have before particularly alluded—it may be asked whether I have any suggestion to make, by the adoption of which the inconvenience of the present practice may be lessened? If we compare broadly the sytems of treating amputated surfaces by immediate and consecutive union, the advantages of the former are so many and so obvious, the inconvenience of the latter so prominent and so frequently pointed out, that it would require that the inconveniences which I have shewn to exist should be reiterated until they become familiar; and that the correctness of the evidence should be universally admitted, before the majority of men's minds would be prepared for any change.

But although the profession would be disposed to resist any attempt, however dexterously made, to substitute generally the one mode for the other, yet I have too great a confidence in the discretion of my professional brethren to doubt that they would be prepared to admit that some modification in the existing system might be advantageously adopted. I am not an advocate for the substitution of consecutive for immediate union in any class of cases, but I am prepared to recommend a modification of the present method of treatment in a large class.

If the causes of the adverse results of immediate union have been correctly indicated, I apprehend that a means of remedying them will be naturally presented.

If it be a consequence of the sudden suppression of a secretion to which the economy has been long accustomed, we may fairly assume that if the secretion can be maintained during a certain variable period of time, by artificial means, we may employ the method of immediate union without any apprehension of those complications to which allusion has been made, and at the same time avoid those objections of which consecutive union cannot be relieved. Virtually, the proposition is carried into effect every day. An attempt is made to secure immediate union in such cases: it is unsuccessful; consecutive union obtains; the secretion is indefinitely prolonged, but the evils of consecutive union come into play.

My proposition is made under the belief that the evils to which I have referred may be removed, and the mortality to which I have alluded be lessened. There is nothing startling in it; it is occasionally employed in our own country, though without system, and without any general or definite application; it is sometimes, and more frequently, but in an equally unscientific manner, employed on the continent; among others by M. Maunoir, of Geneva.

It is stated in a few words: before the amputation is performed, establish in the vicinity of the point where amputation is to be practised an artificial suppurating surface, either by means of a seton or issue, and let the secretion be maintained for a term varying with the duration of the secretion at the diseased point.

I have no direct evidence of the efficacy of the proposed modification to lay before the Society, but I have set forth evidence to shew that the maintenance of suppuration, in a large number of cases, is a mode of treatment more in consonance with our knowledge of the laws by which the animal economy is regulated, and whose results are more favourable, than those in which such

suppuration is suddenly suppressed. I believe that the maintenance of the suppuration artificially will lessen the evils of immediate union, and render it unnecessary in these cases to resort to secondary union, whose advantages in any case are almost counterbalanced by great inconvenience.

REMARKS

ON THE

NATURE AND TREATMENT OF PUERPERAL FEVER.

To the Editor of the Medical Gazette.

Sir,

Should the following observations seem in your judgment calculated to throw any light on the subject of a disease, which, notwithstanding the investigations of our most eminent obstetricians, still remains the opprobrium of our art, you will by inserting them in one of your earliest publications, oblige, sir,

Your most obedient servant,

R. Yates Ackerley, M.R.C.S.

1, Great Homer Street,
23d May, 1838.

So great has always been the mortality resulting from puerperal fever, that it has been appropriately termed by a celebrated writer* "the puerperal plague." Notwithstanding the many publications on the subject, which, year after year, issue from the press, it will be found, I fear, on a fair retrospect of the results of our treatment, that very little has hitherto been gained. In truth, the great majority of cases of malignant epidemic puerperal fever present to us a sad memento of the inefficacy of our present practice—a dark history from the contemplation of which the mind shrinks, and the pages of which we would fain have blotted from our recollection. A repetition of instances, however, brings increased conviction, and the truth recurs in all its vivid reality.

The principal doctrines on the subject may be classed under two heads of diametrically opposite signification. These are the *inflammatory* and the *non-inflammatory*. Amongst the advocates of the former we have the names of Denman, Gordon, Armstrong, Hey, and Mackintosh; and of the latter, those of Walsh, Manning, White, Clarke, Hamilton, and Burns.

Upon this simple view of the present state of professional opinion, a very natural question arises:—How is it that men so distinguished for soundness of judgment, extensive experience, and acute investigation, entertain such opposite views respecting the pathology and treatment of this disease? This question I shall endeavour to answer. Preparatory to this, however, it will be necessary to give a brief outline of the cases which have recently come under my observation.

Case I.—Mrs. Dougdale, a stout, healthy, and rather plethoric woman, 22 years of age, was confined of her second child on the evening of the 4th of February last. Her labour was natural and easy, not exceeding six hours from the commencement. I visited her on the morning of the 5th, when every thing appeared to be going on favourably. During the night, however, she was attacked with rigor, not very severe, but returning slightly each time she moved in bed. On the 6th, about eight o'clock, A.M., I found her labouring under symptoms of severe fevere, face flushed, skin hot and dry, tongue slightly furred, pain in the head, abdomen tender in the whole of the lower part, the pain aggravated on pressure, or on attempting to take a full inspiration: pulse 130, with some degree of hardness. The bowels had been open once, from a dose of castor oil, which she had taken on the preceding day. Lochia, which had been very copious, were now scanty, but not suppressed. The secretion of milk was also checked; had great thirst, and anxious expression of countenance. Under these circumstances I bled her to the amount of eighteen or twenty ounces, but without effecting any improvement in the state of the pulse. An immediate dose of ten grains of calomel, and one of opium, was given, to be succeeded every three hours by a pill containing three grains of calomel, and one quarter-grain of opium. Warm fomentations to the abdomen, and a saline effervescing mixture *ad libitum*. 12, noon:—Pulse 150, small and compressible; pain in abdomen increased, and extending over the whole surface. Respiration hurried, occasional delirium, tongue loaded with

* Dr. Blundell—Lancet.

white fur, had vomited once, bowels freely opened, stools fœtid. Ordered a blister to be applied to the abdomen, and the medicines to be continued. 11, P.M. :—The symptoms had all become worse. The blister had only risen partially, tongue dry and brown, teeth covered with sordes; severe pain in the region of the diaphragm, respiration very short and difficult, and frequent vomiting. Considerable effusion had taken place into the cavity of the peritoneum; pulse very small and quick, so as scarcely to be counted; mercurial fœtor clearly perceptible. From this period to the time of her death, which occurred at three, P.M. on the following day, she sunk rapidly, notwithstanding the free administration of cordials and stimulants. The distension of the abdomen had increased till the size was equal to that displayed at the full period of gestation.

CASE II.—Mrs. Hargreaves, aged 27 years, of a delicate and scrofulous constitution, was confined on the morning of the 4th February of her second child. The labour was natural, as in the former case. On the following day she appeared very well, except that the lochia were scanty, and the after-pains rather severe. No tenderness on pressure of the abdomen; pulse 90, and bowels confined, for which she took a dose of castor oil.

On the 6th she was a little feverish and flushed at times; tongue white and furred; after-pains still troublesome, but did not complain of any pain on pressing the abdomen. She had experienced no distinct rigor, and the pulse was 104. During the night she became delirious occasionally, and on the following day decided symptoms of puerperal fever had set in, similar to those described in the former case. She died on the 9th, about 3 P.M.

This case is remarkable for the insidious manner in which the disease made its advances. The prostration of strength was so great from the commencement, that I did not think it advisable to take blood.

CASE III.—Mrs. Fletcher, aged 24 years, a strong and healthy woman, was confined of her second child on the 5th February. This case differed from the last in having a well-marked rigor, which came on the third day after delivery. She never became delirious, nor did she complain of much pain in the abdomen, except on pressure. The other symptoms ran the same course as in the former cases. Effusion took place, and she died on the 10th. This patient during the whole period of her illness complained of a sensation as though the bed upon which she lay was continually rising and falling with a gentle undulatory motion.

CASE IV. — Mrs. Perry, aged 23 years, of a sound constitution, and enjoying good health, was delivered of her second child on the evening of the 6th February. She was attacked with rigor on the afternoon of the 9th, and was visited the same evening. Her symptoms were similar to those of the first case, except that she became delirious almost immediately after the rigor. She was bled freely, and large doses of calomel and opium were administered every four hours. Hot bran poultices were also applied to the abdomen, and on the 11th she was convalescent.

CASE V.—Mrs. Green, aged 20, of very delicate constitution, was delivered of her first child on the morning of the 28th February. Owing to the low state of her health she had been advised not to suckle her child. She was attacked on the morning of the 2d March with severe pain in the abdomen, rigor, quick pulse, and all the symptoms of puerperal fever. The disease was very quick in its progress; effusion took place in the course of the afternoon, and she died about 12 o'clock the same night.

CASE VI.—The subject of this case was a Mrs. Lloyd. I had frequently attended her whilst labouring under severe attacks of hæmoptysis, which had broken down a constitution naturally strong. She was 38 years of age, and was confined of her second child on the 4th March. On the 6th I found her afflicted with all the symptoms of puerperal fever. She had been seized with shivering about 12 o'clock, A.M. Her pulse, which the day before had been natural, had now increased to 135. She was bled immediately, and again in six hours afterwards. Flannels soaked in hot spirit of turpentine were frequently applied to the abdomen, and the tenderness gradually abated. Some effusion had taken place on the 8th, which became absorbed as the system was brought under the influence of mercury.

CASE VII. — Mrs. Ashton was confined of her second child on the 20th March, and on the evening of the fol-

lowing day was attacked with puerperal fever. Being of strong constitution she was freely bled, and subjected to treatment similar to that described in the last case. She could not be considered out of danger until the 1st of April.

Thus, out of seven cases, there were four deaths and three recoveries—a fearful mortality; but not greater, I believe, than is usual when the disease assumes the malignant epidemic character.

Of these, the children of the parties named in Cases 1, 3, 4, and 5, died of erysipelas within a fortnight after birth, the disease attacking them in various parts of the body, and under different forms. In case No. 1, gangrenous erysipelas made its appearance simultaneously on the head, chest, and abdomen. In Nos. 3 and 5, the head and face were seized with the disease in the spreading cutaneous form; and in case No. 4, the thighs and abdomen were affected with phlegmonous erysipelas. The other children survived without having exhibited any symptoms of the disorder. I may observe, that at the period to which I have alluded, erysipelas was prevailing to so great an extent that it might, without impropriety, have been said to be epidemic. Numerous cases were met with, both in private practice and in some of our public institutions. Cases of puerperal fever were also encountered by other practitioners in the neighbourhood.

Now it will be found, if we compare the two diseases, erysipelas and puerperal fever, with each other, that there exists a striking similarity, not only in the constitutional symptoms, but also in their progress, termination, and pathology. Let us take a case of erysipelas of the head and face, for example. We have first some constitutional disturbance, with flying pains in different parts of the body. Then follows a rigor, of longer or shorter duration. As reaction takes place, we find an erysipelatous blush making its appearance on some part of the face, spreading rapidly along the cutaneous surface, until the whole head and face are involved in one mass of disease. This is accompanied with infiltration of serum and lymph into all the subcutaneous cellular tissue, particularly about the eyes, which become completely closed. In some cases this effusion takes place external to the cutis, and between it and the cuticle, in the form of vesicles or blisters. The pulse becomes small, thready, and very frequent (from 130 to 150 in the minute), with great debility and sinking of the vital powers. The disease may now begin to subside altogether; or, whilst the parts first attacked are improving, may continue its course unchecked along the mucous membrane of the nose, mouth, and ears, proceeding to the larynx and fauces, and occasioning, by its swelling, such difficulty of respiration and deglutition as to cause death. Or the disease may suddenly leave the surface, and, by what is termed metastasis, attack the meningeal membranes of the brain. This, in a very short time, terminates fatally, with all the symptoms of inflammation of the last-named organ. Nor is it unusual for the disease, when situated on the chest or abdomen, to be translated to the serous membranes lining those cavities. Cases of this description are always attended with the most imminent danger.

How exactly do many of the symptoms of puerperal fever coincide with those of erysipelas! We have the same predisposing causes, the same premonitory febrile symptoms, the same rigor, the same rapid spreading of the disease, the same tendency to effusion, with that quick and small pulse which is so peculiarly characteristic of the disorder. In addition, we have the same sinking of the vital powers, expedited only by the particular state of the system in puerperal patients, and the superior degree of organic vitality of the peritoneum over that of the skin. Again, puerperal fever, like that of erysipelas, sometimes arises epidemically, sometimes sporadically. The co-existence of these diseases at the same time, has been frequently noticed by practitioners, and the fact was particularly noticed a few years ago, in the Infirmary and Lying-in Hospital of Edinburgh

Dr. Warren *, in his late excellent work, makes some remarks which bear so exactly on the subject in question, that I shall extract them. The Doctor describes an operation on an unmarried female, for the removal of a scirrhous breast. The case, it appears, terminated fatally about three weeks afterwards, with all the symptoms of puerperal peritonitis. This enlightened physician proceeds as follows:—"The immediate

* On Tumors, p. 251.

consequences of the operation were in every respect favourable; the wound was in a healing state when she was seized with fever. The existence of erythema erysipelatosum in the hospital at the time, gives us reason to believe that this patient was affected and died with an erysipelatous inflammation of internal organs. No cause we are acquainted with can so well explain this sudden and unexpected termination; nor is such an occurrence new to me: I have seen numbers of patients perish a few days after operations, at the time that erysipelas prevailed in the hospital, without the slightest external erythema. In such instances I have been in the habit of stating to you that these patients died of erysipelas, as truly as if they had been covered with an erythematous eruption. The disease is constitutional; it may affect the skin, and generally does so; it may affect the internal organs without affecting the skin, and in such cases is most dangerous."

Thus we find that different structures of the human frame, as the skin, cellular tissue, mucous and serous membranes, &c., are all liable to the erysipelatous inflammation. We find it passing from one to the other by continuity of surface; we find it leaving one structure, and attacking another of a different kind, by metastasis; and further, we find, that, though it generally makes its appearance on the surface, yet it may commence in the internal organs, without the skin exhibiting the slightest indications of its presence. From these premises I think we may very fairly come to the following conclusions:—

1st. That the same morbid state of the atmosphere, the same poison, will give rise both to puerperal fever and erysipelas.

2ndly. That erysipelas may primarily attack various structures of the human frame, either internal or external.

3rdly. That the skin and cellular tissue (of which latter the serous membranes are only a modification) are the structures generally subject to the disease.

4thly. That the puerperal state is favourable to its attacking the peritoneal membrane instead of the surface. And

5thly. That puerperal fever and erysipelas are essentially one and the same disease.

To return to the question which I have undertaken to answer. Why are practitioners so much at variance as to the nature and treatment of this disease? It would appear that those of the first class, judging from the pathology of the disease, concluded at once that it was of an acutely inflammatory nature, and, consequently, that nothing but the most active depletion and strict antiphlogistic treatment could in the least check its progress. The other class, observing the meagre success which followed the heroic use of the lancet, ran into the opposite extreme. Avoiding the inflammatory doctrine, they were compelled to account for the disease by imagining *a peculiar action of the vessels combined with some debilitating poison, sui generis*, leaving us altogether in the dark as to what that action or debilitating poison really was. Pursuing an opposite mode of treatment from the others, it is probable that the supporters of the latter system obtained similarly successful results.

Now if we bear in mind the erysipelatous nature of the disease, I think we shall have no difficulty in accounting for the effects produced by the efforts of each class of practitioners. It is evident that a more uniform and successful plan of treatment may be reasonably expected. It is my fervent hope that the foregoing remarks may excite the attention of those medical men who have more ample opportunities than myself of investigating the subject; and if they should prove the means of saving but one of my fellow-beings from the jaws of this "pitiless disease," my object will be fully attained.

In conclusion, I would merely suggest the frequent and extensive use of hot spirit of turpentine and hot-water blisters to the surface of the abdomen in the early stage of the disease, immediately after bleeding or leeching. This plan was successfully adopted in my last two cases, with the view of exciting on the surface a disease resembling that which existed internally, on the same principle that in cases of gout attacking the internal organs similar remedies would be employed. And why should we despair of being able to accomplish our wishes, seeing that the skin is the texture most liable to be attacked, and that a sudden translation from one membrane to another is sometimes naturally effected?

PERFORATING ULCER OF THE STOMACH.

To the Editor of the Medical Gazette.

Sir,

I send you the inclosed case, which has no pretensions to a place in your pages for any other reason than that it is useful to be reminded of long-established truths.

Samuel H. Bibley, Surgeon.

4, Oddy's Row, Islington Green,
May 25, 1838,

Sarah Mitchell, a servant, ætat. 25, of the sanguineous temperament, was seized at two o'clock in the afternoon on Monday, May 21, with severe pain in the region of the stomach, and sickness, almost immediately after eating a small portion of lobster: the sickness did not amount to vomiting. Her friends gave her some gin and warm water two or three times without any relief; the pain became intolerable; a friend then administered forty drops of laudanum, but without any relief. Her mistress, who had been from home, now returned, and sent for medical advice. I saw her at half-past three o'clock, P.M., and found her partly undressed, writhing to-and-fro in bed; she sat up, and told me she was suffering great pain at her stomach, and between her shoulder-blades, high up; said she should die if not soon relieved; and that she had eaten but an exceedingly small portion of the fish. Her countenance was very much sunk, strongly reminding me of cholera; eyelids drooped; nose pinched up; no pain of the head; cold perspiration covered the surface; the tongue was remarkably clean; breathing oppressed; action of the heart feeble; pulse at the wrist slow, small, and feeble; some tenderness over all the abdominal region, more particularly near the stomach; had not passed any water lately; bowels had been moved freely the preceding evening; extremities cold.

℞ Tart. Antim. & Potass. gr. iij. Pulv. Ipecac. ℈j. Aquæ Menth. Pip. ʒj. M. ft. haustus statim sumendus. This was followed up with draughts of warm water.

7 o'clock, P.M.—She had vomited once or twice a very small portion of lobster, some half digested food, and warm water, altogether about a pint and a half; this had no smell of spirits or laudanum. Sickness had left her; bowels had not been moved; had passed no water; symptoms still the same, except that her pulse had risen to 80. She begged of those around her not to leave her: her feet were put in warm water as she complained of feeling very cold. This measure, and putting her into bed afterwards, afforded so much relief, that there was reason to hope that the worst was over; still her countenance was sunk; she said to me, "The pain between my shoulders is now a greater trouble than the pain at my stomach."

℞ Chlorid. Hydrarg. gr. iv. Ext. Colocynth Co. gr. vj. M. ft. pil. ij. horis 4tis. donec alvus responderet sumendæ cum 4ta. parte misturæ sequentis.

℞ Sulph. Magnes. ʒj. Infus. Sennæ Co. Decoct. Aloës Co. aa ℥ij. Aq. Menth. Pip. ʒj. M.

During the night the pain changed its character; it was like the griping from the action of medicine; she had no sleep; no motion; passed a little urine; no sickness; suffered much pain: countenance quite collapsed: in the forenoon I found her moribund; there was then for the first time a sensation to my hand of fluid in the abdominal cavity; she spoke to me, and raised herself in the bed; but in a minute or two she sunk down, and breathed her last. She was first seized at two, P.M. on Monday, and died about eleven, A.M. on Tuesday. She had had advice for some dyspeptic symptoms, and a slight rash over the neck and chest, about three months before this attack, which quite disappeared after taking a mild tonic and aperient, for four or five days. I could not learn any thing of her former history.

Autopsy.—No emaciation; abdomen tense; much gas escaped from peritoneal cavity; there was the slightest blush of peritoneal inflammation incipient on the anterior surface of a small portion of the ilium; the stomach was adherent to the surrounding viscera in many places: for the most part the adhesions were old; some apparently not many days' standing: in anterior and superior surface we found a large ulcer, big enough to admit one's thumb, with hard and thick edges, extremely abrupt and circular, internally rounded and irregular; the peritoneal coat being the last to give way.

The mucous lining was not even vascular, nor yet the peritoneal at any part.

On the posterior wall was another ulcer, of nearly similar dimensions; the mucous membrane had ulcerated, and there was a *slough* somewhat strongly attached underneath.

The gall-bladder was full; two quarts of fluid were found in the peritoneal cavity; no other material disease was detected in any other viscus.

Putting the post-mortem out of sight, the principal features of this unfortunate case were, that the whole system was collapsed from the very commencement; that the bowels were obstinately constipated; that there was no vomiting, and little sickness, whilst the tongue remained perfectly clean till she expired. Three or four similar cases have come under my observation lately, all in females; in one of whom (so insidious is the disease) the stomach gave way whilst the patient was taking a lesson in painting, at a distance from her home. This case speaks so plainly for itself, that it needs no comment from me; but I cannot forbear entering a a protest against the imprudent liberty taken with so large a quantity as forty drops of laudanum without a competent opinion; although in this case it could not possibly have done harm.

CONSTRUCTION OF ARTIFICIAL TEETH.

To the Editor of the Medical Gazette.

Sir,

I do not remember having ever seen in your Gazette any remarks upon the construction of artificial teeth. As it is in some measure connected with the profession, and may be interesting to many of your readers, I have sent you the accompanying observations, which you are at liberty to insert if you consider them of any value.

The most accurate and scientific way of supplying the deficiencies of nature is by resorting to the pressure of the atmosphere to keep the teeth in approximation with the gums. To elucidate this, I shall make some remarks upon the processes of modelling and fitting.

The art of modelling depends in a great measure upon the temperature of the wax, the steadiness of the hand, and the consistence of the gums. If the wax be too hard, the soft parts of the gum give way upon the necessary pressure, and an accurate model is not obtained; if it be too soft, it is found to be not sufficiently tenacious to retain its form, but after having been pressed upon the gum, in the act of withdrawing it it becomes distorted and incorrect. The material of which the cast is composed being plaster of Paris, is also liable to alter its form, in consequence of the evaporation of the water with which it is mixed.

A gum may be of a different consistence in different parts. Where it forms a thick covering to the alveoli, it will most likely be of a soft consistence; but where the alveoli are thinly clothed with gum, there will not be so much yielding. The pressure of wax of a uniform temperature throughout will therefore produce a yielding of the soft parts, without a corresponding yielding of the hard parts, and consequently the model will be imperfect.

The usual mode adopted after fitting the model is to retain the teeth *in sitû* by means of a spiral spring placed on each side, in approximation with the mucous membrane covering the buccinator muscle, and connected with the upper and lower artificial teeth. This is not, however, the most perfect way of repairing the loss, especially in those cases where the whole of the upper teeth are wanting, while the whole of the under ones remain, the usual method of proceeding under such circumstances being to fit gold caps over the lower teeth, in order to afford an attachment for one end of the spiral springs. The mouth thus becomes unnecessarily filled; the motions of the teeth are confined to the extent of the springs; and the latter often produce considerable irritation of the mucous membrane with which they are in contact. But since the pressure of the atmosphere amounts to fifteen pounds on every square inch, and there are generally several square inches of surface on the gums of those persons who have lost their teeth, it is obvious that if they were accurately fitted, they would retain their situation without any spiral springs, and by the pressure of the atmosphere alone, even in opposition to the force of gravity, the powerful action of the muscles, and the great mobility of the parts entering into the formation of the mouth. I have often

seen cases where, notwithstanding these opposing forces, there has been an adhesion requiring a weight of many pounds to overcome it. This will of course vary according to the extent of surface presented by the gum to be fitted; but in most cases help can be supplied upon these truly scientific principles, the advantages of which are, in addition to those above mentioned, that many cases can be treated which would be otherwise abandoned.

I remain, sir,
Your obedient servant,
THOS. EDWD. EDEN, M.R.C.S.

248, Regent Sreet,
May 4, 1838.

FRACTURES OF THE CLAVICLE.

To the Editor of the Medical Gazette.

SIR,

WILL you allow me to make a few remarks on fractures of the clavicle, suggested by the case which appeared in your number of the 26th ultimo, by "Observer?"

We are aware that this bone, from its prominent situation and connexion with parts having extensive motion, is so liable to be fractured, that some attention is claimed to a case which tends to invalidate a point in our science which experience has hitherto so far confirmed as correct. It is in most surgical works laid down as one of the most infallible symptoms of fractured clavicle, that when the accident occurs in any part internal to the corocoid clavicular ligament, or, in other words, in that extent of bone included between the above-mentioned ligament and the sternum, that the patient is disabled from raising the hand of the affected side to his brow, and that if such an action can be performed, we may, without further inquiry, pronounce that the injury does not consist in fracture of that bone.

Mr. S. Cooper, in his Surgical Dictionary and lectures, says, "the patient finds it impossible to raise his hand to his forehead, because this action renders a semicircular motion of the humerus necessary, which cannot be done when the humerus has lost its *point d'appui*."

The following case, however, proves that this does not hold good under all circumstances, and that the above statement may even be productive of harm, by dispensing with a manual examination, no fracture being supposed to exist from the facility with which the hand is moved to the head:—

H. P., while hunting, was thrown from his horse, falling on his right hand and side, while his forehead, striking against a stone, was severely bruised; complains of great pain in the right shoulder and collar-bone, which part is swollen. He can easily raise his hand to his head without increasing the pain, and has considerable motion with the limb.

On examination, the shoulder, on the injured side, was approximated to the sternum; and, on tracing along the bone with the fingers, a depression was perceptible on its centre, together with a crepitus, and the fractured end of the inner portion, or raising end, could be plainly felt.

Sir C. Bell says, perhaps there never was a patient, sober enough to know any thing of his own situation, who was not conscious of the nature of the accident that had befallen him when his collar-bone was broken; the pain is greater than in any other fracture; the fracture is generally oblique; and the weight of the arm and shoulders makes the one end of the broken bone fall under or shoot past the other.

The action of the clavicular portion of the sterno-cleido-mastoideus slightly elevates the sternal portion of the clavicle, whilst the weight of the whole upper extremity, and the action of the pectoralis major and subclavius muscles, depresses and carries forward the scapular portion to such an extent, that often one portion appears to ride over the other.

In fractures occurring at the scapular extremity of the clavicle, where it becomes bound by the coraco-clavicular ligaments, little or no displacement can take place in the fractured portion, except such as is produced by the application of the force which causes the fracture.

The articulation of the clavicle with the sternum is unquestionably the only firm basis of all the motions of the shoulder; and this is well illustrated in circumduction of the arm, which combines all the movements which the shoulder-joint is capable of executing.

In this action the clavicle describes a cone, of which the apex is at the sternal extremity. The importance of this bone is negatively proved by the confined sphere of motion in the anterior extremities of animals that do not possess a clavicle.

It is demonstrable that if there be a breach of continuity in this, the fulcrum of movement to the shoulder-joint, that function must be suspended, and the power of circumduction of the extremity inevitably lost.

Hence we shall view the case now adverted to as of very rare occurrence, and as not militating against the usual diagnosis of fractures of the clavicle.

I am, sir,
Your obedient servant,
F. C. MOSELEY, M.R.C.S.

80, Connaught Terrace,
May 31, 1838.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'ALEMBERT.

A Practical Treatise on Fractures, illustrated with Sixty Woodcuts. By EDWARD F. LONSDALE, Surgeon, Demonstrator of Anatomy at Middlesex Hospital School of Medicine, &c. Churchill, 1838.

THIS is a very elaborate work, extending to between five and six hundred pages, and comprehending the whole subject of fractures. We have, first, general observations, in which the important principles common to injuries of this class are discussed in a manner calculated to render them intelligible to all; after which individual fractures are taken up, and their whole history and treatment detailed with the utmost care and minuteness. Frequent use is made of woodcuts; and we think, upon the whole, that it would be extremely difficult for any one not to understand the subject who reads the work with any degree of attention. The opinions of other writers are fully and fairly discussed, and we think that both practitioners and students will find the volume a very useful one, the subjects being discussed by the intelligent author in a spirit of great candour.

The Narrative of a Recovery from Tic Douloureux. By the Rev. C. E. HUTCHINSON, Vicar of Firle cum Beddingham, Sussex. London: Roake and Varty, 31, Strand. 1838.

OUR attention has been directed to this pamphlet from seeing it repeatedly advertised, and we turned to it, anxious to learn the remedy which had been of service in the painful complaint to which it refers. Mr. Hutchinson attributes his restoration to health to the use of the waters of Carlsbad; but of this the evidence is by no means satisfactory; and we think the most pointedly correct part of his narrative is, that he calls his restoration to health "a recovery," not a cure.

Mr. Hutchinson was attacked with the first of those symptoms which ended in tic douloureux, in June, 1828; but it was not till two months later that it assumed a very acute character. His description of the onset of the disease is vivid.

"On rising one morning from my bed, I suddenly felt a sharp pain between the first and middle finger of the left hand. My first impression was, that a wasp had stung me, but before I could examine the part from whence the pain proceeded, I received a second and a more severe shock, repetitions of which occurred in the most rapid succession for about five minutes; it then ceased as suddenly as it began. Though I cannot compare the pain with any thing I ever felt (nor do I think the imagination can easily conceive, much less can words describe, the exquisite acuteness of the paroxysm), yet I have before me a most vivid recollection of the intensity of these first sufferings.

"I well remember, that on receiving the second shock (the first was scarcely sharper than the sting of an insect) I was instantly thrown into a state little short of phrenzy.

"I grasped the affected arm with violence—rolled on the floor—then suddenly starting up, ran round the room, till at length being worn out with the agony of the torture, I sank on the bed in a state of almost senseless exhaustion."

On recovering from the stupor, I found my face covered with a cold perspiration, and my whole body in a state of nervous excitement."

From this time the pain continued to recur at irregular intervals. At first great benefit was derived from the car-

bonate of iron, but after a time this failed, and a great variety of remedies were subsequently tried, and successively failed. Among these may be mentioned quina, cinchona, gentian, arsenic, antimony, bismuth, mercury, hyoscyamus, conium, belladonna, colchicum, &c. &c.; while externally a great variety of counter-irritants, from the mildest up to the most severe. The latter, under "a celebrated surgeon in Kent," is thus described:—

"A blistering ointment was well rubbed into my arm from the shoulder to the wrist, which soon occasioned a pricking sensation: this part of the operation being finished, another ointment was spread upon lint, and thus applied to the affected parts; to this were affixed strips of diachylon plaster, and the whole was finally swathed by voluminous folds of several yards of linen bandage. This treatment was intended to form (as my friend expressed it) a continual vapour-bath.

"I was directed to wear this portable steam apparatus for a fortnight, and during the time to pursue a course of medicine similar to the one which I had of late been following. The diet was to be light, nourishing, and somewhat generous, as I was allowed two glasses of port wine daily, and home-brewed beer.

"At the end of the fortnight, the bandages being removed, the whole arm presented a most unsightly appearance, being covered with a crop of pimples in different stages of maturity.

"My medical adviser having cleansed the surface by rubbing it briskly with some detergent lotion, repeated the same treatment which he had used on my first consulting him, and referred me to his former directions. But before the expiration of the second fortnight, I tore off the bandage, being unable any longer to endure the torture it occasioned. The pain arising from the irritating ointment was trifling, and the inwardly burning heat, though very distressing, was bearable; but the confined state of the arm, by adding cramp to my other sufferings, almost drove me to madness, and overpowered my resolutions to persevere. Here, however, I should remark, that shortly after the removal of the bandage, the pain entirely left the arm, and fixed itself under the ball of the thumb, from whence it never shifted until its final departure in the autumn of 1832."

Warm sea-bathing, vapour baths, and shampooing, were afterwards resorted to—but all without effect. At length, weary of successive failures, and worn out with suffering, he went to Toplitz, whence his physician sent him to Carlsbad. Here he drank the waters, gradually increased to ten beakers of six ounces each, taken daily before breakfast, and in about a fortnight his disorder "seemed to be much aggravated." Still he went on, and in about a week more experienced a great mitigation of pain. Regarding himself as cured, he left Carlsbad, four days after which he had a recurrence of the disease, though to a less extent. He continued to suffer periodically, as before, and had recourse to carbonate of iron, pushed to the extent of about an ounce daily. In the spring of 1832 he went to Brighton, where he took the fictitious Carlsbad water, and continued to drink it for six weeks. He now felt much relieved, and has been better ever since. We regret to say that in our minds the taking the Carlsbad waters, and the recovery, do not clearly stand to each other in the relation of cause and effect, and we are disposed to reiterate the opinion of a "very eminent metropolitan surgeon," given at page 15:—viz.

"The Tic is a very capricious disease, and but little understood: after obstinately resisting the most powerful remedies, it frequently yields to the most simple and unexpected ones, such as change of air, travelling, &c, and I make no doubt you will lose yours in this manner."

MEDICAL GAZETTE.

Saturday, June 9, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

ANOTHER JOINT-STOCK SCHOOL OF MEDICINE.

THE present century will certainly form a remarkable æra in the history of medicine, marked as it is so peculiarly by the conversion of what was once pursued as a scientific profession by some of the most liberal and enlightened men in the community, into the speculation of joint-

stock companies, and the tool of politicians.

We have seen advertised this week a scheme which presents a further step in the course which we have always feared would follow the establishment of the medical school in University College. The real nature of that institution was early evident, in spite of the endeavour to gloss it over by a new and high-sounding name. A society of gentlemen, with some good intentions, and many with an honest view to their own pecuniary profit, wished to establish a proprietary school; but to give it a degree of eclat, and to enable it to present a fair face before the older and better schools already existing in the metropolis, they proposed to call it a University, and thus enlisted in their favour a powerful political party, which were then crying aloud against the privileges monopolized by Oxford and Cambridge. Of course another party soon sprang up from the opposite side of the political world, and not to be outdone by their antagonists they established another proprietary school.

But though they both copied the real Universities in part of their system—though they tried to hide the joint-stock company in the College—though their teachers were "Professors," — and though they had Deans and Members of the Senate, yet the Joint-stock Colleges had but the imperfect form and the feeble power of other hybrids. They then naturally enough looked for something which they might append to the unprofitable teaching of common knowledge, and with great tact they pitched upon medicine, which was peculiarly adapted for their purpose in many points, but in none more than in being both compulsory and expensive to a large number of students resorting annually to London. This, indeed, suited well enough for both classes of proprietors; the politicians lost nothing by their scheme, but could now talk of conferring degrees—of having B.A.'s and M.B.'s, in place of the mere A.B.; and the money-riskers thought what was at a discount might now rise to a premium.

Still the scheme did not quite succeed, and the whole affair was tottering, when the politicians made a hit fully as good as the speculators had when they pushed the medical department. They stole fresh names, and talked of chancellors, and vice-chancellors, and registrars; and one had hoped for proctors and bedylls, scholarships and fellowships, exhibitions and endowments: their tinsel was, while it remained new, so like the gold of the real Universities, and it was laid on so thickly, that one could scarcely catch a glimpse of the Joint-Stock Company beneath it. Thames began to call Isis and Cam her sisters, and the Senate of the new seemed so well inclined to adopt the systems of the old Universities (albeit they had once derided them), and to cherish their abuses, though they had once declaimed against them, that one might have fancied London had at last a real "University." Shareholders might have asked for dividends; and one would have wondered what the word could mean, or would have thought that they were talking of dividends from fellowships that they had gained by being double-firsts or wranglers; but one would scarcely have remembered that the Colleges of the University were incorporated Joint-Stock Companies. Such might have been the case, had the scheme been allowed to work quietly for a few years more—had there been but one or two Colleges, and these somewhat purified from their trading character, by the prudent withdrawal of the more mercenary of their proprietors, who would soon have been tired of getting no interest for their money. But, un-

happily, a competitor appears, and that in his plain working dress, disdaining the cap and gown. He comes into the streets, with his apron and sleeves on—he calls his progeny a College, indeed—the QUEEN'S COLLEGE OF MEDICINE AND SURGERY; but instead of a Senate we have *Directors;* the college-chest is spoken of as a *capital* of £20,000, in 2000 shares of £10 each; it has its solicitor and its architects; and there is talk of declaring dividends on deposits. But will its brethren ackowledge it, exalted, as they now are, from the same kind of origin? It will cost them a bitter pang to remember their own birth—that it was not in a palace, but in a counting-house; but the confession must be made, and they must try to persuade the Queen's College to adopt more aristocratic names than any they have yet got, on condition that they will then admit it into their own society. It puts us much in mind of the plot in comedies, in which, when a *nouveau riche* has, by crouching to the great and imitating their manners, by dropping one name or adding another, at last obtained admission into the circles of the great—he holds up his head and struts among them, till both he and they begin to forget that he sprung from behind the counter, when on a sudden his pride is quelled by meeting with an old school-fellow, or fellow-shopman, in the butler or footman of one of his great friends, whom he must either bribe to silence, or risk that his own origin should be divulged. Just like his, will be the feelings with which the pseudo-liberal University College, or the Aristocratic King's, will look upon the Trading Queen's. But we wish that there was nothing else in all this scheming, than what we might merely laugh at; that these *bourgeoises gentilshommes* were only acting a comedy, to amuse us in mimicking the manners of the great. There is much of real mischief in it all; and many evil consequences must necessarily result, should such schemes as the one referred to ever come into actual operation.

The teaching of medicine being placed in the hands of a joint-stock company, in which political influence and partizanship have no influence, and the appointment of the professors resting with the directors, who will be men for the most part totally incapable of judging of relative degrees of professional merit—every abuse of family influence, of jobbing, and of corruption, which has been falsely charged against the older schools in the election of their teachers, will exist with real and increasing force here. We shall now hear of—"if you will vote for my friend, Dr. —— for medicine, at Queen's College, I will vote for your brother to be a director in the Royal Thames Union Purifier." Lectureships on Chemistry will be bartered for solicitorships to asphalt and galvanized metal; professors of the art of medicine, and of all its collateral sciences, will come into the market, with attorneys, architects, and all the tribe of hungry hangers-on, which feed on the deposits of share-holders. In the colleges already formed, it was fortunate, in this respect, that the politicians had more power than the money speculators, and were obliged, in order to maintain their own respectability, to make choice of the best men in the profession, who would leave their previous occupation. But here the chief influence will be among those who merely want to make money, and the director of a small joint-stock company is not the man to stop at a trifle in the road to attain this end. No physician or surgeon of any repute will think of attaching himself to such a company merely for the chance of his share of the entrance-money of his pupils; for nothing could be more degrading to a man who has received a

liberal education, than to be made such a tool of shareholders as the teachers would be: indeed, if medical teachers will allow themselves to be led to market with the other retainers of little joint-stock companies, who can wonder that practitioners should be ground down by Poor-law Commissioners, or should degrade themselves and each other by tenders? Already the profession was thus becoming a trade; but now, as if that were not low enough, it must be required to become a marketable commodity for stock-jobbers to deal in.

One thousand supernumerary shares in this precious scheme, it is said, are to be appropriated to the students of Queen's College, but they are not to take any part in the management till the expiration of the period of their education; nor will they be required to make any advance on their shares till that time. This probably means, that to all students, some shares receivable three years after date of entrance, will be given in return for his entrance-money. We can scarcely see how a person who neither pays for a share, nor takes part in the management, or profits or loss, can be called a shareholder, however we confess to be of a *profession*, and therefore may be ignorant of all the quirks of the trade. But we can see clearly enough the introduction of a new influence over the medical teachers who may unhappily become connected with these joint-stock schools. Not only are they to be the tools of the directors, but their own pupils are to be their masters after three years; and for any offence may remove or retain them nearly as they please. Holding this power *in terrorem* over them, they may extort from their teachers just what they please; may attend lectures or not, as they please; may, in short, direct all the business of the school.

Much of this, however, depends on the supposition, probably an erroneous one—that the new Joint-stock Company will ever carry into effect what it purposes. Indeed, we think it very problematical whether such a scheme as this can succeed: something more than mere stock-jobbing was found essential to the original proprietary schools. The directors are indeed sanguine: "if," say they, "its success be equal to that of the medical classes of the London University College,—and there is no reason why it should not—a dividend of fifty per cent. will be declared on the original deposit." Certainly, we see no reason why the same methods which have been resorted to in Gower Street should not succeed as well in any other region; but they must succeed considerably better, to yield fifty per cent. We suspect that any one who invested his money in the London University would deem a dividend a fine wind-fall.

We trust and believe that the "Queen's College of Medicine" will never exist except on paper; but should we be wrong, it is sickening to think what medical teaching would thus become. Twenty years ago it was the province of the best of a profession of gentlemen—now it is descending every year into lower hands. It is a trade, and worse than a trade—stock-jobbers buy and sell it.

MEMOIR OF THE LATE THOMAS BLIZARD, ESQ.

The extraordinary success which attended Mr. Blizard's short professional career, and which has made it the model which every aspirant has at some time hoped, but few have ever expected, to imitate, will, we are sure, render a brief memoir of him interesting, and perhaps useful to some of our readers, if we can point out how so early eminence was attained, and so rich a harvest reaped.

Mr Blizard was born in 1772. He commenced his professional life by being

apprenticed at Surgeons' Hall, in 1789, and he pursued the greater portion of his education at the London Hospital, to which his cousin, Sir W. Blizard, was attached. Having received his diploma, he was elected assistant-surgeon to that hospital in 1795, and he at once commenced the teaching of anatomy in its school. In 1796, he was appointed Professor of Anatomy to the Corporation of Surgeons, and delivered lectures in that capacity for three years. In 1797, he became surgeon to the London Hospital. He now resigned his anatomical lectureship, for his time was already fully occupied in the duties of the hospital, and his increasing private practice. To these he applied himself with the most unremitting zeal, daily learning in one the means, and earning in the other the reward, of bestowing benefits on both. From this time his practice increased every year; and in 1816, finding it occupy every hour that he could, consistently with health and comfort, bestow upon it, he resigned the surgeoncy to the London Hospital. In the same year he was chosen one of the council of the College of Surgeons; and in the following, when his practice in the preceding six months had produced him a larger sum than is recorded of any medical practitioner, he resigned every professional occupation, and retired to the continent.

The incidents in Mr. Blizard's life are few, but they were important, and they furnish a clue to his professional success. He began by being a teacher of anatomy, and the earnestness with which he pursued that science is sufficiently proved by his being so early chosen Professor to the College. He laid the same foundation for eminence in surgery, which John Hunter, the Clines, Abernethy, and nearly every one of his contemporaries, and even of those of the present day, who can congratulate themselves on their career, have laid; all these have commenced their course and gained their first honours in anatomy. He was strictly a hospital-surgeon; his education was wholly conducted at a hospital, and he became early connected with one as an officer. But his own hospital education did not cease when he was appointed to teach others; he was always a pupil in his own school; and from every case that presented itself he was daily drawing lessons, of which others, as well as himself, reaped the benefits. The hospital was at once his study and his theatre; and the store from which he brought out that supply of knowledge which he dispensed for the advantage of the public.

Mr. Blizard's mind was peculiarly adapted for drawing the greatest possible advantages from the opportunities that he thus enjoyed. The prominent features of it were vigour of intellect, and immoveable firmness in carrying into action every design which he believed to be right. One who was long a pupil at the London Hospital says, that the opinion which he formed of him from observing his practice was, that he saw further—saw clearer—and acted more energetically, than any surgeon he ever knew. His surgery was indeed purely practical: his anatomical knowledge was so exact, that no anticipated difficulty of manipulation deterred him from an operation when he deemed it necessary; nor did any unexpected accident embarrass him in its performance. Nay even after twenty years of retirement, his anatomy was still good and clear—as easily producible as ever in discussing a case, or examining a preparation; no rust had accumulated on its polished and well-tempered, though long-neglected, surface. The excellence of his lithotomy, which he always performed with the long and slender-beaked knife, which still bears his name, has always been enthusiastically described by his old pupils; yet he was not fond of operating, and so little anxious to display his own powers, that he was always as ready to assist others in their difficulties as to take the chief part himself.

Mild, and even affectionate, as his manners were towards his patients, and in the extensive circle of his friends, his spirit was truly indomitable when he was opposed in the pursuit of what he deemed to be just and honourable. His success was as little the result of servility as of private interest: he fairly and honestly worked his own way, and often against the tide; repelled by no obstacle—distrusted by no fear—retarded by no discouragement or indolence; entirely by his own industry he gained the knowledge which first brought him into public notice; and by the unremitted exertion with which he pursued every advantage when once attained,

he had earned by his profession, and at the age of 46, an ample fortune.

His retirement when at the climax of his reputation, and when his income from his practice had attained an amount perhaps unparalleled in the history of surgery, marked, as much as any event of his life, the vigour of his mind, and the remarkable union of kindness and firmness which had always distinguished him. Mrs. Blizard had, for some time previously, been in a weak state of health, and he found that his professional engagements prevented him from affording to her the comfort of his own constant attention. Change of air, too, was recommended; and, with scarcely one thought for the reputation and success which he had so hardly obtained, and for which, with most men, the love would have grown with their growth, he resigned all, and retired to the continent. There he remained for nearly seven years, and then, Mrs. Blizard's health being but little improved, returned to England, secluded himself almost entirely from society, and for thirteen years more devoted himself to her alone. One might see in the interest which he still took in every thing that related to his former profession what self-denial it cost him to be separated from it; but the feelings of affection and duty steadily prevailed, and, except to give his gratuitous advice or assistance to his friends, he took no part in it. In 1837 death at last deprived him of his only care, and broke almost the last tie which had held him to the earth. But this tie, painful as it had been, seemed necessary to his life. From the time of Mrs. Blizard's decease one might see an evident alteration in his manner and appearance; he was no longer anxious to live; and when his last illness seized him, he rapidly, and as it were voluntarily, succumbed to it.

Mr. Blizard never forgot his obligations to his profession, but with a noble spirit he liberally contributed to every object proposed to him for the honour and advance of surgery, and for the relief of his less fortunate brethren. We find him, accordingly, a large contributor to the hospitals and infirmaries in the neighbourhood of his residences, especially to the Gloucester Infirmary and to the Brighton Hospital. The London Hospital will readily acknowledge his repeated donations to advance all its charitable and scientific objects, his last act of charity being to bequeath 500*l.* to its funds, and 100*l* to its Samaritan Society.

IRISH MEDICAL CHARITIES.

To the Editor of the Medical Gazette.

Sir,

The surgeons of the County Infirmaries in Ireland have published, within the last few weeks, and circulated amongst the members of the House of Commons, a memorial in vindication of themselves, and of the management of the institutions under their charge, to which objections were taken by the Medical Assistant Commissioners, and have been recorded in their reports.

They do not blame me so much for my report as for the letter which appeared in the Medical Gazette, March 24th. I should, therefore, be much obliged to you to insert, as soon as possible, the inclosed letter, in reply to Dr. Jacob, of Maryborough, one of the most active among them.—I remain, sir,

Your very obedient servant,

W. P. Borrett.

Queen Ann Street,
May 24th, 1838.

To John Jacob, M.D., Surgeon to the Queen's County Infirmary, &c. &c.

Sir,

I have just received from a friend a copy of a printed document, entitled "General Remarks and Explanations, in reference to the Report of the Assistant Medical Commissioners of the Irish Poor Inquiry, on County Infirmaries; with observations on the Irish Medical Charities Bill, as introduced by Mr. French and Mr. Sergeant Ball: by the Surgeons of the County Infirmaries of Ireland:" and, as I observe the signatures of so many members of my own profession attached to this document, and that it has apparently been circulated by yourself on their behalf, I trust you will allow me to address to you some remarks upon its purport and contents.

It is satisfactory to find that you speak of my Report, as a medical assistant commissioner, as "being drawn up in a fair spirit," but I must decline the contrast with the Report of the late Sir D. Barry to the disadvantage of that gentleman. For although his animadversions on some of the Irish county infirmaries which he visited may not be agreeable to their

officers, who may perhaps think them unjust, yet even with the slight personal knowledge which I had of Sir D. Barry, I feel bound to say that he discharged his duty as a commissioner with a desire of acting fairly and justly by all parties. Nearly three years having elapsed before these objections have been raised to the accuracy of his statements, I think it may be inferred that the censure passed on him is not merited. I may add my regret that any members of our profession should deem it right to attack a deceased member in the tone and spirit which this document evinces; especially as I cannot discover that the explanations given in reply to his Report, shake its general evidence and purport in any essential particulars. Dr. Corrie, Sir D. Barry's colleague, is, I observe, also alluded to; but I cannot take the liberty of interfering with any reply which he may think fit to make—or to reply for him, should he deem a reply from himself unnecessary.

As to the praise given to my own colleague, Mr. Roney, I can and do most cordially concur in your recommendation to the public to read his Report, as I hope they will mine; for that will be the surest course to arrive at a just estimate of either. I rejoice to find that in some respects our views coincide, though the surgeons of the County Infirmaries are in error if they think I concurred from any "obligation," save that of being desirous to speak an honest opinion.

These points I have first adverted to, as they are the basis of what I would say in answer to the uncourteous tone of some of the remarks on myself. And although I have disclaimed any contrast to the disadvantage of one medical commissioner, I can assure you I have no contrary feeling towards Mr. Roney, who being an Irishman, a member of the Irish College of Surgeons, &c. and, as you state, "possessing some community of feeling with his professional brethren," naturally receives from them the tribute of their approbation. All I desire to explain to you is, that my professional sympathies are quite as strong as those of any of my brethren, either in Ireland or in England, although my mode of evincing those sympathies has not been pleasing to the medical officers who have signed the "Remarks."

After bearing the testimony to the fairness of my Report which I have already cited, you say, in allusion to my letter on the Medical Charities Bill, that I have adopted the language and words of Sir D. Barry; and in the next paragraph you complain of some parts of that letter, averring that you "have in vain searched" my Report for facts to support my charges. Surely it might have occurred to you, that if I quoted the words and language of Sir D. Barry, it was in *his* Report, and not *my own*, that the facts and evidence were to be found to support my quotations. And it could not have escaped your notice, on a fair perusal of my Report, that in some of the medical charities which I visited, not being county infirmaries, I detected abuses similar to those of which Sir D. Barry speaks in his Report on County Infirmaries; but I have *nowhere* fixed them upon the latter class of institutions in particular, as you have erroneously stated.

Again, you say "it was my duty to give Parliament the benefit of my information, in the Report printed by their orders, without reserving it for unauthorized publication in a medical journal." You should have considered, before you blamed me, the different nature of the two communications, and the distance of time between their publication. The Report was made under a specific inquiry, and definite instructions. The facts I then learned, and the inferences I drew from those facts, are to be found in that Report, of which it is enough to repeat that you admit the fairness. But in writing comments on a Bill extending not only to the Infirmaries and Dispensaries visited and reported upon by me three years ago, but to all the institutions of this kind now existing in Ireland, it is obvious that I was justified in using existing evidence besides my own. And whatever information I might have gained since my Report was made, Parliament could at all times call for; but I could not, even if I had desired it, or felt it my duty, give it to Parliament, the Commission being closed. This I am ready to maintain—that I have not made any assertion without evidence, either taken by myself or others; and as to my inferences and suggestions, the only point that comes in dispute is, whether they are warranted by the facts.

With reference to the "Remarks" on the Medical Charities Bill, I beg to add one or two comments. You justly observe, that "the appointment of medical inspectors is calculated to insure the proper working of the medical charities of the country;" and I think, even if the Bill now before the House of Commons were confined to a system of medical inspection alone, it would work a benefit of the highest kind. But I cannot think that because these charities require inspection, a new curriculum of medical instruction should be introduced into such a measure—thus attempting medical reform by a side wind. And when to this is added the evident object of maintaining the monopoly pointed at by Sir D. Barry and myself, and vindicated by your remarks, I must confess

that I think this addition to the Medical Charities Bill has monopoly alone for its object, and not medical reform. True it is that your vindication asserts the existence of the "intellectual superiority" of Ireland in these respects: but intellect knows of no monopoly; or if it do recognize any exclusiveness or superiority, it is self created, and disdains the fetters of municipal or fiscal regulation. That the three professions of Divinity, Law, and Medicine, do possess a species of monopoly is true; for the government wisely prohibits their practice without a guarantee to the public that the professors have attained certain degrees of proficiency as the basis of their right to practise. But this is the privilege of the public, not of the profession; and the grand point should be to make all subject to the same test, all being entitled to the advantages which the passing that test leaves the candidates for professional fame, and, if you will, professional profit, at liberty to attain.

But you speak of me as labouring in my late capacity of medical inspector of the Infirmaries under the disadvantage so common amongst Englishmen—"a deficiency of acquaintance with the actual condition and wants of this country." I pass over the inconsistency of applying such a remark to a Report of which you speak so well, and the obvious answer, that a person who is free from local prejudices, or even patriotic, and hence justifiable, preference for the institutions of his country, would perhaps, in an inquiry almost partaking of a judicial character, give a more unbiassed opinion, than those who entertain these honest prejudices, and hence, it may be, have a tendency to treat even abuses with respect and affection. But when you "acquit me of any disposition to exercise favour or affection in your behalf," and infer that my object is to create "an unfavourable opinion" of you and your colleagues, in your "official capacities," I must not allow you, or them, or any who may see your remarks, to imagine that I have such a bad opinion of the class of practitioners to which you belong, or have been actuated by any hostile feelings of a personal character, however I may deem the existence of abuses in the sytem of the medical institutions of Ireland a reason for seeking their reform. For instance, in my Report, p. 52, I readily acknowledge that "the members of the Irish College of Surgeons are at great pains and expense to acquire sound professional knowledge and experience, and are subjected to a very strict ordeal before entering upon practice; and hence they are entitled to preference as a general rule." And I have expressly stated, in my letter on the Medical Charities Bill, that "it is not proposed to interfere with the existing rights of those who are now attached to the County Infirmaries."

As a further proof that I do not entertain any prejudices such as you would apparently infer against me, I may add, that upon graduating in arts in Cambridge, I went for the purpose of medical education to the Dublin schools—that I attended to dissections, lectures, and the practice of the hospitals; and passed a considerable time, subsequently, at two distinct periods; at the Lying-in Hospital—an institution unequalled in the opportunities it affords for instruction in that particular line of practice. The result of my experience since, in London and Paris, proves to me that Ireland is not behind those great schools of medicine in the talent of her medical professors; who, therefore, I should imagine, if they would do themselves justice, would trust to intellectual competition alone, and cease to rely upon the broken reed of a narrow provincial monopoly.

I could quote several passages from my Report, to show that I have not charged upon *individuals* such faults as belonged to the *system*, nor withheld *praise* wherever I conceived it was justly due. But I have already far exceeded the fair limits of a reply. In conclusion, therefore, whilst I repeat my regret that the remarks published by yourself and your colleagues should have been written in such a tone as to assume the character rather of party feeling than temperate remonstrance, I do not desire to imitate the example you have set me. My Report has been some time before the public, and I have received many gratifying testimonials in its favour. To this, and to my letter in the Medical Gazette of the 24th March, to which you have by your remarks given no little importance, I refer for my opinions; and I repeat my readiness to vouch for any assertions in either of these for which I can fairly be held responsible. If I am in error, I can only hope that the intention to do my duty will protect me, with all who take the trouble of searching for truth, from imputations of unfair or illiberal conduct towards any of my professional brethren;—and when I speak of professional brethren, I include men of every country where the honour of the profession and a high scale of medical education are uppermost in the hearts and anxieties of its professors.

I have the honour to be, sir,
Your very obedient servant,
Wm. Penrice Borrett, M.D.
Fellow of the College of Physicians, London.

9, Queen Anne Street,
Cavendish Square, May 21, 1838.

PENNSYLVANIA HOSPITAL.

Case of Varicose Vein, cured by means of Needles introduced through the Veins, after the method proposed by Davat.

George K——, a German, aged 57 years, was admitted into the wards on the 19th of July, 1837, for varicose veins, from which he had suffered for several months. He had had a large ulcer caused by the veins, for which he had been treated in the city by bandages, &c. The ulcer was much reduced in size when he entered, and, after appropriate treatment, healed. On the 12th of August, Dr. Norris introduced two acupuncture needles, one behind the vein, and the other through and through it in a line oblique to its axis, and surrounded both by a figure-of-eight ligature. Little pain was caused by the operation; the limb was then elevated in a fracture-box; lead-water cloths applied, and the antiphlogistic treatment directed.

August 15th.—The patient complains of no pain; little inflammation has occurred; ligature tightened, and treatment continued.

17th.—Slight inflammation at the sutures; same treatment.

19th.—The needles and ligatures were removed; some inflammation around the part, but none to any distance above or below.

24th.—Inflammation increased; slight ulceration at the points where the needles entered; a poultice to the part, and antiphlogistic treatment continued.

September 4th.—The ulcers have healed; the vein perfectly obstructed; bandages and compress applied along the course of the vein.

7th.—Allowed to walk about; has slight porriginous eruption; treated accordingly.

15th.—Vein obliterated entirely; patient walks without feeling any inconvenience from it.

17th.—Discharged—entirely well.

Within a few weeks the patient was seen, having had no return of his complaint, and continuing constantly at work.

Case of Rupture of the Larynx, from a blow on the Pomum Adami, cured in two weeks.

F. N——, aged 45 years, a watchman, whilst attempting to arrest a man in the neighbourhood of the river, on the night of the 19th of October, 1837, was knocked down and struck on the throat by a large piece of coal. He was seen immediately by a physician, who found him nearly strangled, unable to speak, and with constant spasm, whenever he attempted to speak or swallow. He was bled freely, and sent to the hospital eighteen hours after the accident.

October 20th.—The throat much swelled, and inflamed externally; fauces slightly reddened; aphonia complete; breathing stertorous; barely able to whisper, and swallows with great difficulty. The cartilages of the larynx are loose and crepitant, the thyroid separated and moveable one on the other. Examination of the larynx causes violent gagging. Ordered sixty leeches to the outside of the throat, and warm cloths afterwards, to promote the bleeding. Gruel and tea for diet.

21st.—Rested well; voice somewhat stronger; less pain in the throat; swallows rather better. Ordered the same number of leeches; injection to bowels, and other treatment continued.

23d.—Voice quite audible, though very hoarse; swallows well; no pain on slight pressure on the larynx; sitting up in bed; has swallowed but little since his admission. Ordered weak broth for diet.

24th.—Voice gradually returning. Ordered blister to the throat, to be followed by a poultice.

26th.—Walking about; union of cartilages quite firm; voice improving. Blister repeated; treatment continued; brown mixture for cough, which troubles him slightly.

28th.—Voice nearly well, though hoarse; swallows as well as ever; cartilages united entirely.

November 4th.—Patient discharged; voice strong; no motion in the cartilages; slight hoarseness.

December 15th.—The patient was seen to-day; he is able to attend to his duties; speaks clearly, but is unable to call the hour without some difficulty. Has been free from pain since he was discharged.—*Philadelphia Medical Examiner*, April 25.

ABSORPTION OF WATER THROUGH THE CUTICLE.

Professor Berthold has repeated Seguin's experiments on the absorption of water through the uninjured cuticle, and has arrived at results differing considerably from his. The conclusion which Seguin drew from his experiments (see *Annales de Chimie*. 90, 92, 93 was that there is no condition under which an increase of weight by the absorption of water in bathing takes place—a doctrine which, though it seemed contrary to some well-known facts, has been very generally received. Berthold's experiments were made by carefully weighing himself in scales which would carry 125 pounds, and turn with less than a drachm. The temperature of the baths

was 22°—28° R. The experiments were made in August 1835, four hours after a meal taken in the middle of the day, and varied only in respect to the time during which the immersion continued. He found that in a quarter of an hour he gained 3 drachms; in three-quarters of an hour, 7 drachms 20 grains; in an hour, 1 oz. 30 grains; so that, reckoning the loss of weight which was constantly taking place by the pulmonary exhalation at seven grains per minute, which is the calculation that Seguin also adopted, the real increase of weight in the bath was 4 dr. 45 gr. in a quarter of an hour; 1 oz. 4 dr. 35 gr. in three-quarters of an hour; and 1 oz. 7 dr. 30 gr. in an hour.

This calculation does not allow for the loss of weight that would take place in the bath by the washing off of loose portions of cuticle or other adhering substances, nor for that by the perspiratory and sebaceous secretions. The latter, in vapour baths, are carried on with remarkable activity. He quotes two experiments made by a person, aged 32, who remained for half an hour in a Russian vapour bath, at 43° R., and who in each case lost about a pound and a half in weight.—*Müller's Archiv*, March 1838.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO HAVE RECEIVED DIPLOMAS.

April 1838.

Frederick Philpot, Bayswater.—Jas. Williamson, E. I.—James A. De La Hooke, Gravenhurst.—William Smith, Manchester.—William G. Salmon, Thornbury.—Alfred Master, Norwich.—Henry Lynch, Everton, West Carlow.—James Taylor, Hull.—Chs. Senior, Huddersfield.—John Smith, Millridge, Leeds — Edward Edlin, London.—Geo. Bakewell, Spring Vale, Staffordshire.—Fred. G. Rose, London.—Richd. Griffiths, Worcester.—Gillies Calder, Manchester.—Wm. K. W. Baylie, Cork.—Thomas K. Strover, Bengal.—William B. Dolton, London.—Wm. Williams, Dolgelly.—Chs. Taylor, Boston.—Joseph Jowett, Lincoln.—John Fox, Cavain —Dan. G. Thackeray, Nottingham.—James L. Lowry, E. I.—Wm. Jamieson, Newcastle-upon-Tyne. — Robert P. Collison, Liverpool.—Charles W. G. Guthrie, London.—Richard Squires, Liverpool.—John Baker, London.—Bury J. Dasent, London.—Robert Bacon, North Walsham.—Robert L. Campbell, London.—Robert M. Cartwright, New South Wales.—Charles Creed, Halifax, Nova Scotia.—John D. Tweedale, St. Martin's-lane.—John Wood, York-road, Westminster.—James W. Agnew, Ballydare, W. Antrim.—Edmund C. Hobson, Australia.—Geo. M. Perry, London.—Richard L. Parry, Conway, N. W.—Fras. B. Reynolds, Arlington, Gloster.—Alfred Aspland, Hackney.—Charles Holmes, Oxford.—Richard E. Turner, Southmolton.—Thomas M. Price, London.—Dennis Blanchet, Lower Canada.—James Boulton, Pontypool.—Robert Molloy, London.—John Smith, Derby.—Theodore Parrett, Newport.—Henry Crawford, Feversham.—John W. Sparrow, Plymouth.—Perry Dicken, Exeter.—Geo. Weathers, Lambeth.—Edwin F. Fox, Bristol.—Alexander Euston, Bengal.—Thomas Mather, Ashton-le-Willows.—Andrew Moffitt, Fermanagh.—James Holmes, Castle Ferm.—Fred. Baillie, London.—Peter Cooper, Leicester.—Michael Mulreamy, Donegal.—Stephen Staley, Stroudwater.—Evan Evans, York —Charles A. Holrombe, Pembroke.—Samuel Hey, Leeds.—David Unwin, Coggeshall—Charles Greig, Bristal.—Arthur Kempe, Exeter.—Benjamin Hobson, Birmingham. — John W. Houghton, Dudley. — Rice H. Howells, Carmarthen.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, May 31, 1838.

Joseph Helmer, Romaldkirk.—Samuel George Hayes, London.—Henry Sockett Humphreys, Oswestry.—John Johnston.—Richard Ryther Steer Bowker, Appleby.—Daniel Britten Meek.—John Tomlinson Shaw, Wirksworth.—John Stewart Allen.—Thomas Durran.

Thursday, June 7.

John Jolliffe, South Ranaly, Lincolnshire.—Thomas Hatton Wardleworth, Pendleton.—William Wood, Silsoe, Bedfordshire.—Edward Harris Derriman.—William Henry Merry, Maiden Newton, Dorsetshire.—Richard Forrester Wells, Highgate.—Stephen John Peach Parker, St. Helen's, Jersey.—Adam Bell, "Dreadnought."—Anthony Martin, Edgbaston, Birmingham.—George Brunton, York.—Thomas Ayling, Hampshire.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *June* 5, 1838.

Age and Debility	16	Fever, Typhus	4
Apoplexy	5	Heart, diseased	2
Asthma	5	Hooping Cough	9
Cancer	1	Inflammation	8
Childbirth	2	Bowels & Stomach	3
Consumption	42	Brain	2
Convulsions	15	Lungs and Pleura	7
Diarrhœa	1	Insanity	2
Dropsy	6	Locked Jaw	1
Dropsy in the Brain	8	Mortification	1
Dropsy in the Chest	1	Paralysis	3
Erysipelas	1	Small-pox	15
Fever	16	Unknown Causes	153
Fever, Intermittent, or Ague	2	Casualties	6
Fever, Scarlet	2		

Increase of Burials, as compared with the preceding week . . . 68

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

	THERMOMETER.		BAROMETER.	
May.				
Thursday . 31	from 45 to 71		29·81 to 29·84	
June.				
Friday . . 1	44	69	29·87	29·86
Saturday . 2	50	68	29 78	29 73
Sunday . . 3	44	67	29·73	29·71
Monday . . 4	48	67	29·67	29·71
Tuesday . 5	39	59	29 81	29 87
Wednesday 6	44	60	29·94	30 05

Winds very variable. S.W. prevailing.

Generally clear, except the evenings of the 31st ultimo, and 1st, 3rd, and 5th instant, when rain fell; distant thunder on the evening of the 31st and morning of the 5th, accompanied with heavy rain.

Rain fallen, ·9125 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 16, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton-Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXII.

Diseases of the Parenchyma of the Lung (continued).—Phthisis Pulmonalis.—Pathological History (continued).—Recapitulation to explain the Changes.—Origin of Phthisical Disease; from Inflammation; from Constitutional Disease.—Symptoms of Phthisis.—First Stage, Irritation and Obstruction; Second Stage, Tuberculous Deposition; Third Stage, Softening, Ulceration, &c.—Complications.—Physical Signs of Phthisis.—First Stage, Percussion and Inspection.—Stethoscopic Signs.—Difficulty and Importance of the Diagnosis.

We had, in the last lecture, so many points to discuss, and so many views to examine, that I think it right to aid your memory by once more stating a summary of the pathological changes which are produced in the lungs by phthisical disease, with a reference to their analogies to other lesions.

1. Induration, whether granular or diffused, caused by the deposition in the tissue, of animal matter of low organization, under the influence of chronic inflammation, or of defective vitality in that part of the blood which maintains the nutrition of the tissues.

2. The conversion of this induration into crude yellow tubercle, by the substitution of a friable albumen wholly destitute of vitality. This change has its parallel in the increase of fragility and opacity, which precedes the conversion of effused lymph into pus. Sometimes the indurated tissue is destroyed by direct ulceration, without the formation of yellow tubercle.

3. The formation of vomicæ by the softening of crude tubercle, and the original deposition of soft tuberculous matter. These bear analogy to suppuration, and the formation of purulent depôts.

4. The evacuation of vomicæ, and the formation of ulcerous cavities, which is like that of fistulous ulcers from abscesses.

5. The hardening of tubercle without discharge, into cretaceous tubercles; like the conversion of abscesses into indolent tumors.

6. Accidents contingent on the preceding changes, such as hæmoptysis, from the obstruction or ulceration of blood-vessels. Perforation of the pleura, and consequent pleuritic pneumothorax. Intercurrent inflammations, bronchitis, pneumonia, ulceration and other changes in the bronchi, trachea, and larynx.

Besides these, various changes are commonly produced in other parts of the system, such as tuberculous disease in the mesenteric, and other lymphatic glands, spleen, liver, brain, ovaries, and other parts, and tubercle, combined with induration, or some low form of organized structure on the serous membranes, the pleura, peritoneum, membranes of the brain, &c.; inflammation and ulceration, probably proceeding from tuberculous deposits in various parts of the intestinal mucous membranes; chronic inflammation of the joints, and brown softening of their synovial membranes; diseased nutrition of the interosseous ligaments; greasy degeneration of the liver; and general wasting of all the tissues.

Now, instead of giving you a detailed account of the symptoms of phthisis, for which we have not time, I shall endeavour to class them according to the pathological stages of the lesions that give rise to them. But before any of these lesions are actually formed, we have found it necessary to assume that there is a cause, either local or constitutional, or partaking of the character of both, which precedes them. So, also, if we attend to different cases of pulmonary consumption, we shall find some in which the individuals had enjoyed very good health until they were attacked with one or more severe colds, or inflammations of the chest, or a fever accompanied by pectoral symptoms sooner or later, after which the phthisical disease commenced. In others, again, the cough and other symptoms begin very gradually, without any very obvious cause, and with as little apparent external reason soon increase to a serious extent, and the consumption runs a rapid career. In a third class of cases, the patients have been out of health, in a debilitated or cachectic state, before the commencement of the cough and other local symptoms, which become suddenly developed after exposure to cold, the stoppage of an habitual evacuation, or some other cause likely to occasion local irritation or plethora.

In the first class of cases we have the development of phthisis from local inflammation, without any evidence of prior constitutional disorder, unless an hereditary predisposition, which may be traced in some of these cases, may be considered as such. The acute inflammation, whether pulmonic, pleuritic, or perhaps bronchial, imperfectly treated and only partially subsided, passes into a chronic form, and either immediately develops phthisical indurations in the lungs, or by lowering the vital powers generally, leads to their formation from perverted nutrition, or from the irritation of any fresh exciting cause. Under any of these circumstances, chronic inflammation, either by its own local effects, or by its depressing influence on the constitution, or by both combined, becomes a sufficient cause of pulmonary consumption: and, as you may suppose, consumptive disease originating in this way, is sometimes more limited in its extent, and manageable in its course, than that first arising from a constitutional disorder. As the cause is more local, so the lesion is more confined to a part; and we see instances of it in the very partial indurations or tuberculous depositions which we frequently meet with in the lungs of elderly persons who die of other complaints. In at least half the instances in which I have thoroughly examined the lungs of persons in London and in Paris, above the age of forty, without reference to the disease which caused their death, I have found some traces of those lesions which, occurring in greater extent, constitute phthisical disease. You can very easily discover these by passing the lungs quickly between your fingers, and when you feel a hard body, on cutting into it it will be found to be either a grey induration or a yellow tubercle, or the remains of one; whilst nothing in the rest of the body betokens any scrofulous taint. Yet the same local causes that produced these solitary tubercles, may engender many; and the greater the number that the local cause thus develops, the more effect will it have on the constitution, which becomes in a manner inoculated by it. Hence, therefore, also, out of a local cause, such as a latent or neglected pneumonia, pleurisy, or peritonitis, may arise a general tuberculous disease, involving, more or less, the whole system. Among the local causes of consumption are to be reckoned, also, the habitual inhalation of fine solid particles, which is contingent on certain occupations, such as those of needle-pointers, dry grinders, stone-masons, miners, colliers, and the like. The dependence of the lesions, in these cases, on the mechanical irritation of the inhaled particles, is sufficiently proved by the presence of these particles in the indurated lung, which in the case of colliers is completely blackened by them. With these cases may be associated those of lesions, resembling those of phthisis, produced in the experiments of Saunders, Cruveilhier, and others, in the lungs of animals, by the injection of mercury into the air-tubes, or into the blood-vessels.

In the second and third class of cases, which are unfortunately the most numerous, the disease is certainly constitutional; and the cachectic condition of the system, as well as the circumstances that seem to occasion it, fully correspond with the pathological views which we have taken of the origin of tubercle. Imperfect nutrition, whether from deficient or improper food, or from a permanently disordered state of the digestive or assimilative organs; unhealthy air, whether from closeness, humidity, or impurities; long continued exposure to cold (as from insufficient clothing), not intense enough to produce inflammation; depressing passions, such as disappointed love, anxiety or distress from reverses of fortune, or other severe calamity; venereal excesses; profuse and very weakening discharges; adynamic fevers; irregularities of the uterine function, especially those that lead to chlorosis; the sudden suppression of habitual discharges, or of long-established cutaneous eruptions,—these, singly or com-

bined, are the most common causes of the constitutional origin of tuberculous matter: they all tend to destroy the balance of the functions, and diminish the tone of the system, and with it that rich fibrinous and vital condition of the blood, by which proper nutrition and the organic functions are sustained. The impoverished blood, defective in that vital albumen with which the tissues are fed and renewed, deposits in its stead a degraded matter, imperfectly or not at all organizable, like that resulting from the lower degrees of local irritation or inflammation. The lungs, the lymphatic glands, and a few other organs, become the first seat of these deposits, because their textures are in freer relation with the blood than those of other parts are; and if there be in these organs also a congestion, an irritation, or an inflammation, the deposition becomes more extensive and rapid in proportion, although, if the inflammation be acute, it may also produce organizable lymph, together with the degraded albumen, tubercle. And mark how the natural activity of the nutritive process will determine the rapidity of the tuberculous deposition, and the progress of the disease. In young persons nutrition is most active; so is consumption spontaneous and rapid; and at no period is this more manifest than between the ages of eighteen and twenty-five, when growth becomes completed, yet the vessels and their blood do not immediately lose their habit of deposition. But throughout the whole period of childhood and youth tuberculous disease is very common, and is more easily engendered than when nutrition is less active, under the influence of the causes which I have enumerated. You can understand, too, why this morbid nutritive activity, this disposition to deposit albuminous matter (in such cases inorganic, tuberculous), should be shewn in women after the completion of utero-gestation, and in persons on the speedy healing of large suppurating wounds—circumstances which, as long as they continue, are known often to suspend the progress of consumptive disease. For a further account of the constitutional causes of tuberculous disease, and for the only good description of the condition of the system which precedes its development, I must refer you to the work of my friend, Dr. (now Sir) James Clark, on Pulmonary Consumption and Serofula, by far the most complete work which we possess on the subject.

Let us now quickly enumerate the symptoms of pulmonary consumption in the order of the stages of the lesions which produce them.

First stage.—The indurations are accompanied as well as preceded by various irritations, both local and general. Of the local irritations one of the earliest is *cough*, which at first is generally slight, and merely hacking, but more or less constant; either dry, or attended merely with a thin transparent mucous expectoration. Another occasional sign of local irritation is *pain* in the chest, which is commonly referred to the sternum, and is rather a soreness, or an unusual sensibility to cold or exertion, than a continual pain. Of the more general irritations, *quickness of the pulse* is the most constant, but it is not universal. The quickness is frequently not uniform at first, but depends on trifling causes of excitement, and the pulse may be very slow and weak in the intervals; but as the organic lesion increases, it gradually becomes more constant, and is accompanied by an irritated state of other functions—a general febrile state. But even then there is not power enough in the circulation to maintain a general or constantly increased heat; it is manifested most towards night, after the accumulated excitements of the day; and it is generally most felt by flushing of the face, and heat in the palms of the hands and soles of the feet, where the thickness and hardness of the cuticle prevent the perspiration and evaporation which moderate the temperature of other parts. Like other weak febrile movements, this terminates by perspiration more or less profuse, which, occurring in the night, leaves the pulse lowered, but the frame weakened and exhausted in the morning. It is only in the severer cases that this general irritation, or *hectic fever* as it is termed, becomes marked at this early stage of the disease. There is often also gastric irritation, with the tongue white and red at the edges, thirst, costive bowels, and turbid urine. These symptoms are perhaps more remarkable in this than in the after stages, when the irritation is more confined to the organs of respiration and circulation.

The symptoms from *obstruction* comprehend those from the *obstruction to the air, to the blood,* and *to the motion of the lungs.* The indurations, granular and diffused, when extensive, *by obstructing the passage of air* to the lungs, occasion the short breath felt at first only on exertion, so common even in the early stage of consumption. Nay, cases happen in which an abundant formation of miliary tubercles, together with the œdema or bronchorrhœa that they excite, prove fatal in their first stage, by the obstruction which they cause to the passage of air. We have seen that tubercles, or any other partial obstructions, may become a cause of pulmonary emphysema, or dilatation of the air-cells, in

the manner which I have already explained to you. The indurations, by *obstructing the blood-vessels*, give rise to many formidable pathological effects. They may thus cause sanguineous congestion, hæmorrhage, inflammation, œdema, gangrene, and atrophy of the pulmonary texture, hæmoptysis, profuse bronchial secretions, effusion into the pleura, disease of the heart, and so forth; and the symptoms which these lesions produce may be variously grouped in the history of different cases of phthisis. The hæmoptysis occurring in the early stages of consumption is generally from this cause; and it is a serious symptom, not only because it may endanger life by the loss of blood, or direct suffocation, but also because it is often accompanied by hæmorrhagic consolidation and rupture of the textures of the lung, which tend to accelerate the disorganizing process, and promote the further deposition of tubercle. In some instances, however, hæmoptysis is followed by decided relief to the dyspnœa and cough, having removed a congested state of the vessels. The *obstruction to the motion of the lungs* may be caused by the same circumstances that impede the free admission of air to them, but in case of extensive solid deposition may also result from their mechanical resistance to the respiratory movements; and this not only constitutes a physical sign which we shall consider afterwards, but it also keeps the intervening unaffected texture in a fixed state, liable to consequent congestion and further deposit, and adding further to the incapacity of the organs. When once the integrity of a nicely adjusted apparatus like that of respiration is extensively injured, you see how the mischief becomes extended; disorder begets disorder; and unless the counteracting or reparatory powers come soon into action—unless the indurations are diminished, or the vessels blocked up—the whole of that part of the lung may soon become a solid mass. Thus, I believe, sometimes arise those extensive masses of induration that are so commonly met with in the superior parts of the lung.

Second stage.—On the conversion of the grey or dark red indurations into crude yellow tubercle, and the original deposition of this matter, in addition to the symptoms of irritation and obstruction, which still continue, there are indications of an increasing cachexia, a depression of the functions. The pulse loses strength; the fever is of shorter duration; and the sweats are more profuse; except at times of excitement, the colour of the cheek fades, or is reduced to the circumscribed hectic patch; the expectoration becomes more abundant, or less thin and transparent, and particles of curdy or cheesy matter can sometimes be detected in it. But all these symptoms more properly and fully belong to the

Third stage, in which the tubercles become soft, partially or entirely liquid, and are evacuated by the aid of the secretion and ulceration of the adjoining textures. Then comes on, in addition to the other symptoms, the copious and heterogeneous expectoration of pus, mucus, softened and occasionally solid tubercle, blood, shreds of lymph, rarely portions of pulmonary tissue, sometimes very fœtid. Then follow the usual constitutional concomitants of extensive unhealthy suppurating ulcers, confirmed hectic and marasmus, rendered here more pronounced by the importance of the organ affected, and the relations which it bears to the process of sanguification. Then are the dyspnœa and cough increased by the continual discharge of matter into the air-passages, and by the extension of the diseased depositions and ulcerations of the tissue. Yet it is a curious circumstance, that these symptoms are often inconsiderable in proportion to the terrible extent of the organic mischief that has been produced: the dyspnœa is not painful, like that of asthma; it is a state of breathlessness, rather than of distressing oppression; the cough is not so violent as in chronic bronchitis; the pain may be but slight, or none at all; the countenance, though thinned, hectic, and with the sharpened nostrils habitually moving at every breath, may have a clearness in it, with a colour in the lips and a brightness in the eye, which are never seen in other diseases; and the frame of mind is often in the same unconscious and hopeful state, indicating a degree of freedom from those painful struggles in which the vital powers commonly contend with other serious disorders. Now I apprehend the chief reason of this exemption from suffering lies in a sort of balance that is produced among the injured functions. The available part of the lungs is reduced to a surprising extent; but so is the mass of blood that has to pass through it. The free expectoration and the colliquative discharges are continually bringing down the bulk of the circulating fluids to the lessening capacity of the remaining lung. The night-sweats especially are a periodic discharge of the amount of fluid which is beyond what the reduced system of blood-vessels can quietly hold; they generally cease when the fluid ingesta are carefully reduced. So the secondary pulmonary irritations, congestions, and inflammations, are continually relieved by the purulent expectoration; it is a safety-valve which gives vent to these before they can

cause distress; and although the destructive process is here perpetually proceeding, the lungs are decaying, the body consuming, and the strength failing, yet it is all by even degrees, a *facilis descensus;* the thread of life dwindles away, fibre by fibre, without struggle or shock; and gentle is the parting of the last filament, when the body drops to earth and the soul rises to eternity.

But the progress of consumptive disease is not always thus painless and unharassed: the sufferings from dyspnœa, cough, pain, heat, and feelings of extreme weakness and sinking, are sometimes very severe. There are those whose animal sensibility is more acute than their organic life is active; to such persons any disorder is distressing; and, even in consumption, the cough, the pains in the chest and sides, the alternate chills and heats, the oppression of dyspnœa, the faintness of debility, besides innumerable pains and aches of various parts of the body, are perpetual sources of complaint. But without an unusual degree of sensibility in the subject, the course of consumptive disease may be rendered rough and painful by what may be called the accidents or irregularities contingent upon it. Intercurrent congestions, hæmorrhages, and inflammations, taking place in the lungs or their investing membranes, may give rise to symptoms of these several acute lesions, superadded to those of phthisis. I have said that a free expectoration tends to prevent these accidental complications; and, accordingly, their occurrence is often preceded or accompanied by a suppression of this discharge or an alteration in its quality*. But there is an accident which especially tends to ruffle and hasten the course even of the quietest forms of consumptive disease; this is perforation of the pleura, and the consequent pneumothorax and acute pleurisy which it produces. As I have already described this lesion, and the symptoms which it induces, I need not now dwell on it; but I would remind you how characteristic the sudden increase of dyspnœa and accession of sharp pleuritic pain must be in the cases that were before the most insidious, and how much the addition of these lesions must increase the distress of the patient, and hurry him towards his grave. I think that perforation of the pleura occurs generally in cases where the constitution is decidedly tuberculous. It implies a want of that self-preserving energy by which the mischief of ulceration is limited by the timely effusion of plastic lymph. This is a part of the nutrient function, and I suppose this is commonly more active in women than in men; for if I can judge by my own experience, perforation of the lung very rarely occurs in females: I have never met with one instance, yet I have seen at least twenty cases in the male sex; and, as far as I can recollect, the cases recorded by authors are chiefly of that sex.

Other symptoms unconnected with the chest occasionally attend pulmonary consumption. The larynx is very often diseased in phthisis; the hoarseness or loss of voice being an early symptom, and taking attention from the real and chief seat of disease. The lining membranes of the larynx, and the vocal ligaments, are in such cases thickened, eroded, and ulcerated, and the cartilages and bones may ultimately become carious or necrosed. There is even in these lesions something of the intractable character of tubercular ulceration; therefore beware of *laryngeal phthisis*, for if it be not already complicated with tubercles in the lungs, they may be formed at any time, for the leaven is in the system. The disorder of the digestive organs, which, in the earliest (the irritative) stage, had something of the gastritic character, with red-tipped tongue, thirst, and indigestion, perhaps some tenderness in the epigastrium, commonly passes off when the pulmonary irritation is relieved by the discharge. At this period, however, the bowels often become disordered, and constipation and diarrhœa alternately prevail, dependent on inflammation and ulceration, often complicated with tubercular depositions in the follicular structure of the mucous membrane of the intestines. The mesenteric glands, too, become frequently involved in similar disease; and thus arise additional causes of exhaustion and atrophy in the colliquative discharges, and the obstruction to nutrition that ensue. The character of the alvine secretions sometimes shews a deficiency of bile; but this is a symptom which more frequently precedes phthisis than accompanies it. Sometimes there are great tenderness and even pain in the abdomen during the whole course of the disease, with occasional exacerbations: these symptoms generally depend on granular or tuberculous depositions on the peritoneum, combined occasionally with inflammation of the membrane. More rarely, tubercles occur in the brain or spinal marrow, or their membranes, and cause symptoms of mental disorder, convulsions or paralysis. Acute hydrocephalus seems to have some further connexion with scrofulous or tubercular disease; for it sometimes co-exists with tuberculous depositions in other parts, although there

* Dr. Stokes has made similar observations with respect to the influence of expectoration, in relieving the local symptoms of phthisis.

be none in the brain or its membranes. I cannot now dwell on the details of other symptoms arising from the irritations or obstructions, the weakness and the wasting, which tubercles bring in their train. The emaciation in the last stages is great, but that from scirrhus of the stomach, or tabes mesenterica, is greater. There is a blanching with the emaciation, that is more remarkable than its degree; the blood-vessels are reduced, as well as other textures, and you scarcely ever, in tuberculous consumption, see the redness of the knuckles and distinctness of the veins which accompany even greater degrees of emaciation from chronic diseases of the abdomen.

Let us now pass on to the consideration of the physical signs of pulmonary consumption, tracing them in the different stages of the textural lesions which we have described. In proportion as these lesions are of great or small amount, or are concentrated within a small space, or are scattered widely through the lung, they will produce more or less appreciable signs. Thus the miliary indurations, even in considerable number, may be scattered through the tissue of the lung without producing any distinct diminution or change in the respiratory sound, or in the resonance of the chest on percussion. Sometimes there is a general sub-mucous or sub-crepitant rhonchus; but this proceeds less from the tubercles than from the secretion which their irritation causes in the adjoining bronchial tubes: it is the sign of a partial bronchitis, therefore, and can only be taken in evidence of the probable presence of tubercles, when it continues permanently day after day, instead of tending to become sibilant, and to pass off as the rhonchi of common bronchitis do. But it seldom happens that even the early indurations are equally scattered through both lungs. Their tendency is to accumulate in greater numbers or in little clusters near the apices of the lungs, and generally more on one side than on the other. Here, then, will be a concentration of effect, and a difference between the two sides of the chest; and if we explore the corresponding region on the exterior, which are the clavicles and the space immediately beneath them, and the upper ridge of the scapulæ, we may find differences in the sound on percussion, or those of respiration, and the voice, which, according to known principles, may be interpreted as signs. The clavicle on one side, when struck lightly downwards on its middle, yields a sound duller than that on the other; and especially if this difference extends to the other parts just mentioned, it is exceedingly probable that there is tubercular consolidation of the lung in that part. Great care must be taken to strike both clavicles at the same point, or both infra clavicular spaces in the same mode, or the comparison will not be a fair one. You should, to avoid error, have the parts quite uncovered, and tap either directly, or on a finger, on corresponding points of both clavicles. Below these bones, gentle percussion with the flat fingers, mediate or immediate, generally succeeds best; but various kinds of percussion should be tried in doubtful cases, and in different stages of the respiratory act—on a full breath, and after exhausting the lungs. You are to remember that the consolidations may be merely granular and scattered, and that they would not have body enough to affect the vibration of the walls, if the stroke of percussion be hard: nay, sometimes the gentlest possible *patting* of the space under the clavicles is the only mode in which any difference can be detected. In the posterior region, however, and above, on the scapular ridge, considerable force is required to get any pectoral sound at all, and this must be used by mediate percussion on a finger laid along it; comparing, as usual, the two sides. Always seek for differences in the sound where differences ought not to exist; and vary your mode, place, and force of percussion, at different times; but let these be carefully the same in each act of comparison. When the infra-clavian spaces fail to give any signs of disease, try the parts below, at the sides, and in the back. Between the scapulæ is not an unfrequent seat of dulness, especially in children, where the disease occupies the bronchial glands.

You should not omit to use your eyes, too, in scrutinizing the condition of the chest, by making the patient breathe fully, whilst you stand in front, in a good light, and watch and feel its motions. It requires more consolidation than is usual, in the doubtful stages of the disease, to produce any considerable irregularity in the shape or movements of the chest; but we can often perceive a slight difference between the two sides: the upper ribs do not move quite so much on one side as on the other. The collapse of the infra-clavicular space, so remarkable at later periods, does not take place at this stage.

The stethoscopic signs are more delicate, and perhaps more equivocal, than those of percussion. The indurations may form slight partial obstructions to the passage of air, and thus cause a permanent slight wheezing, whistling, or roughness in the respiratory sound, not removed by full inspiration or cough.

If more numerous or extensive, they may transmit the sound of whiffing, bronchial breathing in parts where naturally the vesicular only is heard, whilst the soft vesicular breathing is impaired in its force. The sound of expiration may become unusually audible, and sometimes equals that of the weakened inspiration, which you know is almost the only sound in purely vesicular respiration. When, as it often happens, the partial indurations are accompanied by a dilated or emphysematous state of the neighbouring air-cells, the sound on percussion will be less altered than usual; but the breathing will be more whiffing, or more obscure, according as the dilated cells are more flaccid, or more rigid than usual. When more rigid, they diminish not only the sound of respiration, but also the motion. The sound of the voice is transmitted by the indurations in an unusual degree, being sometimes only a diffused resonance, but sometimes quite a little voice under the clavicle; but without the articulation of the oral voice. So also the sounds are occasionally transmitted with uncommon clearness from the subjacent arteries, being either double - from the second sound of the heart, or single from the mere impulse; and now and then the single pulse is accompanied by a whizzing or blowing, confined to that part indicative of partial obstruction of the subclavian artery, from the pressure of the indurations at the apex of the lung.* I have known it to intermit regularly, occurring only in two or three pulses during the first part of expiration.

Now all these stethoscopic signs derive their importance directly from the situation in which they are heard, and from their comparison with other parts. There are often bronchophony, you know, and bronchial respiration, naturally, near the sternum, between the scapulæ and in the axillæ; therefore you must not place confidence in such signs in that neighbourhood, unless they be much more distinct on one side than on the other; or accompanied by dulness on percussion. You may trust to them more towards the humeral end of the clavicle; the angle formed by this bone and that of the shoulder is the proper stethoscopic corner, and the signs heard there are the most unequivocal; but even here a permanent discrepancy between the two sides gives the surest indication of disease; for there are many modifications of the natural sounds heard there.

I must not disguise from you that this is a very difficult point of physical diagnosis, and requires much experience and nice perception on the part of the auscultator. But you are not on this account to be discouraged from attempting it; on the contrary, it the more deserves and requires your study; and if you will only devote proper attention to the method, and take every opportunity of practising it, you may in time acquire the means of detecting phthisical disease in its earliest and least intractable stage, and you may thus be enabled to save many valuable lives from otherwise inevitable destruction. You must not neglect attention to the general symptoms; for besides the assistance which they give in the diagnosis by the character of the cough and pulse, they are, as I have often told you, of the utmost importance in guiding the practice, which is to be adapted to the state of the whole constitution as indicated by them, as well as to the local disease, which the physical signs alone may have discovered.

The cases which we have to deal with are commonly those in which there is a cough, which, from its duration, its unusual character, or from its being a new symptom in the subject, excites some apprehension. Is this a common cold? Is it a "nervous cough," a "stomach cough," or a "liver cough?" Or is there a permanent cause of irritation in the pulmonary tissue itself? We first endeavour to answer these questions by physical examination. If there be no tuberculous disease there will be none of the signs that I have just been describing, but those merely of bronchitis or catarrh; various rhonchi, sibilant, sonorous, and mucous, in various parts of the chest, generally in the middle regions, where the large bronchi pass; not so much in the upper parts, and never confined to them. Then you may inquire whether there are other symptoms which may justify you in considering the bronchial irritation as secondary, and referrible to an excitable nervous system, to a disordered stomach, or a congested liver; and if you find them, well and good; act accordingly. But my experience has led me to look with suspicion on all coughs which last an unusual time, until by repeated examination I have found no cause for them in the lungs. Many a consumptive case has been dressed up, even to its last stage, in the delusive names "liver" or "stomach coughs."

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* This subclavian arterial murmur has been noticed by Dr. Stokes as a sign of incipient phthisis. I have never met with it except in cases where percussion and the other signs indicated a considerable extent of consolidation.

LECTURES ON BLOOD-LETTING.

Delivered from time to time,

At the General Dispensary, Aldersgate Street,

By Henry Clutterbuck, M.D.

Formerly one of the Physicians to that Institution.

Lecture VII.

On the Use of Blood letting as applicable to different Diseases.

Having pointed out, as well as I could, the general effects of blood-letting as a remedial agent, it remains for me to shew its application to diseases of different denominations; and here I must recal your attention to what I before stated, namely, that there are few diseases in which, on certain occasions, this remedy may not be had recourse to with more or less advantage. As, however, it is not equally adapted to all, nor even to the same disease at different times, and under different circumstances, it becomes necessary to speak of it more in detail, for the purpose of pointing out the various circumstances which are to guide us in the use of this powerful, though far from universal means of cure. To know and to understand these thoroughly, and to be able to apply them justly, constitutes in fact the true art of physic, and is the only real test of practical skill.

Among the diseases to which blood-letting is particularly adapted, inflammation undoubtedly claims the first place, for it is here that the power of the remedy is most strikingly displayed, and its employment the most frequently called for. But even here the exceptions and qualifications are so numerous, as to require particular attention. Inflammation is a subject of the widest range, and involves in its consideration that of most other diseases. There are very few, indeed, and these of comparatively small importance, that either do not consist absolutely in inflammation, or which are not consequences, more or less remote, of this affection. To be convinced of this, you need only look attentively at the list of diseases furnished by writers on methodical nosology. In doing this, however, you must be careful not to be misled by names; for these seldom give us any information in regard to the real nature of the disease spoken of. You must also disregard what are merely symptoms, for in numerous instances these have been ranked as distinct species of disease. When I mention the terms hydrocephalus, hydrothorax, anasarca, gout, rheumatism, catarrh, diarrhœa, dysentery, &c. (all of which consist in inflammation of different structures, though the names do not at all imply this), you will readily see the grounds for the assertion just made.

In order to the better understanding the use of blood-letting in the treatment of inflammation, it is desirable to inquire a little into the nature and consequences of this disease; for it is a subject about which widely-different, and even opposite, opinions still prevail among practitioners. It is often asked, "What is inflammation? In what does it essentially consist?" These are questions to which, I fear, no very satisfactory answer can be given. We know it only by its effects, and by comparison with the healthy state. It is even difficult to give a strict definition of the term, that is, to give in the compass of a few words such a character of the disease as will enable us at all times to recognize its presence. Some of the characters of inflammation are obvious and striking, so as not easily to be mistaken; they have accordingly been noticed from the earliest times. Such, for example, are the cardinal signs "redness," "heat," "pain," and "swelling." Wherever these co-exist, we may be sure that inflammation is present. As, however, any one of them, and even more than one, may exist without inflammation; and as such signs are not always open to observation (as where the disease is situated internally), it becomes necessary to resort to other means of ascertaining the existence of the disease. The most satisfactory are, perhaps, the local signs of hardness and tenderness on pressure, (supposing the part to be within the reach of manual examination), with impeded or disordered function; and, above all, if present, a febrile state of system *(pyrexia)*, indicated chiefly by heat of skin, furred tongue, and thirst—symptoms which, alone and of themselves, always serve to shew the existence of inflammation. With due attention to these different signs, there will be little risk of overlooking the disease, wherever seated.

The intrinsic nature of inflammation (by which is to be understood the physiological condition of the inflamed part) is a point about which opinions are as unsettled at present as at any former period. Indeed, the most opposite views appear to be entertained on the subject; and certainly not without detriment to practice, which can hardly fail to be more or less influenced by them.

One party, I believe a large one, at the head of which stands the late John Hunter, maintain that inflammation con-

sists in, or, at all events, is always accompanied by, increased vascular action in the part. This is supposed to be proved by the more forcible and rapid movement of the blood through the part, as shewn by the enlargement of the veins coming out of it, and the more rapid flow of blood from them when punctured, as well as by the increased pulsation of the arteries leading to and entering the part; and if to this be added the enlargement or new growth of the solids, and the production of new fluids from it, in the way of secretion, together with the increase of sensibility and of irritability which so generally charaeterize inflammation, wherever seated, it seems difficult to question the increased vitality and activity of an inflamed part.

There are those, however, and not a few, who espouse the opposite opinion, contending that, although it cannot be denied that the larger arteries going to an inflamed part are acting with preternatural violence, the capillaries are in a debilitated state, and the circulation through them retarded*. There are others, again, who take, as it were, a middle course between the two extremes, and maintain that, in the first stage of inflammation, the capillaries, as well as the arterial trunks from which they are derived, are in a state of increased action, thus giving rise to the increased circulation observable in and about the part; but that, in the further progress of the disease, the vital contractility of the capillaries is exhausted, and the circulation through them in consequence retarded, or altogether stagnant.

To me, I confess the opinion of Mr. Hunter, viz. that in inflammation there is always an increased action of vessels, appears the most consistent with the obvious phenomena of the disease, as pointed out above. The contrary opinion, however, pretends to be founded on demonstration; and the microscope is appealed to in support of the fact; the web of the frog's foot, and the mesentery of the rabbit, in an inflamed state, having been made the subjects of examination, when the red particles of the blood are seen, it is said, to be retarded in their course, and sometimes even stagnant.

I hardly need remark how fallacious are all conclusions drawn from the microscope alone. Different observers see different things. One of these, Kaltenbrenner, asserts that he saw, in the mesentery of a rabbit that was submitted to the microscope, globules of blood bursting through the sides of a vessel, and forcing a passage for themselves through the cellular tissue into another vessel; thus forming new canals, through which the circulation was kept up! In fact, when you consider the nature of the subjects submitted to observation in these experiments, the minuteness and transparency of some parts and the opacity of others; added to the natural imperfection of the instrument, and the different faculties of different observers; it is not easy to place implicit confidence in such reports, however faithfully made, especially when opposed to the more obvious facts above stated.

But although it be admitted that the vital activity of an inflamed part greatly exceeds that of health, it is not simply an increase of the natural action of the part that is taking place, but one of a morbid or preternatural kind; producing, as might be expected, preternatural results. The structure, as changed by inflammation, is different from healthy structure; the feelings and actions of the part are all of a morbid kind; and the fluids formed by it, in the way of secretion, are likewise morbid. So far we may go with safety; but of the precise or intrinsic nature of the action which constitutes inflammation, and which gives rise to such surprising changes, we are wholly ignorant. And the consequence of our ignorance is, that we cannot treat the disease theoretically—that is, in accordance with the presumed nature of the disease—but rather empirically, being guided almost wholly by the light of experience and observation. We are not, for example, on the one hand, to stimulate or excite either the inflamed part itself or the general system, with the view of obviating the debility supposed to constitute the proximate cause of the disease; nor, on the other hand, are we to use blood-letting, or other debilitating means, for the simple purpose of lessening the violence of action. Yet both stimulants and blood-letting are highly useful at times, in the treatment of inflammation, each according to its appropriate circumstances; as I shall endeavour to show you hereafter.

But although so little appears to be known of the intrinsic nature of inflammatory action, we are nevertheless sufficiently acquainted with some of its most important characters, as well as the laws which regulate its course, as to be able to deduce from them practical inferences of considerable value.

In the first place, then, I may observe, with regard to the progress of the disease, that inflammation is seldom stationary; it is almost always either increasing or diminishing in violence, as it proceeds. In

* This notion, as observed by Mr. Morgan, of Aberdeen, in his First Principles of Surgery, &c., was first published by Berlinghierri, of Pisa, as long ago as 1765, though it has of late been given out as a new doctrine.

most cases it sets out mildly, so as often to escape notice for a time — becomes more and more strongly marked as it proceeds, till it arrives at a certain point, when it begins to decline; and so continues to do, till it wholly terminates. This course it pursues spontaneously, without the aid of art, and even sometimes in opposition to it—that is, where the treatment altogether is not the most judicious.,

A great number of cases of inflammation might thus be safely left to pursue their course; nothing more being necessary than removing, if possible, the exciting cause, and avoiding every thing that is likely to aggravate the disease; and indeed, a large share of ordinary medical practice is, or should be, so conducted. But as we are seldom able to estimate justly the degree of disposition actually present — in other words, the tendency there may be in the disease to subside in any particular case, prudence obliges us, as the safer course, where important organs are concerned, to have recourse to active treatment, though it may be productive of present inconvenience to the patient.

The disposition of inflammation to subside of itself is, however, neither universal nor equal, but differs greatly in different cases. There are some inflammations that have little or no tendency to go off of themselves, yet will yield to artificial means of cure. Such is the case with some of those termed *specific*, of which the venereal is an example. There are others again, that neither cease spontaneously, nor are capable of being removed by art; as in the instance of carcinoma.

The disposition varies greatly also in different constitutions; so that the same disease proves more difficult of cure in some individuals than in others; and as the difference can seldom be ascertained before hand, we are never certain of success.

The time required for inflammation to run its natural course depends, in some measure, upon the violence of the disease. The more acute it is, the more rapidly, in general, it goes through its different stages. More appears to depend, however, upon the nature of the organ affected. Compare together, for example, the progress of inflammation in the lungs, with that of ligamentous inflammation in the form of acute rheumatism, and the difference you will perceive to be very striking: the former often running its course to complete disorganization within the space of a few days, while the other continues with violence for many weeks, and at length subsides without injury to the structure of the part affected.

There is considerable regularity observable in the periods of duration in inflammation, varying, however, in different organs. This has been most noticed in the case of *idiopathic* fever (so called) (cerebral inflammation); but the same tendency to observe certain periods was taken notice of by the ancients* with regard to other inflammations, and might be more clearly seen in the present day, but for our constant use of herculean remedies, by which the natural course of disease is disturbed, and often interrupted. You will find it of some advantage to become acquainted with the natural course and duration of different inflammations, as you will be thereby enabled to form a more correct prognosis, with regard to the probable termination of the disease, than is otherwise possible.

Another most important character of inflammation is its constant tendency to change the structure of the part affected, altering, at the same time, both its physical and vital properties. It is, in fact, essentially a disorganizing process, and is probably the primary cause of all morbid changes of structure. Hence the danger of continued inflammation, however mild, in organs, the functions of which are essential to life; and consequently the necessity for suppressing it as early as possible, before it goes to such a length.

In the application of blood-letting to the cure of inflammation generally, we shall have to attend more especially to the following points:—The degree of the disease; the stage of it; the part or structure which it occupies; and the kind or nature of the inflammation, as *specific* or otherwise; for all these have a material influence on the employment of the remedy.

MR. MAYO ON MESMERISM.

To the Editor of the Medical Gazette.

Sir,

The subject of mesmerism attracts at present an attention so general, that I venture to address to you a fourth letter upon this subject, with the purpose of communicating to your readers additional facts of which I have been an eye-witness. It is perhaps unnecessary

* This may be seen by reference to the writing of Hippocrates, who, as a general rule, limits the use of blood-letting in inflammation of the lungs to the fourth day, the period of crudity; and he cites, as extraordinary, the case of Axonion, whom he bled as late as on the eighth day (the day when the crisis usually takes place.) The crisis, or termination, in this case, did not take place till the twenty-fourth day, and then by expectoration, sweat, and urine.

to remark, that to Dr. Elliotson the whole credit is due of having been the first in our own country successfully to explore this vein of physiological inquiry. My own mind, I will frankly confess, was so strongly prejudiced against the idea of any new principle operating in "animal magnetism," that if I had had sufficient leisure to devote to this investigation, I certainly should not so have employed it. Even when I had seen, being almost accidentally present at one of Dr. Elliotson's public demonstrations, that a principle perfectly new to me was brought into action, it was only Dr. Elliotson's most liberal readiness to shew the phenomena under any suggested modification of the circumstances, and to try himself, or to allow others to try, any new and safe experiments over and above his own comprehensive series, to which he is daily adding, that it occurred to his visitors to propose, that tempted me, by the facilities thus placed within my reach, to sift these wonderful phenomena. And in giving this account of the phenomena which I have so had the means of witnessing and of verifying, I trust that I am in some degree returning to Dr. Elliotson the obligation which I owe him for those opportunities of observation which he has afforded me. Because I know that he is censured, inconsistently enough, for his credulity on the one hand, and on the other for the kind of publicity which he has endeavoured to give to the facts he has elicited.

I am glad, in his defence against the first accusation, to make it manifest that if he is credulous, he is not distinguished for credulity on this occasion from all other scientific pursuers of our common profession; and at the same time to express my opinion, that in exhibiting his patients publicly, he has adopted the only means by which he had a chance (I do not say of establishing the truth of), but of gaining any thing like general assent to, the marvellous principle the verification of which he has come upon. What the utility of that principle may be remains of course to be proved; it may or not appear in the end as great as it now promises to be in a few of the most difficult and baffling cases of nervous disorder; but if it were only for rescuing out of the hands of empirics a means of such extraordinary power as mesmerism, by developing its real agency, the public would be deeply indebted to the spirited physician to whose merits I am happy to bear my testimony on this occasion.

Having heard the present subject frequently canvassed in society, I have been amused at the different forms of disbelief, and with the different modes of becoming persuaded of the general truth of mesmerism, displayed by different persons.

The arguments which I hear advanced against mesmerism are commonly the following:—

First, that *the instances in which it is maintained, that a specific influence is demonstrable, are so few.* But it is forgotten that similar effects to those now obtained by an English physiologist, upon English patients, have been obtained during a series of years in France and Germany; and that it requires the repetition of the manipulations daily for months to bring out all the phenomena, even in persons who seem the most susceptible.

Secondly, that *Dr. Elliotson exhibits the same persons over and over again.* It is, however, unreasonable to deny him this privilege, if the cases, so repeatedly shewn, best exemplify the mesmeric phenomena; and it is not true that the two patients referred to in the above remark are the only persons exhibited, as I have myself seen five others produced in the theatre of the North London Hospital.

Thirdly, that *the facts shewn are phenomena of hysteric disease.* This statement is untrue, and evinces that the party making it either has not seen the phenomena adverted to, or has not seen hysteria.

Fourthly, that *mesmerism is an old story, like the philosopher's stone, which, though taken up occasionally by credulous people, as often and as surely after a time falls into disrepute and is forgotten.* My answer to this objection, which is the best that I have heard, is the following:—Before the present time the genuine effects of mesmerism had not been obtained in a pure state, that is to say, in a state free from admixture with, or leading to their being distinguished from the results of hysteric disorder, or the agency of the imagination. Before the knowledge of the subject had advanced thus far, men of sense who were invited to witness the phenomena, seeing that some of those

exhibited as mesmeric were evidently referable to the workings of the patient's imagination, naturally, rather than admit a new principle, concluded that this would probably explain the rest. The perplexity arising from the source to which I now advert, and the prejudice resulting from the systematic imposture with which the practice of mesmerism had been tainted, had their full weight with Cuvier, as appears by the following passage from the *Anatomie Comparée*, in which that great authority expresses his own belief in mesmerism:—

"Pour terminer ce tableau rapide de l'action du systême nerveux, il faudroit indiquer aussi l'action que les systêmes nerveux de deux individus différens peuvent exercer l'un sur l'autre. L'abus qu'en ont fait les charlatans, et l'exagération avec laquelle ils en ont parlé, l'ont tellement décriée, qu'il est presque interdit aux philosophes d'en parler.

"Il faut avouer qu'il est très-difficile, dans les expériences qui l'ont pour objet de distinguer l'effet de l'imagination de la personne mise en expérience d'avec l'effet physique produit par la personne qui agit sur elle, et le problême se trouve souvent très-compliqué. Cependant les effets obtenus sur des personnes déjà sans connaissance avant que l'opération commencât, ceux qui ont lieu sur les autres personnes après que l'opération même leur a fait perdre connaissance, et ceux que présentent les animaux, *ne permettent guères de douter* que la proximité de deux corps animés, dans certaines positions, et avec certains mouvemens, n'ait un effet réel, *indépendant de toute participation de l'imagination d'une des deux.* Il paroît assez clairement aussi que ces effets sont dus à une communication quelconque qui s'établit entre leurs systêmes nerveux *."

I build the belief, which I fully entertain, that mesmerism will not again fall into discredit, upon the circumstance, that now in many of its phenomena there is recognizable the working of a determinate physical influence, the laws of which are susceptible of, and are actually receiving, inductive development. I build my confidence further upon this circumstance, that mesmerism is now beginning to be investigated by English physicians and surgeons, and that there is a force of good sense and genuine science, the thing, perhaps, more than the pretension, in the followers of Harvey and Hunter, which enables them, in their own studies (like the followers of Locke in metaphysics), to obtain just and philosophic conclusions in fields of inquiry where some of their neighbours become bewildered in profitless and visionary speculation.

But although English physiologists may succeed in working out this problem, it does not follow that the English public, which habitually assumes a right of decision on all cases of medical science, without regard to or even examining the evidence adduced, will be in a hurry to receive its demonstration at their hands. I think, indeed, that I have observed in society that the opposers of mesmerism are not equally distributed through all its classes. It appears to me, that those who are most against its reception are to be found among literary men. The philosophers express no bigoted refusal to examine mesmerism; but, on the contrary, assert their readiness, on sufficient proof being advanced, to admit its doctrines. The former soon become inextricably entangled in the mass of absurdity with which, since its origin to the present time, mesmerism has been encumbered; the latter are sensible that although much that has hitherto been claimed for it may be false, the inquiry may nevertheless lead to the establishment of some important truth. The former are disposed to have no thought on the subject beyond considering such phenomena semi-miraculous, and contrary to and violating the laws of Creation; the latter are accustomed to conceive the discovery of principles entirely new as within the legitimate scope and expectation of science.

It is not less curious to observe, that the grounds upon which believers have been convinced of the genuineness of these wonders are not always the soundest.

The experiment by which I have seen many persuaded is, the insensibility displayed by some of Dr. Elliotson's patients, when in the state of somnambulism, to strong electric shocks. This is amusing enough. Because that state of insensibility, wonderful as it is, and before unobserved, is

* Leçons d'Anatomie Comparée, vol. ii. p. 117, edition 1805.

not the result of mesmerism, but belongs to peculiar fits, which patients *who have never been mesmerized* exhibit.

Another experiment, which has been influential on the belief of many, is the catalepsy into which the mesmerized patients admit of being thrown, and in which any one extravagant and difficult posture is maintained for so long a period, that no force of determination, it is supposed, could enable the patient to continue it, if it were voluntary. With myself this argument has less weight. Why should not practice and resolution enable a person, bent on maintaining a deception, to keep up any exertion, of which the body is physically capable in any state?

Those experiments alone, it appears to me, should be viewed as conclusive, in which a sensible change in the condition of the patient, either from consciousness to unconsciousness, or from one posture of the body to another, are produced by movements on the part of the operator, at a time when the back of the patient is towards him, or such other means taken as totally to prevent the possibility of the patient receiving any intimation through the ordinary avenues of sensation, of the moment when the expected change is to be exhibited by him. Having now, in numerous instances repeated, or seen repeated, successful mesmeric experiments, in which these conditions were observed, I am certain that a special physical influence is called into action through the mesmeric manipulations.

The facts which I have it in view to mention in the present letter, refer to the different classes of effects produced by mesmerism; to the source of the mesmeric influence; to the laws of its transmission and reflection; to mesmeric prevision, and clairvoyance.

I. The conditions of the system produced by mesmerism, which I have witnessed, may be grouped in the following order:—

1. Paroxysms of hysteria, muscular twitchings, and sense of pricking in the parts towards which the hand of the operator has been directed; difficulty of raising the eyelids; a sense of weight across the forehead; drowsiness, and dropping off for a few seconds into something like common sleep, sometimes followed by a continued torpor; and with inability to stand, the weakness of the legs lasting many minutes. Either of these often constitutes the entire effect, which can be produced in different individuals, by the mesmeric manipulations. But the last, the production of a short sleep, or something like it, renders it probable that in the person so affected with time all the subsequent effects may be brought out. In the most susceptible, this is the first effect.

2. *Mesmeric coma*, characterized by complete muscular relaxation, and seeming total insensibility; but the pupils, as in one awake, and contracting on the admission of light; the breathing gentle; the heart's action slightly quickened.—[Vide Med Gaz., April 21, 1838.]

3. *Mesmeric somnambulism*, for which the term *mesmeric delirium* had better be substituted. This state may be considered the same with the state that alternates with the natural state in double consciousness, and probably has some near resemblance to common sleep-walking. When following coma, it is characterized by the return of consciousness, attended with a more or less vivid expression of satisfaction on the part of the patient. The patient has a manner more animated than natural; her recollections are imperfect; her voice and articulation changed. Her senses are only partially restored; hearing and vision, and the muscular sense, have, indeed, returned; but taste, smell, and touch, are absent. This is the state of Jane Okey, in the North London Hospital, on awaking from coma. In Elizabeth Okey the restoration of sensation is less complete. It is doubtful whether this patient regains the muscular sense; and the scope of her vision appears to be as limited as if she looked through a tube. I have seen Elizabeth Okey go into this state out of her natural state; and Jane Okey was liable to fall into fits of this kind before her admission into the hospital. Of the different patients that I have seen on these occasions, the two Okeys alone have exhibited this delirium.

4. A state, which I have before described as a *half stupor*, to which the term *mesmeric trance* had better be appropriated. This state I have only seen produced in the two Okeys, and in a little girl of fourteen, with the christian name of Hannah, whom I have mentioned in a former letter as having been cured, under Dr. Elliotson's care, by mesmerism of nervous paraplegia.

In the two Okeys the trance is brought on, when they are in the state of coma, by rubbing, once or twice only, the eyebrows; when in the state of delirium, by using half the manipulation necessary to induce coma. In Hannah, who never exhibited the proper state of delirium, the condition now under consideration was brought on by half waking her from coma, a result which is equally obtainable upon the two Okeys.

In the mesmeric trance, the patient is motionless, but is capable of supporting herself in any posture; her senses seem at first as completely suspended as in coma. Yet hearing certainly remains, and seems even more acute than natural, and though the eyes seem fixed on vacancy and half closed, yet the pupil is natural; and I think that some of the phenomena that I have seen produced in this state are assisted by vision. The trance is the most important in every point of view of the different conditions into which the patient can be thrown by mesmerism.

In the first place, it is the condition in which, if the experiment were ever justifiable (and circumstances are conceivable which would render it so), a surgical operation should be performed, the patient consenting to its performance, which, of course, would not be felt by him. In the second place, it is the condition in which the most satisfactory evidence can be obtained of the independence of the mesmeric influence on the imagination. The patient so entranced is in the highest degree of susceptibility. If she is placed in a chair opposite to a blank wall, and no one within several feet of her, movements directed towards her by any one at such a distance, and so exactly behind her that she cannot possibly see or hear those movements, are followed by sensible effects upon her. These same, it is true, are producible upon persons in the state of mesmeric delirium, but with much less certainty, for it is found that any thing that calls the attention, or happens to animate or excite the somnambulist, for a time completely defeats the attempt to produce in her mesmeric effects; and, of course, *telling her to be still*, if she can be induced to do so, is giving her an intimation to prepare to manifest some customary phenomena, and necessarily throws suspicion upon the result.

Another phenomenon which is displayed in the mesmeric trance is artificial catalepsy. This did not at first attract observation. The patient, who perhaps before sat talking in the most lively manner, being delirious, is now, having exchanged the state of delirium for that of trance, sitting perfectly still. Then, if you move your hand towards hers, and afterwards draw it away, her hand follows yours; the same with the foot, or with the entire person, which will rise, if you rise from a sitting posture opposite or behind her, and *remain fixed*, it would seem indefinitely, in the posture to which you have conjured it. For some time the permanence of the effect was not observed, for more and more motion was, as it were, put on, till, as the result of the increased excitement, the patient dropt into coma. I have seen Elizabeth Okey maintain one posture of considerable muscular exertion, in this artificial catalepsy, for upwards of a quarter of an hour. If her arms were forcibly bent and crossed when in this state, they reverted, on being let go, to the posture to which the mesmeric traction had drawn them. At the same time the posture admits of being permanently altered by renewing the mesmeric traction. On awakening the patient after the long period spoken of, she expressed no sense of fatigue or uneasiness, or recollection of recent exertion, though, for the time specified, all her limbs had been in rigid muscular action.

The cataleptic state has two artificial terminations; one is that just referred to: when, more and more movement being excited, at last the patient falls comatose. The other is effected by pointing the hands towards the cataleptic limbs, when, after a few seconds or minutes, they drop relaxed.

Finally, this state of trance is the condition in which the patient may be led to talk of herself, of the changes which are to follow in her health, and to predict their occurrence. Here it is evident that the imagination steps in to a greater or less extent, to furnish the patient with anticipations of what is to happen to her, or to be done for her relief.

II. The reader may recollect that, in my second letter, I have narrated experiments to shew that an intention on the part of the operator is not necessary to the production of mesmeric effects, and

that, from other experiments, it had been deduced that the only condition necessary was *free motion, or motion independent of efforts made to sustain or move any substances supported by the moving part*, made either towards or from, or in a direction crossing, the party mesmerized. It has been since ascertained that, in the experiments to prove the latter conclusion, there lurked a fallacy. The phenomena were indeed true as reported by me, but the induction was incomplete. It is found that, in moving any weight towards a person susceptible of mesmerism, *if the weight lifted does not cover the hands, and so intercept the influence*, the mesmeric effects follow equally as when the empty extended hand is moved. Free motion, as I had defined it, is thus not necessary. The voluntary motion of the hand when carrying a weight supported in it, is as efficacious as if it held nothing.

Further, it has been ascertained that *volition* even is unnecessary. If another take hold of my arm at the elbow, and wave it, my arm and hand remaining perfectly relaxed, before a patient in the state of mesmeric delirium, or mesmeric trance, the same effect ensues as if it were moved by my own volition.

Thus, neither intention nor volition are requisite, nor even muscular action, beyond that tone which is exerted in all living muscles. And of this it remains to be ascertained whether it be essential, or whether the mesmerizing influence proceeds in no degree from the muscles, but from the nerves of the integument or some other source. At present I only see evidence to shew that voluntary motion is useful secondarily. The effects, however, seem to bear a proportion to the quantity of motion of a living body in a certain relation to the patient.

Of the phenomena which have been already described, some appeared, indeed, originally to have no dependence on, or connexion with, muscular action; while others seemed as clearly to point to that as the cause of the mesmeric phenomena. I will make some further observations on both classes of instances. The first that I refer to are the effects of contact in sustaining mesmeric action. When two persons in mesmeric delirium have hold of each other's hand, and one is thrown into coma, the other quickly becomes comatose; or if one, in a state of delirium, takes the hand of one already comatose, she speedily drops into coma. Again, the two comatose patients holding each other's hands, as long as this contact is maintained it appears impossible to wake either by the manipulations that otherwise succeed; or if imperfect waking is produced, coma returns instantly: at the same time, if, continuing the experiment, contact is broken, either wakens instantly on the customary manipulation being used. Here, certainly, the mesmeric influence is not shewn to have any relation to muscular action: contact alone, in these cases, places it in activity.

On the other hand, the phenomena of mesmeric sequence seem all dependent upon muscular action. The muscles of the patient follow actions in the like direction made by the operator, either with the corresponding muscles of his frame or with others. If he sits behind the patient and opens his mouth, she opens hers. If with his hands in contact presented towards his mouth, he opens them from the wrist *gapingly*, her mouth opens; lifting a finger opposite her upper eyelid, raises it; moving the hand from side to side, causes her lower jaw to move in sequence.

All this seems easy enough to comprehend: one can imagine an influence emanating from the nearest muscle at the moment of its action, and dragging into parallel action the muscle of the patient, but it *seems* more difficult to imagine by what connexion of effects the following phenomena are produced. If you stand behind the person entranced, and fold your arms, she does the same; and what is far more curious, if, when standing behind her, you place your arms and hands in some complex and novel disposition, and keep them there, she seems to puzzle over the problem, and slowly and deliberately brings her arms into a position exactly like or coming near to your own.

It is to be observed that these phenomena, which are the most difficult to get out (even more, therefore, than the others), require that few persons should be near the tranced person, and that those who are present should be quiet and motionless. The proximity and motion of assistants often completely spoil, by interposing other influences, the effect you wish to, and would otherwise produce.

I have mentioned before, that several

persons combined together produce more effect than one; and this seems to be the case, whether they energize separately or united by holding hands, the last one only doing the manipulations.

In considering the source and nature of the mesmeric influence, the means of waking the comatose or delirious entranced patients must not be overlooked; and, in the first place, it is found that, for the most part, they display a greater or less tendency to wake spontaneously. The two Okeys generally wake in a few seconds or minutes, from the state of trance into delirium; but this is not constant. Hannah used to remain entranced till she was artificially awakened.

It has been mentioned that the readiest mode of wakening these patients out of the coma, or delirium, or the trance, is to blow upon the eyelids. Another method is to rub the eyebrows. One cannot help believing that the imagination has a great deal to do with this result. Elizabeth Okey, in her trance, has declared that she could be awoke by other simple manipulations, such as pressing both ears, which has then succeeded; and she has said that she could only be awakened by one person, and so it turned out. The imagination here must help the phenomenon. But it is strange that the slight pressure used on these occasions is effective, when the hand of the patient may be pinched, or pricked, or cut, without her feeling it.

The most interesting phenomenon of this class, however, is the following:—

When the patient is fixed cataleptically with both arms extended, it is easy to dismiss the catalepsy from either limb, as you please. This is done by holding the hand pointed towards the limb you would relax, and at the distance of two or three inches. In less than a minute, that limb drops relaxed. And in the same manner the other arm, or foot, may be set loose. The influence acts locally, just as the previous muscular traction had acted locally.

It is of course extremely probable that the spasm of natural catalepsy might be reduced by the same process; and I certainly saw, on one occasion, Dr. Elliotson seemingly cut short an epileptic seizure, by pointing the hand towards the patient's head, and making repeated gentle movements towards the neck, parallel to the face. The patient's former fits had always been of much longer duration than this; from which she recovered shortly after this manipulation was commenced. On another occasion, on which Dr. Elliotson attempted to restore an epileptic person, by rubbing the eye-brows, the attempt failed entirely, though the patient was Jane Okey.

Jane Okey, on waking from the trance into delirium, frequently now cannot open her eyelids; and her distress is considerable in the vain efforts which she makes, she being under the impression that her eyes are gone. If you hold a finger or your hand, pointed for a minute or two towards her eyelids, at the distance of one or two inches, she recovers the power of raising the eyelids.

III. Among the facts which are the most readily verifiable in mesmerism, and which at the same time constitute the most persuasive evidence of the absence of trick on the part of the persons mesmerized, and establish a parallel between the mesmeric influence and other physical causes in nature, are the phenomena of its transmission and reflexion.

The principal experiments which have been made, illustrating this part of the inquiry, have been the following:—

The patient, in mesmeric delirium, has pushed with both her hands against a partition wall, some one on the opposite side, and at the point exactly opposite her, at the same time pushing against the wall. No effect ensued; the patient continued in the state of delirium.

A similar experiment being repeated with the pannel of a door interposed instead of a wall, in a few seconds the patient has fallen from delirium into coma.

When a like experiment was repeated, a sheet of tin two lines in thickness being interposed, the patient equally became comatose.

When a common mirror was used, the patient pressing with both hands against the glass, the operator pushing against the wooden back of the mirror, the patient again became comatose.

The patient, being in a state of delirium, stood with her back against a shut door. The operator behind the door made a succession of movements, or passes, directed downwards, with his hands pointed towards the patient's head. In about half a minute she became comatose.

A sheet of tin being interposed, in-

stead of the door, and afterwards a mirror with the glass towards the patient, on repeating similar movements behind the tin, and afterwards behind the mirror, the movements being movements *of the hand alone, made near to, and ranging quite within the circumference of the interposed medium*, the patient, who was previously in the mesmeric trance, became comatose.

A silk handkerchief interposed between the patient and the operator appeared to have about as much effect as the tin or mirror.

The instances which have been mentioned sufficiently establish that the mesmeric influence may pass through some solid bodies, and cannot pass through all. It will be most interesting to establish in what proportion different substances, and different thicknesses of different media, interfere with the passage of this influence.

The instances exemplifying the reflexion of the mesmeric influence are even more remarkable than the preceding.

The patient being thrown into the mesmeric trance, stood with her back towards the operator, who was at the distance of five or six feet from her. He held in his hand a small looking-glass, about eight inches by ten, vertically behind her head, so as to reflect the image of her head directly back. He then made a few downward passes towards the .class, magnetizing the image of the back of her head: in a few seconds she became comatose.

The experiment was varied by reversing the looking-glass, presenting the wooden back towards her; on making the same passes, no effect ensued. But whenever, the movements being continued, the mirror was turned towards her, she being in the delirium, fell into coma. It should be mentioned, that she stood with her back towards the only window in the room.

A plate of zinc, and afterwards one of copper, being substituted for the mirror, no effect ensued on repeating the experiment. But a shining plate of tin being used, the patient was thrown into coma.

It was found that the patients could throw themselves into the trance out of the delirium, by magnetizing their images in the glass; that is to say, when looking at a mirror two feet before them, by making at the reflection of their faces downward passes. One was then told to magnetize a pane of glass: this failed to produce any effect; so the efficient reflection, as of light, was due to the amalgam of metal, not to the glass of the mirror.

Finally, the following experiment was arranged. One mirror was held obliquely to the patient's right, a few feet from her; another was likewise held a few feet from her on the left; the inclination of each being such that the operator could see in the second mirror the reflection of the patient from the first, but not the patient. He then made passes towards the reflected image in the second mirror, and the patient, from the state of trance, dropped into coma.

If the mirrors were not so placed as to correspond as above supposed, or if a handkerchief was thrown over the first when they were properly placed, no effect took place.

So the mesmeric influence may be reflected by some of the same surfaces, and agreeably with the same laws, as light. What a world of analogical conjecture and experiment this suggests!

IV. *Of Mesmeric Prevision.*—One Tuesday afternoon, at five o'clock, when I was present at the North London Hospital, Elizabeth Okey, being in the mesmeric trance, said that in eighty-four hours she should be taken ill, and have pain in her side. The following Friday, at five o'clock, when I next visited the hospital, I asked one of the assistants, how the prophecies went on? He answered, "But indifferently; she now says she is to be taken ill to-morrow morning at four." I remarked that the new prediction was not so bad a hit, as it tallied within an hour of the former one. At four, the following morning, she was seized with shivering, followed by acute rheumatism, the inflammation attending which was located in her side.

On Friday, in last week, in the afternoon, Elizabeth Okey began telling Mr. Wood of something which was to begin to happen to her in forty-eight hours: he wrote it down, and sealed the letter. On Sunday, at half after four, I went to the North London Hospital, where were Dr. Elliotson and Mr. Wood, and others. At four, Okey had fallen into coma. Mr. Wood said he would wake her, and begged us to attend to what would follow. On waking, she was furiously violent, and

appeared to know no one present; she seemed to be unable to move her right leg, which had likewise lost sensation. Mr. Wood said he could put her to sleep as soon as we pleased. Accordingly, her hands being held, he pressed his hands, one against her forehead, and the other against the back of her head; in about twenty minutes she became more composed; in thirty-four minutes she fell into coma. She was placed in bed at six o'clock on Sunday afternoon. The seals of several present were then attached to the note containing her statement of what was to have happened.

On Monday, at half after three, she was awoke by Mr. Wood, but with great difficulty. The note was then opened. It described all that had happened very faithfully; but added that she could not be wakened till the expiration of twenty-four hours, whereas she was awakened at the expiration of twenty-one and a half.

These two instances of *prevision* will appear much less surprising to my readers than the former facts which I mentioned. They will, of course, be referred to intentional deception on the part of the girl. It will be said she would have little difficulty in foretelling that which she could, if a clever and practised deceiver, easily act. But not to mention that it is not easy to put on a furred tongue, and other symptoms of rheumatic fever, I venture to assert my entire conviction that there was no attempt at deception in the matter.

The facts appear to me explicable on either of two suppositions, of which it is difficult to say at first which is the most monstrous. One is, that in this highly excited and disturbed state of the nervous system, the imagination may have the power of producing the morbid states on which it happens to run, or which it represents and figures to the mind. The patient, on this supposition, first gives her imaginings as prophecies, and then *unconsciously* determines their fulfilment. What gives this solution some probability is, that the imagination is evidently on these occasions strongly at work; so that the patient, if put into the mesmeric trance, and consulted about her case, will lay down the law as to her own treatment; and did, in the first instance narrated. This feature is evidently a trick of the imagination.

The other hypothesis is, that in this highly excited state of the nerves, the patient actually foresees what is going to happen to her health. This hypothesis it must be admitted requires a greater effort than the former to get it down, notwithstanding a fine metaphysical thought suggested by one with whom I conversed upon the subject—that, *à priori*, prevision is as easy as memory.

For myself, I rest satisfied for the present with admitting the facts which I have witnessed. Their explanation will come by and by.

V. *Clairvoyance.*—Hitherto nothing positive *in mesmerism* of the character of clairvoyance has come to my knowledge. But I confess, *now*, that I have seen so much that I deemed impossible, and know so much to be true that I held to be absurd and visionary, I do not venture entirely to reject the strong testimony which exists in favour of the occasional manifestation of this prodigious phenomenon. Upon the whole, I rather feel inclined to expect that I shall meet with facts which will compel me to believe in something that may bear the name of clairvoyance, which I will admit and endeavour to explain when I see it; but not till then. It is a pity that more in my profession will not take the trouble of going, and seeing, and examining the phenomena already displayed. Their verdict would come with so much more force after having seen the evidence.

I am, sir,

Your obedient servant,

HERBERT MAYO.

19, George Street, Hanover Square,
June 8, 1838.

EXTIRPATION OF THE PAROTID GLAND.

(Communicated by Sir J. M'GRIGOR, Bart.)

ELIZABETH HILL, aged 34, about ten years ago, showed me a tumor seated in the right parotid region, of the size of half a small orange, which was firm and immoveable, and gave no pain. I did not see her again until the latter end of May in this year. The tumor was then of an enormous size, rising to the zygoma, elevating the lobe of the ear, and descending to the middle of the sterno-mastoid muscle. It was still very firm and lobulated on its surface, and its attachments to the parts beneath

so unyielding that there could be little doubt of its being a diseased growth of the parotid itself. It gave her now great uneasiness, and interfered much with the motions of the jaw. The deformity was frightful, and she was anxious for its removal. The serious nature of the operation was pointed out to her, and the possibility of her not surviving it plainly stated; but she was firm in her determination to have the disease removed. I therefore proceeded to the operation on the 29th of May, having the able assistance of Dr. John King and Mr. John Rolph, my coadjutors in the Toronto Hospital.

The propriety of securing the common carotid as the first step, suggested itself to us; but the delay and additional pain by so doing, together with the power that pressure of this vessel against the cervical vertebræ seemed to offer in controlling the circulation through it, led me to abandon that course.

She was placed on a table, the head inclined to the left side, and an incision commenced behind the ear, which was carried down to the lowest part of the tumor. The external jugular vein being divided, was tied at both ends. Another incision was then made in front of the ear, so as to liberate its lobe, and leave an angle of integument attached to it; then carrying the knife parallel to the first incision, a sufficient elliptical portion of integument remained insulated upon the surface of the tumor.

I now began the removal of the mass from below upwards, first by dissecting freely with the knife, and as I got deeper I separated it from its attachments with my fingers. One small vessel only required to be secured in this stage of the operation. But when rather more than half of the tumor had been separated, a band too resisting for any thing but the knife presented itself, and, being divided, a powerful stream of arterial blood issued from the trunk of the external carotid. The mouth of the vessel was visible, and instantly secured with a ligature. The process of separation was continued, mostly by the fingers, until all but the upper portion was removed. This portion evidently dipped deeply down between the styloid and mastoid processes, but was separated from its situation, quite unbroken, without a stroke of the knife, and presented a sort of mamillary projection of the gland. The styloid process was now quite bare, as well as the transverse process of the atlas. The tumor did not adhere to the latter. One other vessel, not of large size, required a ligature before the operation was concluded. The periosteum covering the angle of the jaw had been absorbed by the pressure of the tumor. The chasm presented after the removal of this large mass was extensive, and I had the satisfaction of pointing out to my friend, Dr. Short, surgeon of the 24th regiment (who entered the room just at the conclusion of the operation), the situation occupied by the mamillary projection of the tumor, and the bare styloid process and transverse process of the atlas. The tumor weighed twenty-one ounces and a half (avoirdupoise), and exhibited, when pressed, an oozing of a semi-purulent fluid from numerous points of its surface.

Sufficient integument was left to meet neatly, and was kept in contact by sutures. No hæmorrhage occurred after the operation. She was given an opiate and put to bed. Next day there was retention of urine, which was relieved by the catheter, and the catamenia appeared at an irregular period. On the third day some fever occurred, with some pain in the head. The bowels were opened by infusion of senna and sulphate of magnesia. Next day the bladder expelled the urine. The febrile symptoms abated on the fifth day, but there was profuse suppuration from the bottom of the wound; the integuments adhered below. On the 6th of June she was removed in a carriage from the hospital to an airy situation, where her friends reside; and by the 14th of July the wound was perfectly healed.

The features are a good deal deformed, owing to the paralysis of the muscles of the face, occasioned by the division of the trunk of the portio dura. The orbicularis palpebrarum having lost its power of closing the eye-lids, the globe of the eye-lids appears to suffer from a slight degree of inflammation. She is, however, in perfect health, and very thankful for the result of the operation.

After all that has been written regarding the possibility of removing the parotid gland, I must confess that I approached this operation with reluctance, and nothing but the urgent desire of the patient to be relieved from the horrid deformity, and the steady support

I had to rely upon from my professional friends, would have induced me to undertake it. The result, however, proves to my mind the practicability of taking away the whole gland. I consider, however, the mode of commencing the detachment of the tumor from below upwards, as possessing a great advantage over the method adopted by Mr. Carmichael, as detailed in the second volume of the Transactions of the King and Queen's College of Physicians, Ireland; in which case Mr. C. commenced the separation at the upper part. Had I pursued this plan there would have occurred much greater perplexity in securing the large vessel with such a considerable portion of the mass unseparated below. Besides, I had the power, by grasping the lower part, of exerting much force in raising the tumor, with the vessel imbedded in it, from its situation, before it was divided, which exposed the bleeding mouth so fully to view.

C. WIDMER,
Surgeon to the Forces.

CASE OF IMPETIGO SPARSA,

CURED BY THE SULPHUR FUMIGATING BATHS.

To the Editor of the Medical Gazette.

SIR,

IN requesting your publication of the accompanying case, I do not seek to excite the interest of your readers by any anomaly in the symptoms. My sole object is, an attempt to increase the notoriety of a remedy which has acted specifically in a class of diseases, I will venture to say, amongst the most intractable ever submitted to the treatment of medical practitioners.

A gentleman, 47 years of age, of a remarkably healthy constitution, and temperate in his habits, has had for the last seven or eight years a very troublesome cutaneous eruption on the legs, but much more severe on the right extremity. An inveterate itching, commencing on the inner surface of the right ankle, was soon succeeded by an intensely bright red diffusive inflammation. With these symptoms, crops of small vesicles broke out, pouring out copiously a perfectly limpid fluid. The pruritus and inflammation speedily extended over a larger surface of the leg, and the oozing from the vesicles became extremely annoying, by stiffening and agglutinating the dressings. The vesicles having evacuated their fluid, assumed the appearance of thin laminated scales of a brownish colour, and were easily removed by the finger. The disorder was doubtless much aggravated by the irresistible habit of scratching with the nails. In no period of the complaint was there any evidence of derangement of the bowels or constitutional disorder. During this stage the symptoms corresponded with the *eczema rubrum* of Willan, or the *dartre squameuse humide* of Alibert, Rayer, &c.; the only difference being that the eczema or dartre very frequently spreads over a large surface of the trunk of the body, whereas the disease in question evinced in no stage of its existence any disposition to climb higher than the knee. To go into minute detail as to the treatment would be superfluous. It must be candidly confessed that the complaint obstinately resisted all the remedies usually adopted. Cooling astringent lotions, warm fomentations, escharotics, mild and stimulating unguents—each had its fair trial, none of them did good, and some produced positive mischief. The only application affording any thing like tolerable comfort were pledgets of linen rag steeped in cold water. The eruption had now taken up a firm position for upwards of seven years, and the patient began to abandon himself to despair, when a striking change took place in the character of the eruptive symptoms. The eruption having hitherto been vesicular, gradually put on a pustular appearance, but no mitigation of the pruritus, nor indeed any perceptible diminution of oozing, although the fluid, instead of being limpid, became purulent.

A consultation was now held with Dr. Green, and he decided in naming the disease impetigo sparsa. The pustules were, if possible, the cause of more violent itching than the vesicles, and various superficial small ulcers appeared, probably occasioned by the uncontrollable habit of scratching. The eruption had now occupied the whole surface of the right leg. An ointment composed of Unguent. plumbi superacet. ℥ij., with an equal quantity of prepared chalk stirred in while the ointment was in a melted state, produced a cleansing

effect, by causing an absorption of the fluid*.

After much persuasion the patient now consented to make trial of the sulphur fumigating baths; and here the purport of this communication discloses itself, in the complete and permanent cure effected by this powerful remedy. It was after the third bath that a decided improvement in the aspect of the leg occurred. By a steady continuation of about three baths per week, a disease, of upwards of seven years' standing, has in the course of five weeks entirely yielded, and the appearance of the skin is now, perhaps, more healthy than it was before the discoloration began. When I add, that scarcely any internal medicines were employed, it is but justice to award the whole merit of the cure to the external remedy. It is true that the blue pill, and the Liquor arsenicalis, were had recourse to for a time, but a slight affection of the bowels occurring a short time after commencing those medicines, they were abandoned, and have never since been renewed.

The unequivocal success attendant upon the use of these baths, induces me to offer a few remarks upon a remedy not, I fear, by any means so generally known, at least not so deservedly appreciated, as its merits lay claim to. And it will afford me much satisfaction if the above case, supported as it is by a multitude of others successfully treated by this simple but powerful agent, should be the means of convincing those numerous readers of the MEDICAL GAZETTE who may have patients tormented with cutaneous disease under any form, that they have a remedy—I do not hesitate to say, a specific at hand. And allow me to support this assertion by referring to what is doing, and has for some years been doing, in France and Germany, in regard to these troublesome disorders, hitherto in England ranked among the *opprobria medicorum*. In France, when any important discovery or invention in medicine gains popularity, it immediately attracts the attention of the government, and every facility is afforded in an investigation as to its merits; so that every resource which science can command is put in action to establish the efficacy or inutility of the invention. The result of the thorough investigation as to the alleged curative powers of the sulphur fumigating baths has thus been most satisfactory. In order to form a just decision, the French government directed reports to be drawn up by the most eminent practitioners in the various sections in Paris. An experienced officer appointed by government presided over of each of these committees, and their deliberations were kept secret from each other. The several reports were then handed in to a central committee, who unanimously agreed to publish the following report:—

"We have given it our most deliberate attention, and urge that the sulphur fumigating baths should be used in hospitals and great establishments.

"The committee think it their duty not to dissimulate on the advantages of this method, which cannot but be applicable also to the service of the camp and the army.

"Done at a meeting held the 22d August, 1815. Signed—Leroux, Dubois, Dupuytren, Richerand, Halle, Pinel, Percy, Barons and Professors of the faculty of Physic, Paris."

The consequence of this high testimonial has been the establishment of the sulphur fumigating baths in every hospital, prison, and workhouse, in Paris—I believe I may add throughout France. So generally are they employed, that at one hospital alone, viz., that of St. Louis, the astonishing number of 180,000 baths were administered in the course of the year 1836. In Germany their value is duly appreciated, and the most eminent physician of Vienna, Dr. De Carro, is using his high influence in their recommendation. By comparing things in our own country, your readers will allow that they manage these affairs better in France. From the great success attendant on their application in a large number of well-authenticated cases in London, it might be justly expected that our most eminent practitioners would be convinced of their high utility, and it cannot be denied that such is the fact, for the baths have been erected in two or three of the London hospitals. However, it is much to be regretted that they have not been in a condition to effect one-twentieth part of the good that might be expected. For instance,

* If the lead ointment is melted, a larger proportion of finely-powdered chalk can be incorporated. It is a very useful application, and for the formula I am indebted to the kindness of Dr. Green.

at St. George's, where, by the instrumentality of Dr. Green, a bath had been fixed, it was at one time allowed to get out of repair on account of neglect in the management. Now a little consideration will shew that such a powerful agent should be under the immediate direction and control of an experienced medical officer, and not abandoned to the ignorant hands of a servant. Any one who is in the habit of experiencing the powerful effects of the sulphur-bath, must acknowledge that an opportunity of frequent consultation with an intelligent physician on the spot is quite indispensable. So convinced were the eminent physicians, Le Roux, Pinel, Dupuytren, and others, of the importance of the baths being duly regulated, that they refused to sanction the chief apothecary of St. Louis to the office of director, alleging that the superintendence of the fumigatory process should, only be conducted by a judicious and enlightened physician. So important does Dr. De Carro view this subject, that he remarks, "you might as well abandon the patients themselves to the employment of blood-letting, mercury, opium," &c.

It is to Dr. Green that we are indebted, not only for a very comprehensive Treatise on Diseases of the Skin, but for his successful exertions in procuring the establishment of the baths at St. George's, and other hospitals; and not only common justice to that gentleman, but a higher principle should actuate the management, in taking especial care that the suggestions and advice of Dr. Green, supported as they are by the united testimony of enlightened foreign practitioners, should be strictly followed out.

In conclusion, I have only to observe, that as facts speak for themselves, I make no apology for troubling you with the above case; and if the extensive circulation of the MEDICAL GAZETTE should be the means of attracting the attention of the medical officers of hospitals, poor-houses, prisons, and other large establishments, to the rational and simple, though powerful, treatment of cutaneous and other diseases, by means of sulphur fumigation, I need not regret having requested insertion to these remarks.—I am, sir,

Yours very faithfully,

HENRY RONALDS, M.D.

Kensington Gore, June 8, 1838.

ON THE STRUCTURE OF THE VACCINE VESICLE.

To the Editor of the Medical Gazette.

SIR,

THERE is a prejudice entertained by many against investigations relating to subjects of no very obvious practical importance. But some judge very rashly on such matters; for there may be topics which, to the routine practitioner, may appear trivial, but which, to the more scientific practitioner, appear deserving of notice, and worthy of being made subjects of investigation. For example, by some mere practical men the minute anatomy of the tissues is neglected and deemed little better than laborious trifling; while there cannot be a doubt that minute and accurate anatomy is the very basis of correct pathology.

These are subjects, undoubtedly, on which time and labour may be unprofitably spent; but we should not be over-hasty in deciding what subjects shall be investigated, and what proscribed. It has been well observed, that no fact stands isolated, or undeserving of attention; and I consider that every person in our profession who establishes any particular fact or facts relating either to the healthy or morbid anatomy of any of the tissues, to the symptoms of any particular disease, or to the operation or exhibition of any remedial agent, is deserving of approbation. As an encouragement to the recording of facts, moreover, it should be remembered that, although some facts, at the time they were made known, appeared unimportant, they have afterwards assumed greater importance.

I make these few remarks to bespeak attention to the following observations, which to some gentlemen may appear more curious than useful. If I wished for an excuse for making them public, it would be the following, viz., that they relate to a subject respecting which no facts can be unworthy of notice, or uninteresting. But I care not in what light my observations are viewed, provided they be correct.

Dr. Jenner, who conferred so great a blessing upon mankind by the promulgation of vaccine inoculation, paid little attention to the anatomy of the cow-pock vesicle. He wisely endeavoured to disseminate his beneficent discovery,

and assiduously laboured to distinguish the genuine from the spurious appearances succeeding vaccine inoculation, and to ascertain the circumstances which militated against the success of the operation, or which rendered the system unsusceptible of its influence.

Dr. Cappe, of York, was, I believe, the first who gave an anatomical description of the structure of the vaccine vesicle. He described it as consisting of a congeries of minute cells; and his account has been adopted by all succeeding writers: by Dr. Willan amongst the rest.

From my own observation of the phenomena and progress of the vaccine vesicle, I have been led to entertain views regarding its structure somewhat different from those hitherto made public. At one time I was inclined to believe that the vesicle in question originated in the exhalents of the skin, or, as Breschet has recently called them, the sudoriferous canals. From this supposed seat of the affection I thought that the punctuated appearance of the cicatrix, and the cellular structure of the vesicle, were tolerably well accounted for. But subsequent and more attentive observation has convinced me of the inaccuracy of this opinion, and has led me to believe that the primary seat of the vaccine vesicle is in the villi of the cutis—in the capillary blood-vessels which lie on the outer surface of the chorion. Soon after successful vaccination, a minute inflamed point arises, and by the fifth day has assumed a vesicular appearance. The vesicle soon after presents a distinct depression at its centre, and an elevated appearance of its circumference. When the fully formed vesicle is punctured at one point only, it is found that a very little lymph escapes, and that for its complete evacuation numerous punctures are requisite. Hence, the cellular structure of the vesicle has been justly inferred. It is generally believed that the cellular structure of the vaccine vesicle is derived from the outer surface of the corion. It appears to me that this view is inaccurate; for the structure in question is, I am convinced, an inorganic one, resulting from a partial coagulation, which the contents of the vesicle undergo after being secreted. The cellular appearance of the contents of the vaccine vesicle is analogous to the pseudo-cellular appearance which the contents of vesications produced by cantharides sometimes exhibit, when the cuticle is allowed to remain entire longer than usual. This somewhat remarkable appearance of vesications must be familiar to all practitioners, and will prove even to the most bigoted minds that my opinion as to the cellular appearance of the vaccine vesicle being the consequence of a peculiar change in the fluid which it contains, is at least neither an impossible nor an improbable one. M. Gendrin, who has most recently, and with great minuteness, described the anatomical peculiarities of vaccinia, adopts the generally received opinions. Like his predecessors, Drs. Cappe, Willan, and others, he admits the *organic* cellular structure of the vesicle. He describes the cells as being "disposées circulairement sur deux rangs concentriques. On distingue aisement à la loupe les cloisons radiées assez peu régulières, entre lesquelles sont formées les espèces d'alvéoles qui contiennent le vaccin *." He describes further, the radiating *septa* as inserted into a central infundibuliform process, which produces the central depression observed on the surface of the vesicle. I cannot but think that M. Gendrin's account of the minute appearances of vaccine vesicles is somewhat coloured by fancy; and I am inclined to reject, as altogether fanciful, his description of the infundibuliform process. I am quite convinced that the alveolar structure so minutely described by Gendrin, is an inorganic product; its arrangement varying in different cases. As to the central depression of the vaccine vesicle, it results mainly from the lymph in the centre of the vesicle, immediately under the cuticle. I do not, however, speak very decidedly on this point. But observers will remark how early, in many cases, desiccation at the centre of the vesicle begins to take place, and the regularity with which it proceeds from the centre to the circumference.

The cicatrix succeeding vaccination affords no presumptive proof, I think, in favour of the organic nature of the cellular appearance of the interior of the vaccine vesicle, as some writers would have us to believe. Thus Mr. Bell, the author of a very sensible short treatise on Cow-pox, states, that the indentations of the cicatrix correspond to the

* Hist. Anat. des Inflam. tome i. p. 429.

cells of the original vesicle. The cicatrix is caused by absorption of the capillary vessels on the surface of the corion from pressure of the scab during the desiccation of the vessel. This opinion is proved to demonstration by the beautiful and simple experiments of Dr. Sacco, of Milan. This ingenious physician prevented the formation of scabs, during the progress of vaccine vesicles, by excluding the air from the parts by a very simple apparatus. In those cases where he prevented the formation of a scab, no visible cicatrix was left behind.* Proof stronger than this of the agency of the scab in producing the vaccine cicatrix is not to be desired, nor do I think it could be obtained. As to the punctuated or pitted appearance of the cicatrix, I have only to state, that it appears to me to be merely the natural appearance of the corion exposed to view by being deprived of the villi with which its outer surface is invested in the normal state. Whatever disease or injury removes this vascular layer, and allows the outer surface of the corion to be exposed to view after cicatrization, will produce the same appearance as that met with after vaccination. Thus, in small-pox, burns, or scalds of the cutis, I have seen a cicatrice produced in every respect identical to those produced by vaccination.

I submit, sir, these few hasty observations to your readers, in the hope that they will be speedily subjected to an impartial scrutiny. I remain, sir,

Your obedient servant, K.

* Tratt. di Vaccin. p. 51.

ANALYSES AND NOTICES OF BOOKS

"L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'ALEMBERT.

Treatise on English Bronchocele; with a few remarks on the use of Iodine, and its Compounds. By JAMES INGLIS, M.D. London: 1838. 8vo. pp. 95.

DR. INGLIS informs us, that when he first became physician to the public Dispensary at Ripon, he was surprised to find so many cases of goitre in the records of the institution; but further inquiry and personal observation taught him that the disease is as endemic in some districts of Yorkshire, and other English counties, as in any of the Alpine valleys.

"To such an extent does goitre prevail in some parts of England, that it is an every-day occurrence to see the children running about with pieces of black velvet round their necks, to prevent (as is supposed) the formation of the disease, or charm it away when already formed."—*Preface.*

Dr. Inglis examines the supposed causes of bronchocele as given by authors, but is not satisfied with any of them. His own theory is, that the disease is produced by water which has passed over strata of magnesian limestone.

"Take for example that ridge of magnesian limestone, running from north to south, through the centre of Yorkshire, and margining the shires of Derby and Nottingham. All along that line we have goitre to a very great extent; whereas, on our diverging to either side, the disease is found to diminish. The towns situated on this ridge are Nottingham, Alfreton, Chesterfield, Rotherham, Ackworth, Pontefract, Abberford, Wetherby, Knaresbro', Boro'bridge, and Ripon. After this, the magnesian limestone dips, then re-appears in the county of Durham; it continues its course almost due north from Darlington to South Shields, where at Tynemouth it meets the sea. In many of these towns I know goitre prevails; in the others I should suppose it did, excepting where we approach within the influence of the sea, when the morbid action would be counteracted, for we found before, from Dr. Richardson's statement, that the disease itself was removed by a sea voyage."—(pp. 22—3.)

The disease is scarcely, if at all, known at Harrowgate, though common all around; an exemption which the author attributes to the presence of iodine and bromine in the springs. Dr. Inglis gives a considerable number of tables, showing the proportion of goitrous patients in various districts. Thus, of 5054 patients seen at Bishopton by Dr. Paley, 111 were goitrous, and of these six only were males.

Of 993 physicians' patients at the Ripon dispensary, nineteen were affected with goitre, of whom two were males. Of 62,228 patients at the Halifax Dispensary, 242 were goitrous; while at the Public Dispensary in

London, there were only nine goitrous patients in about sixty thousand cases.

In the treatment of bronchocele, Dr. Inglis relies chiefly upon iodine: but he is averse to the use of the tincture, as he fears the deposition of pure iodine on the mucous membrane of the stomach. He therefore prefers an aqueous solution of the hydriodate of potash, or of the hydriodate of iron, or of iodine rendered more soluble by the presence of a salt, such as the nitrate of ammonia, the hydrochlorate of soda, or the hydriodate of potash.

Of these Dr. Inglis prefers the ioduret of iron, on account of its tonic and emmenagogue virtues; but observes that the preparation as kept in the shops cannot be depended on, and therefore recommends "every physician or general practitioner, who may have to prescribe the ioduret, to have it made under his own immediate inspection."—(pp. 67-8.)

It strikes us that this same recommendation is almost a *settler* to the use of the ioduret in prescribing practice, for the patient will be obliged to get the medicine from a druggist recommended by the physician; a thing "most tolerable, and not to be endured."

In cautioning the practitioner against the too free administration of iodine, our author says that "the same dose that might be beneficial to one, might to another prove fatal. Magendie, for instance, swallowed a spoonful of the tincture, containing about a scruple of iodine, without any bad effects ensuing, whilst a much less dose has been known to cause death in a few hours."—(p. 72.)

Christison, too, asserts that Magendie once swallowed a scruple of iodine in the form of tincture, without suffering any inconvenience. (Christison on Poisons, p. 138.)

Whether Drs. Inglis and Christison have both mistaken Magendie's account, or whether Magendie's statement may have varied in different editions, we know not; but in the last edition he undoubtedly says that he swallowed a teaspoonful of the tincture, and as this is made with 48 grains of iodine to an ounce of alcohol, the quantity swallowed was not a scruple, but only 6 grains*.

When iodine has been given too freely, producing the state which our author calls *iodism*, the best remedies in his opinion are opiates, particularly the acetate and muriate of morphia, and the warm bath, with a mild, sparing, and nutritious diet. Purgatives are hurtful, and so are bitters and astringents in the first or choleric stage.

We think ourselves that iodine is one of the numerous remedies that lose half the credit they might have got, from the too eager desire of practitioners to *push* them, as it is termed, and that the physician who is content with giving half or three-quarters of a grain of iodine a day, dissolved in water with the aid of the hydriodate of potash, will obtain all the benefits of this powerful agent, with little fear of iodism.

Dr. Inglis thinks that the summer catarrhal asthma, commonly known by the name of *hay fever*, is likely to be greatly benefited by the use of iodine; for if a patient labouring under this malady is sent to the sea, "whilst luxuriating amongst the fuci, and inhaling their grateful odour, all his direful symptoms disappear. Here, then, is the first indication of the treatment of hay fever by iodine, chlorine, or bromine."—(p. 76.)

On the whole, this sketch does credit to the industry and energy with which Dr. Inglis has collected information from all quarters on the subject of bronchocele; but we hope that in his next edition he will give a more detailed account of the disease as it appears at Ripon, with the treatment adopted, and its success.

* "Voyant cette innocuité de la nouvelle substance, j'avalai moi-même une cuillerée à café de teinture, et il n'en résulta rien, sinon une saveur désagréable, qui se maintint plusieurs heures, mais qui se dissipa ensuite peu à peu."—*Formulaire*, 8e edit. p. 217.

He gives the strength of the tincture at p. 228.

MEDICAL GAZETTE.

Saturday, June 16, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE ADULTERATION OF DRUGS.

We often read, in medical books, of remedies failing in their effect, on account of some idiosyncrasy of the patient; such is the peculiarity of his humours, that scammony will not purge him, nor laudanum put him to sleep.

That such singularities of constitution have occurred, cannot be denied; but that they are much rarer than is commonly supposed, is equally certain. The constant sophistication of medicines, which is every day, we fear, more prevalent, will supply a solution for many of these apparent enigmas; and the reason that a course of hydriodate of potass has produced no effect, is to be found in the fact that the supposed hydriodate was simply a carbonate!

The College of Physicians of Edinburgh have lately investigated this subject with great zeal and energy, and their Committee have published a report replete with useful suggestions *. They observe, that the practice of adulterating drugs will probably be diminished by the College adding a list of simple characters for the articles of the Materia Medica to the New Pharmacopœia, by which their purity may be ascertained; that it is desirable that the education of retail druggists should be regulated and improved; that druggists' shops should be visited by well-educated members of their own trade; and that the wholesale prices of genuine drugs, and announcements of newly-discovered adulterations, ought to be officially advertised. They likewise recommend that the proceedings of the College be communicated to the College of Surgeons, that the two bodies may co-operate in carrying their resolutions into effect.

To this report there is a valuable appendix, by Dr. Christison, in which he first treats of the actual adulteration of drugs.

Thus the *Iodide of Potassium* (hydriodate of potash) is often adulterated with the carbonate of potash. This may happen to the amount of one, two, or even five per cent., from the process employed in its manufacture, but, in so slight a degree as this, it is of little importance in practice; but the carbonate, with its accompanying water, sometimes amounts to ninety parts in the hundred; an express sophistication, of course, and sufficient to fill our books with cases of idiosyncrasy! It is also mixed with muriate of soda or potash, but seldom to the amount of more than ten per cent. *Iodine* is said to be mixed with oxide of manganese, brick-dust, and other substances of a fixed kind; but these Dr. Christison has never met with. We may add, that these gross sophistications are easily detected by the application of heat; the iodine will be volatilized, while the brick-dust, &c. will remain. Fifteen or twenty per cent. of water, however, are often found in it, which must proceed either from extreme carelessness in its manufacture, or from fraud.

The *Spirit of Nitric Æther* was offered to a country chemist by a travelling member of a wholesale house in London, at three different prices, namely, 2s., 3s. 6d., and 4s. 6d., a pound. The rustic shopkeeper, following the old rule of *medio tutissimus ibis*, steered clear of the good and the bad, and put up with the tolerable, at 3s. 6d. per pound.

Hydrocyanic Acid varies extremely in strength from differences in the process by which it is prepared, and from the decomposition which takes place, unless it is most carefully preserved. Dr. Fyfe found that among samples obtained from very respectable shops some were four times as strong as others. Dr. Christison asks, what would be the result if, after the dose of the weakest acid had been increased to fifteen or twenty drops, the druggist should get a new supply of four times the strength.

We were once told of a case of this kind. An eminent physician having prescribed prussic acid, it was procured for the patient at a respectable, but not

* Edin. Med. and Surg. Journal, April 1838.

first-rate shop. He repeatedly recommended the family to send to a better druggist, but meantime felt himself obliged repeatedly to increase the dose of the acid. At last, after this constant multiplication and addition of drops, his recommendation was attended to, and the patient swallowed an enormous dose of this formidable remedy, prepared in the best manner. She escaped with her life, and that was all.

Strychnia is hardly ever met with pure, and commonly contains a large proportion of brucia, a kindred but weaker alkaloid. This mixture is rather advantageous than otherwise, as pure strychnia is too strong to be very manageable. But strychnia, as used in medicine, generally contains colouring matter, which is inert, and by its greater or less proportion causes a dangerous variation in the strength of the drug. Thus a paralytic patient, under Dr. Christison's care, took strychnia of a less than average purity, until at length one evening the dose was directed to be increased from two-thirds to three-fourths of a grain, in two pills. This being only the addition of one-twelfth of a grain would probably not have been of much consequence; but unfortunately the medicine supplied by the apothecary was changed for one of almost snowy whiteness, and therefore far purer. The patient accidentally took only one pill; yet in the night he was seized with opisthotonos, and all the other formidable symptoms of poisoning with strychnia, and continued in this state for several hours.

It is certainly rather strange that three-eighths of a grain, even of a very pure strychnia, should have produced such violent effects on one who had taken two-thirds of a grain of the coloured sample without any symptom that we hear of; but among the many uncertainties in the practice of physic, we must number the uncertainty whether doses, especially fractions of grains, are accurately weighed out. If, moreover, Dr. Christison is quite correct in his assertion, that, in order to do good in palsy, strychnia must be given in such doses as to exert a certain degree of its poisonous influence, it must be the most dangerous of all medicines. In what hydrostatic balance must we weigh those infinitesimals which are to produce the necessary twitchings and creeping sensations, without any risk of locked-jaw and spasmodic fixing of the chest?

Gamboge comes into the market in three states, known by the names of pipe, lump, and coarse gamboge. The first is perfectly pure; the second is adulterated to the amount of about 11 per cent.; but the coarse gamboge, faithful to its name, has sometimes 23 per cent. of fecula, lignin, and moisture, and sometimes more than one-half. No wonder that physician or layman should complain of the irregularity of the action of gamboge.

Scammony is hardly ever to be procured genuine; and Dr. Christison has often met with well-informed members of the drug trade who had never seen it pure, and did not know what it was when shewn them. When genuine, the proportion of the resin, which is its active principle, varies from 77 to 83 per cent.; while in the adulterated specimens examined by Dr. Christison, the resin was only from 37 to 42½ per cent. of the mass. Chalk and fecula are the principal ingredients employed to form the fictitious drug; and it seems that the latter adulteration is of long standing; for Dioscorides says that "the makers of scammony mixed with it the ὀροβινον ἀλευρον, the flour of a species of pulse, believed, on the authority of Sibthorpe, to be the *Ervum ervilia*, or bitter vetch."

Opium varies in its external characters,

as well as its composition, more than almost any other vegetable remedy; and the only certain method of determining its quality, says Dr. Christison, is by separating the morphia, especially in the form of muriate. It is not a little curious that opiums of nearly the same price, and supposed to be of at least a good average quality, varied in the quantity of morphia in the proportion of two to one; the best producing 12, and the worst 6 per cent. So fallible are the external characters!

Laudanum is made, according to the directions of the Edinburgh Pharmacopœia, with two ounces of opium to two pounds of proof spirit. When made by himself in this way from first-rate Smyrna opiums, Dr. Christison found that a fluid-ounce avoirdupois yielded, on evaporation, in one case 19·1 grains of solid matter, and in another 22·1; and on separating the morphia, the values of these two specimens of laudanum proved to be respectively as 120 to 130. With these facts as a standard, he examined laudanum from seventeen shops in Edinburgh, and three in a county town, and found that the differences in the strength of the tincture were even greater than in opium, and that the morphia was not always in proportion to the solid parts contained in the tincture.

Thus, a specimen procured from a first-rate shop in Edinburgh afforded only 17·1 grains of solid matter in an ounce, while the proportion of morphia was represented by 130, being the same as in Dr. Christison's most successful experiment; the laudanum from another shop of the same class afforded only 10 grains of solid matter in the ounce, and the morphia is represented by 40 only. In another Edinburgh shop, the proportion of morphia was 30, *i. e.* exactly a quarter of Dr. Christison's least successful trial. The country shops are, as might be expected, far better than the second-rate Edinburgh ones; they stand as follows:—

	Solid part of 1 fl.-oz.	Relative Morphia.
18. From a shop in a county town	16·8 ..	90
19. From another of less repute in ditto	17·5 ..	100
20. From a third, do. do.....	15·8 ..	90

The next point discussed by Dr. Christison is the sources of the adulteration of drugs. These are chiefly the foreign merchants and the wholesale druggists; and the apology made for both by Dr. Christison is, that their customers are determined to have drugs at a price for which genuine ones cannot be procured. Thus a wholesale druggist of eminence in London allowed that scammony is rarely to be had pure from any wholesale establishment.

"The reason he explained to me was, that many of his customers in the retail trade,—and his own case was likewise that of all his brethren,—would not give a fair price for a pure article; that they did not ask for a pure drug, but for one of a particular price, often greatly under what it could be purchased for from the importer; and that on communicating with his Levant correspondent, and informing him what he wanted, he received for reply, that no Levant merchant could furnish good scammony at the prices proposed, and that it was therefore mixed up by himself and others with various proportions of chalk, to suit the varying demands of English dealers. The foreign merchant, whose letter I saw, complained of the necessity to which he was thus reduced of disgracing his occupation; sent several specimens, five in number, of his several adulterations, specifying the proportion of chalk and of pure scammony in each; and requested his London correspondent to make the facts as generally known as he could to the medical profession. As

one step towards executing his instructions, the information and specimens were made over to me; and on analyzing the latter, I found them to be composed almost exactly as represented."

The third and last division of the subject discussed by Dr. Christison, is the best method of preventing the adulteration of drugs. Here he goes over, at greater length, the points touched upon in the Report of the Committee,* and insists with great force on the advantages to be derived from the institution of a College of Pharmacy; or, in other words, an association of educated druggists. We wish this enlightened project every possible success; at any rate, the investigation of the present subject must be of infinite utility. When evils are very glaring, to expose is almost to reform them.

PUERPERAL FEVER.

To the Editor of the Medical Gazette.

Sir,

Should the accompanying case of puerperal fever, which has lately been, and still is, so prevalent in this town, appear of any interest to you, its insertion in your valuable periodical will oblige

Your obedient servant,

Charles L. Arnaman.

St. Thomas's Hospital,
June 8, 1838.

Mrs. Holt, aged 27, of delicate constitution, living close to an open stagnant ditch in a badly-drained part of Camberwell, was delivered of her third child on the 27th of February, 1838. On the 5th day after delivery she was seized with shivering, headache, and pain in the abdomen. Four days after I saw her for the first time, namely, on

March 8th.—The tongue red and fissured; great thirst; total loss of appetite; respiration hurried; countenance anxious; skin hot and dry, and of a dusky yellow; pulse 150, small; extreme pain on pressing abdomen.

Twelve leeches to the most painful part of abdomen (towards left groin); to bleed into a linseed-meal poultice, which is to be renewed every three hours.

Pulv. Antimon. comp. ℈ss.; Hydrarg. Chloridi, gr. xvj.; Pulv. Opii, gr. iv.; Conf. Rosæ Caninæ, q. s. Divide in pil. vj. sumat j. 4tis horis.

9th.—Passed a restless night; bowels have not been relieved; all symptoms the same.

Pulv. Jalapæ, Hydrarg. Chlorid. aa. gr. x. statim.—Rep. pilulæ ut heri, cum coch. iij. magna Misturæ Seq. —Liq. Ammon. Acet. ℥iv.; Mist. Camph. ℥iij.; Tinct. Hyoseiami, ℥ss.; Tinct. Digitalis, ℥ij. M.

10th.—Bowels opened by a common enema; skin moist; pulse 120; mouth sore. Diarrhœa came on in the evening, which was stopped by two doses of

Mist. Cretæ comp. ℥iss.; Liq. Opii Sedat. ♏vj.—To leave off pills, and take the mixture every eight hours.

11th.—Passed a comfortable night; pulse 100, weak; no headache; respiration and countenance natural; but little tenderness of abdomen on pressure; milk and lochiæ, which had been suppressed, returned.

℞ Decocti Cinchonæ, ℥vij.; Tinct. Cinchonæ comp. ℥vj.; Tinct. Lavand. comp. ℥ij. M. sumat cochl. iij. magna ter die.

To have strong broths, beef-tea, and four ounces of port-wine daily, which a charitable person in the neighbourhood was kind enough to supply her with.

16th.—Has gone on with the above treatment, and improved in every respect; appetite returned; pulse 90, and stronger.

To take ℥j. of Vinum Ferri in water daily, and to leave off the bark.

The favourable turn in the symptoms seemed to date from the affection of the mouth by the calomel. General bleeding, from her weak pulse and constitution, was out of the question.

* He mentions the insertion of tests in the late edition of the London Pharmacopœia, but says "it would not be difficult to shew that this department of it is not exactly fitted for the practical purposes here contemplated"; we presume, on account of no tests being given for ascertaining the purity either of simple drugs, or their Galenical preparations.

AMAUROSIS.

M. Serre, of Montpellier, states that in certain cases of amaurosis, more especially those in which the branches of the fifth pair are implicated, he has derived great advantage from rubbing solid nitrate of silver on the ball of the eye.

LITHOTRITY,

IN A CASE WHERE THE STONE ADHERED TO THE BLADDER.

A MAN, named Cotte-Boutaillat, aged 57, of strong constitution and sanguine temperament, was admitted into the hospital of the Hôtel des Invalides on the 18th of June, 1837. In the course of the year 1836 (he did not recollect the period more precisely), he began to experience a pricking at the end of the penis, extending along the urethra, every time he made water.

Before this period he had always made water with ease, and had never experienced any retention of urine, either complete or incomplete; nor had he ever remarked any change in the qualities of the urine. Two months afterwards, he passed bloody urine several times, particularly after long and tiring walks. He was then obliged to make water oftener than formerly.

A year after the appearance of the first symptoms, Cotte began to have a sense of weight in the fundament; and he was now obliged to make water every quarter of an hour.

The desire of voiding the urine became more frequent and more urgent the oftener it was satisfied, because, in consequence of the entire expulsion of the urine, the stone became in immediate contact with the mucous membrane of the bladder, and thus caused the desire of voiding the urine when there was not a drop to be expelled. At the same time, the call to go to stool was felt more frequently than usual.

Cotte came under the care of M. Larrey for violent hematuria. He had all the symptoms of stone in the bladder, yet none could be found on sounding; so that M. Larrey confined himself to treating the hematuria with emollient and hygienic remedies.

At a later period the patient was still in the hospital, when the surgical cases were put under the care of M. Pasquier, jun., the principal surgeon.

Cotte was still in the same state, and the hematuria frequently occurred. M. Pasquier, like his predecessor, recognized all the rational symptoms of calculus; but his first attempts at sounding were not more fortunate than M. Larrey's, and no stone was found.

On the 8th of November, however, M. Pasquier succeeded in striking the calculus with a silver sound, and proposed to break it down the following day.

The following was the state of the patient:—Urine of good appearance; occasional hematuria, sometimes demanding the vigorous use of emollients; no suffering while in bed, but, if he walked, there were pains in the hypogastrium, in the fundament, and the extremity of the penis, followed by bloody urine; there was also a marked alteration in his general health. On the 9th of November the patient still felt fatigued by the examination of the previous day, and the breaking down of the calculus was put off till the following one.

On the 19th the *lithotriteur* was introduced without much difficulty. The operator made unsuccessful attempts for a quarter of an hour to seize the calculus, which, nevertheless, he touched every time he opened the branches of the instrument. M. Pasquier being prepossessed by the supposition that the calculus was at the fundus of the bladder, as it is in the immense majority of cases, had not sufficiently examined the other regions of the bladder, when he perceived that he had to do with an adherent and suspended calculus.

Its situation was accurately determined during this sitting, and the operator ascertained that it was suspended to the anterior and superior region of the bladder, above and behind the neck; it occupied the median line, and extended more to the right than the left. It was of more than a middle size, and its free surface, which was rough, was touched by the instrument; hence it was not encysted. It was very solid, and was supposed to consist of uric acid.

The operator being once satisfied that it was an adherent calculus (a rare anomaly), resolved to proceed no farther that day; partly, because the patient was exhausted, and partly, because the instrument was not sufficiently curved to seize the stone.

Nov. 11th.—The patient was suffering from fever, with pain in the hypogastrium; frequent desire of voiding the urine, which was reddish; and headache. The face was red and puffy. These symptoms were evidently caused by the prolonged examinations of the preceding day; suitable treatment was adopted, and the operation was put off.

12th.—The patient is better; the pulse is less frequent; the face less red and puffy; the headache is gone; there is less sensibility in the hypogastrium; the urine is limpid, but the desire of voiding is still frequent.

Nov. 13th to Dec. 29th.—The state of the patient continually varied from better to worse, so that delay was thought prudent.

Dec. 30th.—Cotte's health now permitted a fresh examination of the bladder, which confirmed the former diagnosis both as to the situation and size of the cal-

culus; it appeared to be large and very hard. M. Pasquier settled and described the manœuvre which would be requisite in order to catch the stone with the branches of the instrument.

Dec. 30th: *First sitting.*—The instrument was easily introduced, and the stone was immediately seized. M. Pasquier tried to detach it, even at the risk of bringing away a portion of the mucous membrane; and, in order to do so, he gave the instrument a twisting movement, which he repeated several times; for he justly thought that the trituration of the calculus would be much more easy when it had fallen into the fundus. This manœuvre succeeded completely; a considerable calculous mass fell into the fundus of the bladder, and M. Pasquier ascertained that it was composed of two calculi, which were joined, or at least contiguous.

He then proceeded to break down the two calculi, in which percussion was necessary; but pressure was sufficient to triturate the secondary fragments.

A small quantity of stone was brought away, partly by the lithotritizer, and partly by the *videur;* and these fragments being joined to what was passed with the urine (which was carefully filtered), made a mass about the size of a hazel-nut.

No bad symptoms came on, and the patient continued in a very satisfactory state until January 5, 1838, when the bladder was again examined with the jointed sound. No more detritus was found in the fundus of the bladder, but it was ascertained that a considerable portion of the calculus was still adhering. The bladder, wearied by these examinations, contracted spasmodically, and at the same moment the adherent calculus struck the concavity of the sound.

Jan. 6th: *Second sitting.*—The method of proceeding was the same as before, and several portions of the adherent calculus were broken off. The quantity of stone extracted was equal to the first. The patient was comfortable during the rest of the day, but in the course of the night he was inconvenienced by a fragment of the stone wedged in the orifice of the neck of the bladder.

7th.—M. Pasquier, when he came to see the patient, pushed the fragment back into the bladder by injections of tepid water.

8th: *Third sitting.* — The bladder was examined with the jointed sound, and no fragments were found in the fundus; the adherent portion alone remained, and extended chiefly to the right of the neck. It was necessary to have recourse to the *brise-coque* to divide it; several fragments were detached, and easily broken down. The *videur* was introduced, and assisted the exit of the fluid contained in the bladder; a great quantity of dust was suspended in the fluid. The mass of calculus obtained was equal to the preceding ones.

9th.—Tepid water was injected, and more detritus was obtained. The state of the patient continued to be satisfactory.

10th: *Fourth sitting.*—Two pieces were detached from the calculus, and easily broken down. Tepid water was injected, but the quantity of detritus which followed was smaller than at the preceding sittings.

11th: *Fifth and last sitting.*—The manœuvres to separate the portion of the stone still adhering to the bladder were repeated, and were supported by an assistant, who used moderate compression upon the hypogastric region. They completely succeeded, and the remaining portion of the calculus fell into the fundus of the bladder, where it was easily broken down. A tepid injection was followed by the exit of a very small quantity of detritus.

Up to the 22d of January inclusive, no bad symptoms, either general or local, had occurred; the state of the patient was very satisfactory; and the urine gradually became limpid.

On the 23d of January a bit of gravel was passed in making water. M. Pasquier, on visiting the patient, threw up an injection, and then examined the bladder with the jointed sound; but not an atom of detritus was detected. He then pointed out the necessity of watching the state of the bladder for some time.

24th to 30th.—The examinations were repeated several times, and the bladder was always found perfectly clear; Cotte, therefore, was now considered as entirely freed from the stone.

To finish the case, we will add, that the calculus was almost entirely composed of uric acid, and shewed, on analysis, only some slight traces of oxalate of lime. After every sitting the patient experienced a little feverishness, which rarely lasted till the following day; and at the same time there was sensibility in the hypogastrium, with prickings in the glans, and frequent desire of making water, which soon went off.

Baths were often employed, especially the bidet, as well as clysters, which were at various times simple, emollient, narcotic, and antispasmodic. He also had poultices upon the hypogastrium, with or without laudanum; and took infusions of chamomile, mallow, or violet, sweetened. The regimen was severe, and was often modified according to the indications.

But what contributed above all to the

success of the operation, were the attentions of every kind, hygienic and therapeutic, which are lavished with such exactness upon all the patients under M. Pasquier's care, and which, unquestionably, go incalculably far towards the cure of a patient, particularly when the case requires long and assiduous treatment, as in the subject of this article.—*Gazette des Hôpitaux*, Feb. 22, 1838.

NEW METHOD OF PREPARING COPAIVA FOR ADMINISTRATION.

Pharmaceutists have long sought for some means of administering copaiva, by which its nauscous taste and disagreeable odour should be overcome, without its remedial powers being destroyed. M. Mothe has attained this object by enclosing the balsam in ovate capsules of gelatin. M. Raquin, pharmaceutist at Chancery, has communicated to the Academy of Medicine of France, a method devised by himself, which appears to be an improvement on that of Mothe. Instead of employing the liquid copaiva like the latter, M. R. thickens it with magnesia. If a sufficient time be allowed, six months or more, he asserts that a thirty-second part of magnesia will render the copaiva sufficiently consistent. The mass thus made is formed into ovate pills, and then covered with a very thin covering of pure gluten. N. Cullerier has employed the preparation in more than a hundred cases, and is convinced of its efficacy. This preparation has the advantage over that of Mothe, in containing more copaiva in the same bulk, the capsules of Mothe never being full, and the envelope being thicker. The gelatinous capsule weighing eighteen grains containing only ten grains of copaiva. The glutinous capsule weighing eighteen and a half grains contains sixteen grains of copaiva, magnesia three-quarters of a grain, and the envelope a grain and three-quarters. Thin as is the glutinous envelope it is completely impermeable, and remains so for an indefinite period; the gelatinous capsules, on the contrary, allow the copaiva frequently to transude after some time.

The committee of the Academy to whom the memoir of M. Raquin was referred, have made a report highly favourable to the invention of this pharmaceutist. The committee have satisfied themselves that these capsules are perfectly dissolved in the stomach*.

* Bull. Gén. de Thérap., and La Presse Méd. 1837.—American Jour. Med. Sciences, Feb. 1838.

RETROVERSION OF THE UTERUS.

A very remarkable case of this, and one we believe perfectly unique, has been recently communicated to M. P. Dubois, by Dr. Mayor, of Lausanne. The uterus was completely retroverted, the recto-vaginal septum lacerated, and through this laceration the retroverted uterus, with its contents, a fœtus of three months and a half, passed entirely out of the vulva.—*Ibid.*

BOOKS RECEIVED FOR REVIEW.

Practical and Experimental Chemistry, adapted to Arts and Manufactures: by E. Mitscherlich, Professor of Chemistry in the University of Berlin. Translated from the first portion of his Compendium, by Stephen Love Hammick, M.D., F.R.C.P., &c.

Animal Magnetism and Homœopathy. By Edwin Lee, M.R.C.S., &c. Second Edition, considerably enlarged. With Notes, illustrative of the Influence of the Mind on the Body.

WEEKLY ACCOUNT of BURIALS.

From Bills of Mortality, *June* 12, 1838.

Age and Debility	26	Heart, diseased	4
Apoplexy	1	Hooping Cough	7
Asthma	1	Inflammation	14
Cancer	1	Bowels & Stomach	1
Childbirth	3	Brain	1
Consumption	47	Lungs and Pleura	7
Convulsions	11	Influenza	1
Dentition or Teething	3	Mortification	1
Dropsy	7	Paralysis	1
Dropsy in the Brain	5	Rheumatism	1
Erysipelas	2	Small-pox	30
Fever	15	Unknown Causes	61
Fever, Scarlet	2		
Fever, Typhus	2	Casualties	6

Decrease of Burials, as compared with the preceding week . . . 78

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

June.	Thermometer.		Barometer.	
Thursday . 7	from 38	to 63	30·09	to 30·10
Friday. . . 8	34	62	30·13	30·16
Saturday . 9	32	65	30·15	29·98
Sunday . . 10	44	63	29·78	29·60
Monday . . 11	49	61	29·50	29·52
Tuesday . 12	50	63	29·52	29·57
Wednesday 13	42	62	29·62	Stat.

Winds S.E. and S.W.

Generally clear, except the 10th and two following days, when rain; distant thunder in the S.E. on the afternoon of the 12th.

Rain fallen, 5 of an inch.

Charles Henry Adams.

Wilson & Son, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 23, 1838.

LECTURES

ON

FORENSIC MEDICINE;

Delivered at the Aldersgate School of Medicine,

By Southwood Smith, M.D.

Physician to the London Fever Hospital.

Lecture XIV.

Sudden Death from Diseases of the Abdominal Organs—Acute Inflammation—Chronic Inflammation terminating in Ulceration—Ulceration terminating in Perforation—Distension—Rupture — Application of Cold to the Stomach—Application of powerful Stimuli—Spontaneous Solution.

Sudden death from diseases of the abdominal organs.—The diseases of the organs of the abdomen which are capable of proving rapidly fatal, possess, with relation to Forensic Medicine, great interest. The various affections of this class which have their seat in the stomach, the duodenum, and throughout the whole extent of the alimentary canal, deserve your particular attention. The natural diseases of the stomach which are capable of proving fatal, either quite suddenly or with extreme rapidity, are acute inflammation, chronic inflammation terminating in ulceration, ulceration terminating in perforation, distension, rupture, sudden application of cold, sudden application of powerful stimuli, and spontaneous solution or gelatinization.

Acute inflammation. — It is doubted by physicians in the most extensive practice, and who are the most accurate observers, whether acute gastritis ever really exists as an idiopathic affection. "It is a disease," says Dr. Abercrombie, "described by all systematic writers, but in the records of pathology it is very difficult to find a pure example of it in an idiopathic form. I have been often very much astonished to find, in my own observation, how seldom the stomach shews marks of inflammation, even when the organs most nearly connected with it have been inflamed in the highest degree. In cases of very extensive peritonitis, the peritoneal coat of the stomach is sometimes affected; but even this is rare, and a case of pure inflammation of the peritoneal coat of the stomach I have never seen, and do not find described by any writer.

"The disease which we call gastritis is to be considered, therefore, as seated chiefly or entirely in the mucous membrane, and even here it is extremely rare as an acute and idiopathic disease. It is from the action of the acrid poisons that we chiefly find inflammation of the mucous coat of the stomach; but we cannot consider these cases as necessarily exhibiting the same symptoms which would accompany the disease in its idiopathic form.

"For my own part, I have never seen a case which I could consider as being of this nature; and I am disposed to regard as points not yet ascertained, what are the characters exhibited by the mucous membrane of the stomach in the earlier periods of acute gastritis, and in what they differ from appearances which may exist without any symptoms of gastric disease, or take place after death."

In this state of uncertainty you cannot decide that the occurrence of acute gastritis, as a natural disease, is an impossible event; but while experience teaches that it is so extremely rare, you can never meet its actual presence without being constrained to take it, in a chain of circumstantial evidence, as a strong affirmative fact, evidentiary of the administration of poison. You are called to a case in which there has been present during life

the symptoms of violent irritation in the stomach, terminating in very rapid death; on examining the body, you find a large portion of the mucous surface of the stomach of a bright crimson red colour; such, for example, as you see in the drawings before you. After the most careful examination you cannot detect a particle of poison of any kind. Can you, from the mere inflammatory redness, draw a positive conclusion that the inflammation has been produced by poison. Obviously you cannot arrive at such a conclusion unless it were previously decided that the existence of acute gastritis as a natural disease is wholly impossible; but as long as this remains uncertain, the only conclusion you are warranted in drawing from such an appearance of the stomach is, that it is a circumstance of a suspicious nature; it may satisfy you that poisoning is possible; nay, that it is even probable, and may set you upon the search for further evidence of the fact; but it cannot of itself afford decisive evidence of that fact.

Chronic inflammation. — But while acute gastritis is so rare a disease as to render its very existence questionable as an idiopathic affection, chronic inflammation of the stomach is a very common malady, and one which not unfrequently proves fatal under circumstances of considerable interest and importance in relation to forensic medicine. The mucous membrane of the stomach is very liable to inflammation in this chronic form, and the disease advances slowly and steadily to a fatal termination, oftentimes without giving any indication of its existence. The patient may be affected merely with the ordinary symptoms of dyspepsia, or at most with those symptoms in a more aggravated form than common; but still there may be no such suffering, no such disorder of the functions of the stomach, as to induce the slightest suspicion that an organic disease is advancing with a slow but constant progression to a fatal termination; and consequently the fatal event, when at last it takes place, appears to be quite sudden.

Terminating in ulceration.—This disease consists of chronic inflammation of the mucous membrane of the stomach. The inflammation commonly commences in a small and circumscribed portion of the membrane, which passes slowly into the state of ulceration. The progress of the disease and its ultimate termination vary considerably, according to the kind of ulcer that is formed.

Sometimes the ulcer is small, defined, with rounded and elevated edges, varying in extent from the size of a split pea to that of a shilling. There may be only one such ulcer, or there may be many; and in the latter case the ulcers appear to have come on in succession, and to have spread from one place to another. This form of the disease is often in its progress attended with a distinct train of symptoms, by which its existence is unequivocally denoted; and it ultimately proves fatal by gradual exhaustion after protracted suffering: hence the fatal event being distinctly foreseen, and its cause being ascertained with sufficient clearness during life, cases of this class, of course, afford but little difficulty, and can seldom give rise to suspicion.

Ulceration terminating in perforation.—But sometimes a portion of the parietes of the stomach is converted into a thickened and indurated mass; ulceration takes place in this indurated portion; the ulceration slowly advances, until at last it terminates in complete perforation. At other times, with or without induration, there is formed a large spreading irregular ulcer, which in its progress either erodes the blood-vessels, giving rise to violent hæmoptysis, under which the patient sinks in from a few hours to several days; or it slowly consumes the membranous and muscular tunics of the stomach, leaving nothing but the thin peritoneal coat, which at last suddenly giving way, the contents of the stomach pass into the general cavity of the peritoneum, and excite acute peritonitis, which proves fatal in a few hours. In these latter cases there is often no indication of serious and alarming disease, until the occurrence of the hæmorrhage or the perforation; and then the symptoms are so violent, and so closely resemble those produced by an irritant poison, that suspicion is sometimes unavoidable.

A gentleman, about 40 years of age, who had long been dyspeptic and liable to pain in his stomach, which, however, never assumed any fixed or alarming character, but which required a careful regulation of his diet, was seized suddenly in his counting-house, while applying himself actively to business, with a feeling of extreme faintness. This was followed by the vomiting of a large quantity of black fluid, resembling ink. The following day he continued sick and faint, and vomited occasionally the same kind of fluid, and he had discharges of similar matter from the bowels. The feeling of extreme faintness continuing, with the occasional vomiting of pure blood, he died exhausted on the fourth day from the attack. In the small arch of the stomach, about half way between the cardia and pylorus, there was found a round defined portion about the size of a half-crown piece, much indurated, and about half an inch in thickness. On the inner surface of this portion there was a small defined ulcer, about half an

inch in diameter and more than a quarter of an inch in depth, and the bottom of it was occupied by a firm fungous mass, of a dark-brown colour. No other disease could be detected in any other organ.

A woman, aged 45, had been for several years liable to attacks of pain in the stomach, which at first passed off in a short time, but they gradually became more severe and lasting, until, at length, they continued for several weeks at a time, and were little affected by any remedies. After continuing for several weeks, the attacks left her for weeks and months in very good health, and then again suddenly returned. Three weeks before her death, having been for several months free from any uneasiness, she was seized with one of her ordinary attacks, in which no unusual symptom occurred until, after eight or ten days, she was suddenly seized with copious vomiting of blood. This occurred repeatedly; and she died rather suddenly, about a week from the first occurrence of it, and about three weeks from the commencement of the attack. The stomach had contracted an adhesion of small extent to the left lobe of the liver, and another of greater extent, and very firm, to the pancreas. At both these places the coats of the stomach were diseased, but in the greatest degree at the adhesion to the pancreas. Here they were much thickened and indurated, for a space about three inches long and two inches broad; and the internal surface of this portion was entirely in a state of ulceration. There were also, on the ulcerated surface, several points which penetrated more deeply; and some of these contained coagulated blood, giving every reason to believe that they had been the source of the hæmorrhage. The other parts of the stomach were in a natural state, and all the other viscera were healthy.

A young woman, aged 18, had been affected for about six months with variable appetite and occasional pain in the stomach, which made her frequently sit with her body bent forward, and her hand pressed upon the epigastric region. Little notice was taken of the attacks, as she was going about, and otherwise in good health, when one evening she was heard to scream violently: she was found unable to express her feelings, except by violently pressing her hand against the pit of the stomach; the pain soon extended over the abdomen, which became very tense, and she died in extreme agony twenty-nine hours after the attack.

There was extensive inflammatory deposition on the surface of the intestines, producing adhesions to each other, and to the parietes of the abdomen. On the upper part of the small curvature of the stomach, near the cardia, there was a small perforation, of a size which admitted the point of the little finger. Internally this opening communicated with an ulcerated space on the mucous membrane, about the size of a shilling, with slightly thickened and hardened edges, and a considerable perpendicular loss of substance. The stomach, in all other respects, was entirely healthy.

A gentleman who had long laboured under severe dyspeptic attacks, occurring at irregular periods, after intervals of good health, was suddenly seized with excruciating pain in the pit of the stomach, accompanied by some vomiting, coldness of the body, and a small frequent pulse. From the moment of this attack, nothing that was done afforded the least relief. He continued in the most violent and unceasing pain throughout the night and through the following day; the whole abdomen became distended and tender, with sinking of the vital powers, and he died about thirty hours after the attack. On the posterior surface of the stomach, near the pyloric extremity, there was a space rather larger than a shilling, where the substance of the stomach was entirely destroyed; but the margin of the opening adhered all around very closely to the surface of the liver, which thus preserved the continuity of the part. Below this portion, and very near the pylorus, there was an ulcer on the mucous membrane, smaller than a sixpence, and through this, perforation of the coats had taken place of such a size as would have transmitted a full-sized quill. Through this opening the contents of the stomach had escaped into the cavity of the peritoneum, where there were exhibited the usual marks of extensive but recent peritonitis. Except the two spots now referred to, the stomach was perfectly healthy.

A stout healthy servant was one day suddenly seized with excruciating pain in the stomach, which nothing could alleviate; he expired in eighteen hours. There was found an extensive thickening and induration of the mucous coat of the stomach; and in the centre of this indurated part a perforation had been made penetrating all the coats.

A young lady, of a sallow complexion, and spare habit, accustomed to occasional pain in the stomach and in both hypochondria, which, however, usually gave way in a day or two to medical treatment, was seized one evening with what she called spasm in the stomach, which threatened immediate dissolution. The pains in the abdomen were most agonizing; they seemed to start from the epigastrium as a centre, and extended from thence to the back, and even to the shoulders; the

pulse was not hurried, and the tongue was clean: nothing alleviated in the slightest degree the agony she endured: she moaned incessantly, and died in excruciating pain in twelve hours from the first attack.

On examining the stomach, there was found a round aperture, situated on its anterior surface. The perforation was perfectly circular, and about the size of a pea; it was the result of an ulcer on the mucous coat, which had gradually penetrated the other tunics. This ulcer was hollow and circular, nearly the size of a shilling, and had the appearance as if it had been made with caustic, the orifice being made in the centre.

Sometimes the symptoms, during the attack, are still more intense. A man had suffered, during the space of seven years, sudden and exceedingly violent attacks of abdominal pain. Hitherto, however, he had always been relieved from this pain by a wine-glass full of brandy. One day he was seized with the usual pain; the attack continued without intermission all day; still he attended to his business, and in the evening went to market to buy fish for his supper. On his return the pain became intolerable; he took the usual dose of brandy, but it did not afford him the expected relief; he sat in a bent posture, with a sunken countenance expressive of much agony. He dreaded to go up stairs, but at length making a desperate effort, he ran up and fell as he entered the room. From this time the pain continued excruciating, and he died in agony thirty-six hours from the commencement of the attack.

At the junction of the duodenum and stomach a circular aperture was found large enough to admit a crow-quill. It was the centre of an ulcer that had destroyed all the coats of the stomach to the extent of half an inch.

Yet how suddenly this attack may supervene, and how complete may be the absence of every preceding symptom of disease up to the moment of the fatal seizure, are shown in the most striking manner by the following cases:—

A gentleman, aged thirty-five, in the enjoyment of excellent health, was seized whilst dining in company, with an excruciating pain in the abdomen, which he described as unlike any he had ever felt. The principal seat of the pain was the region of the navel, and was described as shooting from this part as a centre over the whole body. The pain never intermitted for a moment; it was so intense that he earnestly prayed to be relieved by death from his intolerable anguish: his intellect remained clear and perfect to the last, but his strength was rapidly exhausted, and he died in thirteen hours from the first attack of pain.

On examining the stomach, there was found about a finger's breadth below the pylorus, a circular aperture of the diameter of a writing pen. It proved to be the centre of an irregular superficial ulcer of the mucous coat, including in its extent two-thirds of the ring of the pylorus.

Another gentleman, healthy, but of dissipated habits, while singing a song in a jovial company, after dinner, was seized suddenly with symptoms precisely the same as in the preceding case: the progress of the symptoms, the event, and the appearances on dissection, were perfectly similar.

The symptoms of this terrible disease, then, during the fatal attack, are, sudden, acute, and unremitting pain, radiating from the epigastrium, or the navel, to the circumference of the trunk, and even to the limbs. The intensity of the pain is such, that, like the pain of parturition, it absorbs the whole mind of the patient, who, within an hour of the enjoyment of perfect health, expresses his serious and decided conviction, that if the pain be not speedily alleviated, he must die. Coeval with the attack of pain there is a remarkable rigidity and hardness of the belly, from the spastic contraction of the abdominal muscles. And what is truly singular is, that the pulse remains natural for some hours, until the symptoms are merged in those of acute peritonitis.

These cases, which I have selected from many others that have been placed on record by eminent physiologists, sufficiently show the ordinary progress of this formidable malady; and the preparations and drawings before you clearly illustrate its nature. How closely in their external character do these cases resemble those of poisoning with some powerful irritant. Under how great a variety of circumstances they may give rise to the suspicion of poisoning you will readily conceive, and the preceding account of the different forms of this affection as constituting a natural disease, will, I hope, enable you at once to appreciate the full value of the diagnosis between the appearances which you see exhibited in these preparations, and those which are the real results of poisoning.

Distension.—But without inflammation, induration, softening of the coats, or ulceration, it is possible for the mere distension of the stomach, from excessive eating, to cause almost instantaneous death. This may happen under highly suspicious circumstances, because the sudden illness and the rapid death may be unquestionably connected with the last meal. It is often impossible to avoid suspecting

that poison must have been introduced into the food. Take, as an example, the following case:—A corpulent gentleman died suddenly fifteen minutes after dinner. It happened that he lived on bad terms with his wife. A strong suspicion arose that he had been poisoned by her. His wife said that he fell asleep immediately after dinner; that he had not slept many seconds when he suddenly awoke in great anguish; that he called out eagerly for fresh air; exclaimed that he was dying; and actually expired before his physician, who was immediately sent for, could arrive. The body was examined judicially.

On opening the abdomen the stomach was found so enormously distended with ham, pickles, and cabbage soup, that at first nothing whatever could be seen but the stomach and the colon. Here, then, was an adequate cause of death; death produced by the direct impression made upon the stomach by excessive distension. In the present case, however, on examining the stomach more minutely, some white powder was found on its villous coat. This was at first suspected to be arsenic; but on analysis it turned out to be merely magnesia, which it appeared he was in the habit of taking frequently.

And this suggests a lesson. When you have found one cause adequate to account for death, do not rest satisfied with that; but continue the examination of the other parts of the body with as much minuteness as though you expected to find the cause of death there; in this way only can you render your evidence, what I have so often stated it should be your great object to make it, correct and complete.

Sir Everard Home relates a case which affords an additional illustration of the fact that great distension of the stomach may produce instantaneous death, by the direct impression made upon the organ. A child was left by its nurse beside an apple-pie; in a few minutes afterwards the pie was gone, and the child was dead. On examining the body there was found enormous distension of the stomach with the pie; no morbid appearances whatever could be detected in any other organ.

But sometimes this distension proves fatal, not so much by direct impression upon the stomach, as by apoplexy, chiefly congestive.

A man in good health was greedily devouring an excellent dinner; in the midst of his hard labour he became suddenly blue and bloated in the face; a clammy sweat broke out over his body, and he died almost immediately. On dissection, the stomach was found enormously distended with food, and the vessels of the brain were so gorged with blood, that the brain appeared absolutely too large to be contained within the skull. Either of these affections was alone sufficient to occasion death.

Rupture.—Rupture of the stomach, another disease capable of producing sudden death, though not a common event, yet occurs sufficiently often to deserve your attention. It is generally the consequence of over distension, combined with efforts to vomit. The abrupt turn which the gullet makes on entering an enormously distended stomach, produces the effect of a valve at the cardiac orifice, effectually preventing the escape of the contents of the stomach by vomiting. There they remain, irritating the stomach to repeated, violent, but vain efforts, to relieve itself of its load, and at length the coats of the organ give way. Of this, the following case affords a striking illustration:—

A woman, convalescent from a protracted attack of dyspepsia, determined to make herself amends for the privation she had endured by her long abstinence. Accordingly she one day ate enormously. Soon afterwards she was seized with a sense of weight in the stomach; she felt urgent nausea, and made violent but fruitless efforts to vomit. All on a sudden she uttered a piercing shriek; exclaimed that she felt her stomach tearing open, and from that moment she ceased to make any efforts to vomit, sunk rapidly into a state of insensibility, and expired in the course of the night.

In the fore part of the stomach there was found a laceration five inches long, and a great deal of half-digested food had escaped into the cavity of the abdomen. The coats of the body of the stomach were healthy, but the pylorus was indurated; this induration having been the cause of the dyspepsia under which she had so long suffered.

Sometimes the stomach is ruptured, not in consequence of distension from food, but by the accumulation of gas generated by depraved digestion. A violent attack of vomiting at the moment the stomach is greatly distended with gas, is a highly dangerous occurrence.

Another form of rupture which deserves your particular notice, consists in the laceration of only one of the coats of the stomach, and the tunic commonly ruptured is the mucous. This event proves fatal very rapidly, with symptoms which exactly resemble the effects produced by some of the irritant poisons. Dr. Christison has particularly directed attention to this partial rupture of the stomach, as an event very likely in careless hands to be overlooked; but the true nature of which, careful dissection is always capable of disclosing.

A youth aged about fourteen, on the evening after a Christmas feast, at which he ate and drank heartily, was attacked with violent and frequent vomiting. Next morning he said he felt as if the blood in his heart was boiling; he was unable to swallow; the pulse became irregular; and pressure in the region of the heart and stomach caused excruciating agony. These symptoms continued without mitigation till the following day; then at successive intervals he vomited two pounds of blood, and soon afterwards expired.

The inner coat of the stomach was torn in many places; that of the duodenum was lacerated almost completely round. No other disease existed either in the bowels or in any other organ.

In the last place, the stomach may be ruptured without over-distension, from violent muscular exertion. A coal-heaver of the city, in sound and vigorous health, was attempting to raise a heavy weight; suddenly he cried out, clapped his hands over his stomach, drew two deep sighs, and died upon the spot.

On dissection, a lacerated hole was found in the stomach, big enough to admit the thumb; the stomach contained no food, and there was no disease in any other organ.

A tumbling boy, in performing one of the unnatural contortions of the body incident to his occupation, was seized of a sudden with most acute pain in the belly; in a few hours he died in excruciating agony. The stomach was found ruptured in the middle. These cases are highly interesting and instructive.

Application of cold to the stomach.—The sudden application of intense cold to the stomach, especially when the body has been previously overheated, as from drinking a large draught of cold water, is capable of producing instantaneous death. It is not uncommon for persons who are guilty of this imprudence to drop down dead on the spot, and this may occur under circumstances which give rise to a violent suspicion of poisoning.

A man had been quarrelling with a companion; in the height of a fit of violent passion he swallowed a glass of beer; in a moment he dropped down senseless and motionless, and died immediately. His wife suspected the administration of poison; she demanded a judicial inquiry; the body was examined; nothing could be found in the stomach or in any other organ to account for death; the man had died from the sudden impression upon the stomach caused by the cold beer.

In hot seasons, when combined with exposure to a burning sun, a draught of cold water would seem to occasion death by exciting, along with irritation of the stomach, congestive apoplexy. This is often the case in warm climates, in which labourers exposed to the direct rays of the sun can scarcely be restrained from drinking frequently and excessively of cold water. They become attacked first with pain in the stomach, sickness, giddiness, and fainting; next with difficult breathing; and then with apoplexy. Such cases may occur in our own country in a hot season, or at any season when the body is overheated by violent exercise.

Application of powerful stimuli.—The sudden application of any powerful stimulus to the stomach, in a concentrated form, and in large quantity, is capable of producing death as instantaneously as a stroke of lightning. A man, a sailor, in a drunken frolic, or for some trifling wager, drank out of a quart vessel a large draught of ardent spirits. It is not certain how much of the spirits reached his stomach; but while in the act of drinking, he fell down dead on the deck. You frequently read in the newspapers accounts of people dying suddenly from intoxication. Sometimes this happens from congestive apoplexy, superinduced by the excessive stimulus; but occasionally it is produced by an impression made directly upon the stomach.

Spontaneous solution.—The last affection of the stomach to which it is necessary that I should direct your attention, consists of softening or gelatinization of its coats, without inflammation, without redness, and in general without any indication whatever of disease. The nature of this affection is exceedingly obscure. It deserves your special attention, because it is often confounded with the effects of poison.

It is commonly supposed to be produced by the immediate action of the gastric juice upon the coats of the stomach; and the reason why it is not of more frequent occurrence seems to be that during the progress of the ordinary and natural diseases which terminate in death, the powers of digestion are impaired before the fatal event takes place; so that in these cases the secretion of the gastric juice is imperfect, and its action proportionately feeble. Accordingly, it is when a person who had not before been in sound health, and had recently taken a hearty meal, dies quite suddenly, or when a person, who under similar circumstances, has been cut off suddenly by external violence, that the action of the gastric juice upon the coats of the stomach is most commonly seen; and it is then, also, that the solution of the stomach is the most extensive and complete. The experiments of Dr. Wilson Philip upon rabbits, who found in many instances the great arch of

the stomach completely dissolved, when the animal had been killed very soon after eating, fully confirm the correctness of this view. It is probable that in these cases the secretion goes on for some time after the extinction of the animal life, or after apparent death, and that a quantity of this solvent fluid, being in this manner accumulated in the stomach, it begins to act upon the coats of the organ, as soon as the organic life also comes to be extinguished, just as it would act on other dead animal matter. In a case recorded by Mr. Burns, the stomach, when first examined at the usual period after death, was found perfectly sound; but upon a second inspection, two days after, a considerable portion of it had been destroyed by this peculiar solution of its coats. In like manner, in the experiments of Dr. Wilson Philip, the effects of the gastric juice on the stomach were always the most remarkable when the body of the animal had been left for some time after death before it was examined.

But though it appears to be certain that this peculiar solution of the stomach is oftentimes the result of the action of the gastric juice after death, having been preceded by no disease, and not being in a strict sense the result of any morbid action, yet there is sufficient evidence to warrant the conclusion, that it does occasionally exist during life as a specific disease, and that it is the cause of death. But the symptoms of the malady, when it exists, are exceedingly obscure; in adults, it is rare that there are any indications of disease whatever, until the solution is complete, when the contents of the stomach escaping into the general cavity of the peritoneum, there come on rapidly the symptoms of acute peritonitis. From causes not well understood, this affection takes place most frequently in infants or young persons below the age of puberty; in whom it seems to be preceded by obscure symptoms indicating general febrile disturbance, usually accompanied by signs of local disorder of the stomach and bowels, as vomiting or diarrhœa.

The following case affords a striking illustration of the disease. A young lady, previously in sound health, was awakened early one morning with excruciating pain in the stomach, which nothing could alleviate. She expired seven hours after the first commencement of the pain. On dissection, there were found in the back part of the stomach two holes, which were surrounded with softening of the mucous coat.

With the distinctive characters of this softening or gelatinization of the stomach, it is indispensable that you should be familiar; because, without attention to them, the affection may be easily mistaken for the effect of poison. In an interesting case, to which I shall hereafter recal your attention, no less than six medical men in consultation actually mistook this affection for the effect of corrosive poison, and on the evidence given by these witnesses, three persons were imprisoned, and tried for their lives.

The preparations and drawings before you, exhibit in the clearest manner the appearance of the stomach when thus acted on by the gastric juice. You observe the stomach is rendered soft, thin, and transparent; all distinction between its different coats is obliterated; it looks just as if it had been steeped for some time in an acid.

Now this softening or gelatinization may take place in any part of the stomach; but it is oftenest found on the posterior surface. It is sometimes small, commonly as large as half-a-crown, frequently of the size of the palm of the hand, and occasionally of such extent as to involve an entire half of the stomach. Sometimes there is more than one aperture. The margin is of all shapes, but is commonly fringed; it almost always consists of the peritoneal coat only, the other coats being more extensively dissolved, because the gelatinization almost always commences from within, and extends outwards, although now and then the solution begins on the outer surface of the stomach.

The characteristic mark of the affection is pulpiness around the margin of the aperture, with a total absence of the appearance of increased vascularity. Moreover, the organs in immediate contact with the aperture, the liver, the spleen, the duodenum, are often in like manner softened and pierced with holes; but it is at once obvious that these changes are not the result of inflammation from the total absence of increased vascularity, and of all the other signs peculiar to inflammation and ulceration.

Such, then, are the chief natural diseases which have their seat in the stomach, that are capable of producing sudden death, and which you must distinguish from the effects of poisoning. The affections of the other portions of the alimentary canal are so similar that I need merely indicate them. The duodenum, like the stomach, is capable of being ruptured; it is a very rare event, but when it does happen, it closely resembles the effects of poisoning. Melæna and hæmatemesis, the purging and vomiting of pure or of altered blood, may produce rapid death; but these affections can scarcely be mistaken for the effects of poisoning, because the pain which accompanies them is seldom acute,

and the discharge of blood is commonly profuse.

Cholera, cholic, iliac passion, and obstructed intestine, may occasion very rapid death: a person may be in sound health, and dead within 24 hours after the attack of either malady. The symptoms may easily excite the suspicion of poisoning, and you are very likely to be called to examine judicially cases of this kind which have actually given rise to such a suspicion. It is therefore highly important that you should be able to draw the diagnosis with decision and correctness; but I think it will be better to state the circumstances which will enable you to do so, when I describe the symptoms during life, and the morbid appearances after death, of the poisons which most closely resemble these affections.

I have now, then, given you an account of the chief natural diseases which are capable of producing sudden death, and which are liable to excite the suspicion of poisoning, or of some other form of external violence. This detail, though it has occupied much time, was necessary, to enable you to enter with advantage on the study of the changes produced in the several organs, whether by external violence or by the internal administration of poison. The results of natural disease, and the effects of violence or of poisoning, are in general capable of being clearly distinguished, and often present the most striking contrast: for the due appreciation of the distinction you will find that the discussion of the topics which have so long engaged our consideration, will afford you the best preparation.

[The preceding lecture concludes the portion of Dr. S. Smith's course to be given in the present volume.]

THE CROONIAN LECTURES,

For 1838.

By John Clendinning, M.D., F.R.C.P.

EXPERIMENTS AND OBSERVATIONS RELATING TO THE PATHOLOGY AND PATHOLOGICAL RELATIONS OF THE HEART.

Cardiac disease usually complicated with other visceral disease; whence a necessity for fixing the normal dimensions of other viscera, &c.—Having proceeded a certain length in the collection of facts, with a view to obtain correct estimates for the heart, I found that, to render those estimates fully available in the investigation of cardiac disease, it would be necessary to ascertain the normal dimensions of almost every other viscus. The relations of the heart are universal. As the main-spring, so to speak, of the circulatory apparatus, its action governs more or less the actions of all the vessels connected with it, while, through the intervention of those thousand subordinate channels, it distributes nutriment to every part, and receives from every part effete materials and recrementitious fluids. It is obvious, then, that any important modification of its vital powers and functional actions must be speedily and deeply felt all over the numerous and complicated organs and systems of organs that depend on it for the materials of life; and that in its graver diseases, and such more especially as permanently or frequently and greatly increase or diminish its energies and movements, much derangement might not unreasonably be expected in the condition of some or all the other viscera. And that this expectation is well founded, will presently be put in evidence. Meanwhile it may be allowable to glance at one or two other reasons for extending the inquiry, by weighing and otherwise measuring, to other viscera, besides the heart.

One reason is, that such an inquiry would possess physiological interest in so far as it would lead to show how far organic development is modified by sex, stature, race, age, and other influences, and strictly compatible with health. Another reason is, the advantage in pathological inquiries of having a standard by which to distinguish between atrophy and hypertrophy, to both of which every organ is subject, and the intermediate condition of normal nutrition. With respect to the former, or physiological uses of such an inquiry, it may be said that it naturally and necessarily precedes the solution, even partial, of several questions of the higher physiology. It is obvious that over and above the mechanical and chemical elements and forces, there are in the living organism vital or hyperorganic powers; and before the share of causation properly attributable to those latter more subtle causes can be even approximatively determined, it seems plain that the extent to which that most complex condition or phenomenon, Animal Life, is effectuated by the material elements, as mechanical and chemical agents, must first be ascertained; and one of the first amongst the latter inquiries would necessarily be the determining of the law of organic nutrition and development.

Accordingly, the practice of measuring, of which some of the principal results with regard to the heart, lungs, and person, have been already stated, was extended to the other viscera; and for a considerable period of time, in fact for some years, I have availed myself of every opportunity of collecting measurements by weight and otherwise of all the viscera, of the cranium, abdomen, and pelvis—a

search in which I have been indebted to various friends for much valuable and obliging assistance; and of these trials I now proceed to examine the results.

The individual observations have been merged, as before, for convenience, in tables of totals and averages, and the normal average for each sex, and for four intervals of age for each of the following organs, are therein given—viz., the encephalon, the cerebellum, the liver, the stomach, the spleen, the pancreas, and the kidneys.

Weight of the encephalon and cerebellum, according to sex, age, and disease.—To begin with the encephalon. In order to ascertain the normal weight of that organ, or system of organs, I ascertained carefully the weight of between 220 and 230 encephala of persons dead of known diseases above 15 years of age, and equally divided between the sexes. In 188 cases the cerebellum was examined apart from the whole encephalon, and in 34—viz. 8 males, averaging 45¼, and 26 females, averaging 43¼, no separate measurement of the cerebella was made; 140 of the 222 cases were examples of various diseases, exclusive of phthisis and morbus cordis; and of those 140, half were males, and half females. The whole of the first 188 were then arranged tabularly in four columns as before, for the remainder of life above puberty or 15 years, distinguishing the sexes, and classing separately also the cases of phthisis and those of morbus cordis, and distinguishing likewise the encephalon and cerebellum, and the following are the results.

The two classes *varia*, in which, for reasons to be presently stated, are included no cases of phthisis or morbus cordis, gave the following averages, viz.:—

	Enceph.	Cerebel.	Enceph.	Cerebel.	Enceph.	Cerebel.	Enceph.	Cerebel.
Males...	49·33	5·5	43.5*	5	45·80	4·80	47	4·80
Females	42·17	4·75	42·60	4·25	42·75	4 5	39·90	4
Ages...	15 to 30		30 to 50		50 to 70		70 to 100	

* Including three idiots, averaging for the encephalon less considerably than 40 oz.

And the averages for each sex for life above puberty are, for the male encephalon 45½ oz, and for the female 42 oz.; and for the cerebellum the averages are, for the male 5·02 oz., and for the female 4·5. Now these estimates of the encephalon differ very materially from the only estimates I know that have been founded on extensive and accurate observation; viz. from those of Sir William Hamilton, of Professor Tiedemann, and of Dr. Sims. Sir William's average for the adult male is 48¼ oz. avoirdupois, and for the female 44 oz. Dr. Sims' average, deducible from his very valuable table of weights of the encephalon, in the 19th vol. T.R.M.C.S.L., is for the male, from puberty upwards to extreme age, 46¼ oz. avoirdupois, and for the female 43¼ oz. avoirdupois; while Professor Tiedemann's estimate is highest of all, being, according to my calculations, 53¼ oz. avoirdupois for the male, and for the female 44¾.

Table of Estimates of Encephala Averages for the whole of Life above Puberty.

	Male.	Female.	
	Gross.		
Tiedemann	53·25	44·75	No apparent distinction of disease.
	Gross.		
Sir W. Hamilton	48·25	44	No distinction of disease.
	Gross.		
Dr. Sims	46·25	43·25	No distinction of disease.
	Net.		
1. J. C.	45·50	42	Phthisis and Morb. Cord. *excluded.*
	Gross.		
2. J. C. 104 m. 84 fe. 188	46·17	42·11	Phthisis and Morb. Cord. *included.*
3. J. C. 24 m. 10 fe. 34	45·75	43·25	Phthisis alone.
4. J. C. 33 m. 15 fe. 48	47·25	43·60	Morbus Cordis.
	Net.		
5. J. C (cerebellum)	5·02	4·50	Phthisis and Morb. Cord. *excluded.*
	Gross.		
6. J. C. (cerebellum)	5·17	4·69	Phthisis and Morb. Cord. *included.*

The greatest difference is, of course, between the extremes of the scales of estimates, which are on the male side greatest, viz. between Tiedemann's and mine, and amount to nearly half a pound avoirdupois, or one-sixth of the whole. Sir William Hamilton's estimate falls short of Professor Tiedemann's by five ounces, or near one-third of a pound, and differs from mine by about one-fifteenth. Dr. Sims' is something more than seven ounces, or 44-100ths of a pound lower than Tiedemann's, and exceeds my estimate by three-quarters of an ounce. Then, on the female side, Tiedemann is nearly one-sixth of a pound higher than my estimate; Sir W. Hamilton two ounces higher; and Dr. Sims about one ounce and a quarter higher than my average estimate. Now those differences are considerable. Professor Tiedemann and Sir William Hamilton, in particular, have obtained averages very greatly in excess, or else my averages, and I may add those of Dr. Sims also, err very seriously in defect.

Estimates differ, and why?—In explanation of those differences the following facts and considerations present themselves. First, with regard to the number of observations from which each has drawn his averages, it appears that Tiedemann possessed but 52 direct observations, including all ages and both sexes, and exclusive of negroes. The amount of Sir William Hamilton's direct observations appears to have been from 60 to 70, including both sexes and all ages; while Dr. Sims has given nearly 220 observations of subjects above puberty, and I myself a number of cases, above 15 years, somewhat greater than that of Dr. Sims.

Now if the averages obtained from Dr. Sims' observations, without distinction of disease, and from my own, without excluding phthisis and morbus cordis, be summed up and halved, averages will thus be obtained drawn from about 450 observations, including both sexes, viz. for the male, 46·58 ounces; and for the female, 42·68 ounces,—averages not very considerably different from my own, and founded on a total of observations nearly eight times as numerous as either of the totals of Professor Tiedemann or Sir William Hamilton; so that there is much reason, on the ground of superior extent of observation, to conclude that my estimate approaches very much nearer the true one than either of Professor Tiedemann or Sir William Hamilton. Then with respect to the difference between the results of Dr. Sims' observations and mine, there is a circumstance not belonging to this place, but to be enlarged on hereafter, that seems fully to explain that comparatively small difference.

In deducing their averages, distinction has not been made on the part of Professor Tiedemann, Sir William Hamilton, or Dr. Sims, as it has been for my own estimates, of the cause of death; and to this difference in the mode of investigating the matter, it appears to me may, with much probability, be attributed great part of the difference between the results to which our observations respectively lead; and, indeed, all the difference between Dr. Sims' average and mine. With respect to this cause of the difference between the results so often referred to, it will be sufficient at present to state the following averages. My average for various diseases, exclusive of phthisis and morbus cordis, and distinguishing the cerebellum, as already given, is for the whole encephalon 45·5 for the male, and 42 for the female; and if to those averages we add respectively the averages obtained from 34 other cases of various diseases, exclusive of P. and M. C., and of either sex, and in which no separate measurement was taken for the cerebellum—(viz. 45 : 25 for the male, and 43 : 25 for the female)—if we add those to the former and divide, we shall have from the whole 174 encephala the following averages—viz. for the male 45 : 37, and for the female 42 : 62; averages differing little from the standard, being 13-100ths less for the male, and nearly 66-100ths more for the female. So far, therefore, the enlargement of the basis of calculation increases the probability that the first results were near the truth, since 174 observations give in each sex, within a fraction, the same averages as the original dividend of 140. But the supposition that the difference between the estimate deducible from Dr. Sims' series of observations and mine, is owing to want of distinction of diseases which I have not attempted in summing up his weights for averages, seem to me clear, from this especially, that I find, as I shall hereafter state at length, that increase of weight is, in the encephalon, an usual effect or concomitant of morbus cordis; that my averages for morbis cordis alone exceed those deduced from Dr. Sims' table for all diseases, and that my *gross* averages for all diseases are higher considerably than my *net* averages for various diseases, exclusive of phthisis and morbus cordis; the latter or *net* being, as already just stated, 45 : 37 for the male, and 42 : 62 for the female; whereas my averages, without distinction of disease, are, for the male 46 : 17, and for the female 42 : 95 (or nearly 43); and in each sex, therefore, very considerably higher than for the select class *varia*. But as I shall have occasion to return to this subject, I shall at present content myself by assuming the average

for the brain, or encephalon, to be 45·37; or 45⅓ for the male, and 42·62, or 42⅔ for the female.

Cerebellum.—With respect to the cerebellum, of which, as not noticed separately from the brain or encephalon by Professor Tiedemann, or Dr. Sims, the average absolute weight for the males, excluding morbus cordis and phthisis, is, from 15 years upwards, 5 oz. and 2-100ths; and for the females, with like distinctions, 4 oz. and 5-10ths. This ratio between the sexes, of : : 10 : 9, is considerably different from that apparently found by Sir William Hamilton, whose statement is, that the cerebella of the sexes are nearly equal in absolute weight, but that the female cerebellum is rather the heavier. But he makes no mention of the absolute weight of the cerebellum, nor of the number of observations precisely, from which his conclusions are drawn.

Now since the number of observations on which my conclusions are founded is at least three times as large as those at his disposal, on that account I think it not unreasonable to infer that he was misled by paucity of facts, and that he has either overrated the female, or underrated the male, so as to have obtained a ratio very different from the true one *.

Density of the brain, &c.—In another statement, however, of Sir W. Hamilton, I fully concur; I mean with regard to the density of the encephalon. Like him I have found little difference between the different parts of the encephalon; with this exception, however, that the density of the cerebellum usually exceeded considerably, as he also has remarked that of the cerebrum.

In more than fifty observations at all ages, of both sexes, and of various diseases, I have met with but two or three instances in which the cerebrum exceeded or equalled in density the cerebellum. The average cerebral density for the males, I found by the first method (above described) to be 1056, to water as 1000; and for the females 1045; whereas in the cerebellum I found the proportions reversed, the male cerebellum being less dense than the female, the former being 1060·5, and the latter 1064·5. By the second method I obtained the following:—

For the male brain ..... 1031·33,
For the male cerebellum .... 1037·25,
For the female brain ..... 1035·87,
For the female cerebellum.... 1038·35,

so that by this method, also, the female would seem to exceed in the density of the cerebellum; and in both sexes the cerebellum would appear to be denser than the cerebrum. Like Sir W. Hamilton, I have found as yet, by either method, no striking influence of age upon the density of the encephalon; one of the densest encephala I have met with having been that of a female eight years old, found dead in bed; and one of the least dense, that of a male of fifteen, dead of phthisis, a disease in which I usually find the encephalon amply developed. On the whole, it seems clear that the tendency to variation in nutrition and development in the encephalon is not eccentric only, as in the heart it would seem to be, but is also often concentric and condensing in its effects: a difference between the organs, owing probably to this—that the brain is not subject to the distensive forces that affect the heart's parietes, viz., the expansive efforts of the diastole, and the reaction of the fluids in the systole. The liability of the encephalon to abnormal condensation is well shewn by the following recent observation:—A woman of 70 died of apoplexy, after an illness of some weeks. One hemisphere of the brain was observed to be normal, the other to be smooth and dry, with flattened convolutions. Under the floor of the ventricle, on the same side with the flattened convolutions, was a large clot that had burst into the ventricle, and pressed up the hemisphere apparently against the calvarium. The normal hemisphere, in this case, was found to be 1034·5, specific gravity; whereas the compressed hemisphere was 1338·5, shewing an unequivocal cerebral condensation on the compressed side.

LUNGS AND ABDOMINAL VISCERA IN THE NORMAL STATE.

We next proceed to the lungs, and I am quite sensible of the difficulty of dealing with these organs in the same way as with the other. I have already alluded to the fact familiar to anatomists, viz. the unparalleled variability in density, volume, and absolute weight of those viscera. If it be attempted to bring them in some degree out of the field of mere empirical opinion, in which, in great part, they at present lie, into that of science, by the application to them of tests and processes, open to every understanding, whether *technically* instructed or not, we find ourselves immediately involved in opposite difficulties. If we choose density for our standard, we are at once embarrassed by similarity of results, with extreme diversity of organic conditions. The pthisical lung, the pleuropneumonic, the œdematous, and the extremely congested lung, will frequently exhibit like specific gravities, like densities, even in spite of considerable pains in preparation. If we take volume for our test, we are little better off than with density, for the œdematous, the

* I regret that while writing those observations on the cerebellum, I had not an opportunity of perusing Mr. Combe's recent work.

emphysematous, and the extensively tuberculated lung, and the lungs that have been extensively tumefied and consolidated by pneumonia, will often possess the same volume. Linear measurement is obviously open to all the objections almost, already noted, or alluded to, and to some peculiar to itself; and weighing is, with respect to the lungs, exposed to sources of fallacy probably as numerous as any other mode of measurement. There are therefore great difficulties in the way of any attempt to bring within the pale of physical investigation, all or any part of the pathology of the lungs, above probably all the other organs. Yet the necessity of some precise knowledge of those physical characters of the normal lung, that admit of instrumental measurement, is clear; witness the writings of Plouequet and others, on the *docimasia pulmonum hydrostatica*. This has been long felt in forensic practice, and in practical pathology it appears to me to be as desirable as any other branch of scientific medicine. With these feelings the lungs have been for some time included in the measurements by weight, already a considerable number of observations (130 to 140) have been collected, falling short, however, very much of a total capable of yielding trust-worthy results. Those observations arranged according to age, sex, and disease, as the tables of hearts and encephala, give for the lungs the following results:—The class *varia* gives, as the average for life above 15, for the male, 46½ ounces, and for the female, 35½ ounces; the number of observations amounting to 31 for the males, and 34 for the females. These estimates are much less than those of Meckel, which are for both sexes, apparently about 55 ounces.

Abdominal viscera.—With respect to the abdominal viscera, I shall now state shortly the number of observations, and the averages I have obtained for the whole of life above 15; and for each of them, in from 150 to 160 subjects, two-thirds of whom nearly were females, and none of whom had phthisis or morbus cordis, nor any obvious morbid appearance of the viscera, implying unusual weight or density.

Table of Weights of the Abdominal Viscera for Life above Fifteen Years of Age, and excluding Phthisis and Morbus Cordis.

	Weights Avoirdupois.		Number of Observations.	
	Males.	Females.	Males.	Females.
Liver	49· nearly.	44·5	55	101
Spleen	4·75	4·33	50	93
Stomach	4·5	4·40	34	51
Kidneys	8·33	8·	55	98
Pancreas	2·25	2·25	51	84
Lungs	46·5	35·5	31	34

From the table it appears that this average weight of the normal liver may for the adult male be assumed to be about 3lbs. avoirdupois, or a little more, and for the adult female about a quarter of a pound less, or 2¾ lbs.

The spleen appears to weigh, on the average, between 4 and 5 ounces, being in the table 4¾ for the adult male, and for the female 4½ ounces avoirdupois. The stomach would seem to weigh the same in either sex nearly—viz., about 4½ ounces. The kidneys average, for the adult male, something under 8½ oz., or in the table 8⅓ oz.; and for the female, the average appears pretty accurately to equal 8 oz.; and the pancreas seems of the same weight in either sex—viz., about 2¼ oz. Now the totals from which these averages have been deduced, are, for the liver, between 150 and 160, observations of which nearly two-thirds were females. The spleen was examined in between 140 and 150 instances, and nearly two-thirds were females. The stomach was weighed in from 80 to 90 cases, in which nearly three-fifths were females. The kidneys in from 150 to 160 cases, of which something more than one-third was male, and the pancreas in from 130 to 140 instances, of which a considerable majority was female. These totals are too considerable, it is imagined, not to yield averages approaching the truth pretty nearly. From these totals every case of unequivocal and obvious abdominal visceral hypertrophy, and morbid excess of whatever kind, were carefully excluded, as were all cases also whatsoever in which the lungs were phthisical, or the heart diseased.

By such sifting, the totals have been in several instances reduced to half the amount, and less, they would otherwise

have reached; but the ends in view, viz. true averages, have, it is conceived, been more advanced by the selection of healthy viscera, than impeded by the diminution of the totals to be divided.

These numbers differ considerably from the averages of Meckel, which are, for the liver for both sexes and adult life, about 3½ lbs. avoirdupois: for the spleen of adults of both sexes about half a pound: for the kidnies for both sexes about a quarter of a pound each, or about half a pound for both kidnies; and for the pancreas, something between 4½ and 6½ oz.

These estimates of the great anatomist, and accurate and excellent writer, Meckel, are, however, deduced from totals of which the amount is not stated, and in which there is further no statement of any adequate precautions having been taken to distinguish modifications of development produced by age, sex, disease, &c. I feel myself, therefore, at liberty to question the accuracy of the estimates of Meckel, where they differ much from my own, and to say that, considering the number of observations, and the care taken in classification and selection on my part, there is reason to prefer my estimates to his, as approximations coming much nearer the truth. Respecting one organ only are we agreed, viz. the kidnies, regarding which our respective estimates are nearly identical. For all the other viscera his averages are much too high. With respect to density, also, the results I have obtained seem to differ considerably from those announced by Professor Meckel. Density and specific gravity would seem practically to mean nearly the same. Density, to which I have principally directed my attention, signifying the quantity of matter in a given bulk of any substance under observation; and specific gravity, the gravitating force exerted by a known bulk of such substance in comparison with some other. The specific gravity and the density must be mutually proportionate, therefore, to each other. Meckel, however, announces the specific gravity of the liver to be 1500, and that of the spleen to be 1200, if I rightly guess, for there is some typical ambiguity in the text; and I have repeatedly found the liver to differ in weight from its own bulk of water by about 1-20th, and never, that I know of, in numerous trials, more than 1-15th or 1-16th; which would give a density considerably under 1100, compared to water as 1000, and a specific weight still lower; —and I have as much reason to doubt the accuracy of his estimates of the specific gravity of the spleen as of the liver.

Having thus stated what I have judged necessary for my object, with regard to the physical characters of the viscera in their normal condition, it becomes my duty to enter upon the second branch of my subject, or the abnormal conditions of the organs, in so far as that is susceptible of instrumental investigation and measurement, and in so far as such instrumental examination as has been instituted is calculated to throw any light on the causes and effects, signs and treatment, of cardiac disorders.

ON THE FREQUENCY OF THE PULSE AT DIFFERENT AGES.

To the Editor of the Medical Gazette.

Sir,

I should feel greatly obliged by your giving the inclosed note a place in the Gazette at such time as may suit your convenience.—I am sir,

Your obedient servant,

R. Knox.

Edinburgh, May 30, 1838.

In the number of the *Gazette Médicale* for November 1837, there is a note by M. Lisle, *Elève des Hospitaux*, respecting the frequency of the pulse in infants. Having paid great attention to the physiology of the healthy pulse for a number of years, I may be permitted to offer a few remarks respecting the opinions advanced in M. Lisle's note, which I hope you will have the courtesy to insert in an early number of the *Gazette*.

The preamble of M. Lisle's note affirms that all authors, since Galen's time, have contented themselves with vague assertions until the year 1832, when the happy application of the statistical method to the facts of medicine and physiology was first employed. But if M. Lisle will do me the favour to examine a memoir on the Human Pulse, published by me in 1814, he will there find that the phenomena of the pulse had been examined by me, with all the rigour of the statistical method, somewhat more than twenty-four years ago.

But, in fact, this method was also employed by Dr. Bryan Robinson more than a hundred years ago. As the public, however, is in general but little interested as to dates of observation, however important they sometimes really are to the individual observer, I shall proceed at once to M. Lisle's opinions in respect to the pulse.

1st. He is of opinion that Messrs. Levret and Mativie have proved, *par des chiffres*, that the pulse is more frequent in the aged than in the adult. Every thing I have observed is against this opinion; but as my own experiments have not been numerous, the subject is open for inquiry, and I trust will be taken up in an unprejudiced way by some careful observer.

My own opinion is, that the observations of Messrs. Levret and Mativic were too few, and that there must exist some source of fallacy. There is nothing stated to shew that MM. Levret and Mativie were aware of the differential pulse, or of the effects of posture on the pulse; thus it is impossible to say how far those experimented on were placed precisely under similar circumstances. There is a question, then, respecting the human pulse, which has not been settled.

2d. M. Lisle considers as infants (*enfans*) young persons of the ages from 10 to 20 inclusive. The average pulse at these ages I consider as having been already decided; but in this country we should not think of calling such persons *infants*, unless they were wards of Chancery. The mean of the table he gives is, however, clear enough, and accords perfectly with the observations of previous observers; the table of Bryan Robinson, for example, which proves that the younger the person, the quicker will be the pulse. Numerous observations recorded by myself in my papers on the pulse, prove this also beyond all doubt.

3d. M. Lisle concludes from his observations, that "the pulse is more frequent in the evening than in the morning," and that "temperature exercises a manifest influence on the pulse." The latter of these propositions no reasonable man ever doubted; but the former I am bound to believe, from the whole series of my observations, to be erroneous. It is, in fact, the old prejudice of Cullen repeated under another form. If M. Lisle, or any other careful observer, will make himself fully master, as a preliminary step, of the circumstances which influence the frequency of the pulse—such as food, temperature, but above all muscular motion, of which a change in the position of the body constituting the "differential pulse" is but a form—he will soon satisfy himself, as I did many years ago, that the human pulse is, *cæteris paribus*, not only actually quicker in the morning than in the evening, but that it is also more *excitable*—that is, more quickly and more powerfully roused by stimulants of all descriptions; that, in fact, there is a diurnal revolution in the *frequency* and in the *excitability* of the pulse, commencing in the early part of the day, gradually attaining its maximum about midday, and then declining until some time after midnight.

Let M. Lisle, or any person who doubts this, try the effects of a midnight revel, and then contrast this with one carried on at midday, and he will then be quite convinced that his present opinions are founded in error. But he has only to refer to Mr. Smith's experiments, made at my request, and recorded in my Anatomical and Physiological Memoirs, just quoted, to be satisfied that he must have overlooked some important modifying circumstances producing fallacies in his observations. Since writing the above, my attention has been directed to a very valuable and clearly-written paper, by Dr. Guy, in Guy's Hospital Reports, No. VI., for April 1838*. Dr. Guy's object, in conducting his observations, seems to have been to "put the fact of a differential pulse, and its cause, beyond all cavil." With every respect for Dr. Guy, and thanks for his candour and courtesy in noticing my hurriedly-written papers so very handsomely as he has done, I humbly think that these questions were completely set at rest, and their exact *physiological value* appreciated, by Dr. B. Robinson and by myself†. Undoubtedly the first object held in view, in my original inquiry into the physiological laws regulating the human pulse, was to ascertain, 1st, the effects of muscular action upon its frequency. This led to the rediscovery, on my part, of the effects produced by a change of posture; and all the experiments made then and since have completely satisfied me that the cause of the differential pulse is simply "muscular action." Until I observed the brief notice in the Dublin Meeting of the British Association for the Advancement (?) of Science, I had fancied that

* See also MEDICAL GAZETTE of October 28th, 1837, page 190.

† Dr. Macdonnell's observations I never saw, and Dr. Graves's view are strictly a copy of my own.

all unprejudieed persons were satisfied as to the views adopted by me, on this point, twenty years ago. I was therefore a good deal surprised on finding Dr. Guy of opinion that "neither the amount of the effect produced (by change of posture), the circumstances by which it may be modified, nor the cause to whieh it is attributable, has yet been examined with sufficient care." Now I had imagined that all possible care had been taken by me to make out the physiological laws respecting these points, 1st, by observing *my own pulse* and that of three individuals in 1812; and that not as Dr. Guy has done, at *one hour of the day in all,* bnt at *every hour* of the day and night; and 2nd, in respect to the midday pulse, noting it in 25 healthy young men. The discrepancy which Dr. Guy supposes to exist between my early observations and those made at a later period, is in point of fact no discrepaney, and in no shape affects the principle. The average in the differential pulse drawn from *four* persons; namely, in 1812, was *in them* about 10; the average drawn in 1837 from the pulses of 25 young men, that is, from a wider range of observation, gives 3·8, but the faets and the eonelusions from these facts remain simply as they were. Neither can I discern where the discrepaney lies; it is the *fact* that is wanted, as it applies to the individual; the *average*, whether large or small, merely enables us to speak of it as a law applied to the speeies. 2d. As to the conditions whieh modify the phenomenon, I feel at a loss to imagine any whieh may have been overlooked by me. Temperature, diet, age, previous exeitement, time of day, general habit of body, all these were noted in the most eareful manner: so minutely was this gone into, that it was immediately ascertained, that while supporting the back of the person whilst sitting, the pulse was affected by several beats, and therefore all were placed alike in this and in every possible respect. The whole of Dr. Guy's observations have reference to only a single period, that is between 12 and 2; mine range throughout the whole day, by which means I discovered the diurnal revolution of the pulse, both as regards its excitability and its absolute frequeney. His averages are, standing 78; sitting, 70; horizontal, 66; mine in 1812 were, standing, 74; sitting, 64; horizontal, 60. Dr. Guy seems surprised at the difference between Mr. Smith's pulse and my own, yet he allows by his own table, that in some, the difference amounted to 26, and in others to 3. That I took some pains to verify and extend these observations, the candid reader will, I hope, admit, on observing the hours at which many of the observations were made (midnight 2 A.M., 3 A.M., 4 A M., &e.); he will observe, perhaps, also, that they were extensively varied; that the eurious fact was observed of some persons having no differential pulse, &e.; but I with great pleasure admit that Dr. Guy has confirmed all these observations—confirmed them so fully, that he will be pleased to observe again at page 99 of his Essay, that we are strictly in accordance. "The proportion which the differenee between standing and sitting bears to that between sitting and lying, is, in Knox's experiments, more than 2 to 1: my own give the proportion of about 3 to 1 when the exceptions are ineluded, and of about 2 to 1 exclusive of exceptions. Bryan Robinson made it 5 to 1, and Nuck 4 to 1 *."

If Dr. Guy will have the goodness to examine table 13, table 14, and table 16, wherein a series of observations were noted during a walk of 17 miles, he will there find that the law he lays down respecting "*the effect* produced upon *the pulse* by ehange of posture, *increases* with the frequency of the pulse," had not been overlooked. Lastly, that the cause of the effeet produced upon the pulse by ehange of posture was never for a moment doubted by me from the time I eommenced these inquiries. "The effeets produced on the pulse by ehange of posture is merely a case included in the *more general law* of the aceeleration of the pulse by museular exertion," and, *cæteris paribus*, is in the direct ratio of the amount of muscular force employed.

I have the honour to be,
Your obedient servant,
R. Knox.

Edinburgh, 30th May, 1838.

P.S. The laws regulating the effect of muscular eontraction on the pulse are but one, and that the least important of all the others, investigated with more or less success in the two Memoirs pub-

* Page 99, Guy's Hospital Reports, No. VI.

lished in part 1st of my Anatomical and Physiological Memoirs, with Appendix, 1837. The diurnal revolution of the pulse and the diurnal change in its excitability were discovered; the effects of posture, of food and drink, of gentle and of violent exercise, &c., were all noted with the greatest care by experiments upon two persons, whose bodily health was good, and their time and habits under their control. Dr. Guy, whose observations have reference only to one fact and to a particular time of day, will find it extremely difficult, if not impossible, to carry on such minute inquiries upon any great number of individuals: every one must be aware, in fact, that it is impossible to do so, and that my inquiries, extending as they do to the condition of the pulse at all hours of the day and night, are of a totally different nature from those made by Dr. Guy on the pulses of one hundred persons examined between the convenient hours of 12 and 2 P.M., and this, too, without the smallest reference to their diet, habits, &c.

ON THE BEST MEANS OF APPLYING PRESSURE TO THE UTERUS AFTER DELIVERY.

To the Editor of the Medical Gazette.

Sir,

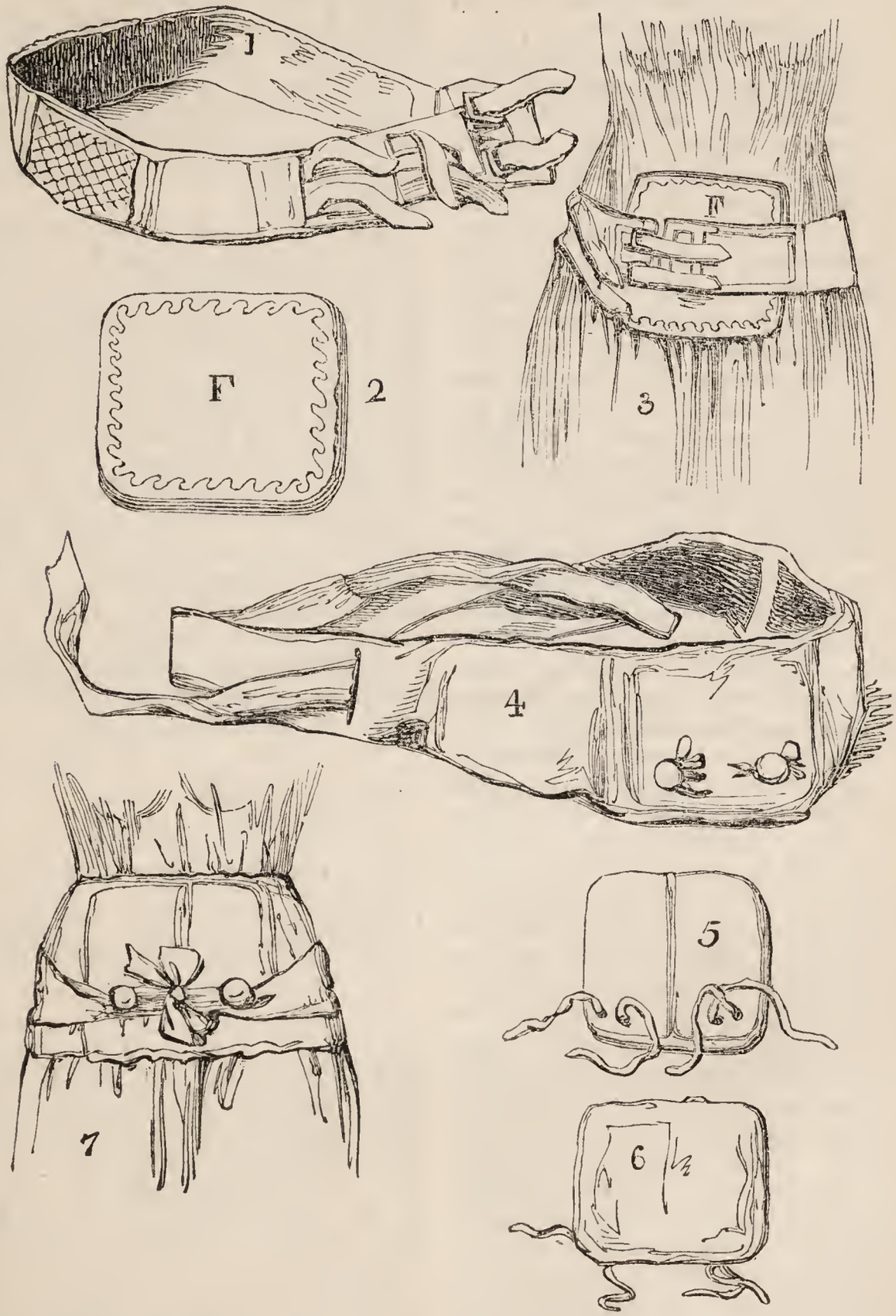
The important principle of making such pressure on the uterus as will ensure its contraction immediately after the birth of the child, in every case of labour, is at length so fully established as to be received throughout the profession as one of the axioms of scientific midwifery. Every practitioner carries out this principle by some one or other of various methods which suggests itself to his mind,—from the simple pinning of the long napkin, to the formidable tourniquet and rolled pillow. With the ulterior desire of eliciting further mechanical improvement, I proceed to give an explicit description of two bandages which I have long used in my practice, and some medical friends, whose judgments I highly value, have tried them with the greatest satisfaction.

In forming these bandages my first object was to make pressure on the region of the uterus by a *firm unyielding substance;* because, by this means, the contraction of that organ was found not only to be more readily produced than by a similar degree of force applied by means of a bandage composed of linen or of any soft substance, but, having been so produced, was more readily maintained contracted. To this principle we must refer the signal benefit derived from pressure by the firmness of the hands in cases of sluggish uterus; but hands and arms soon tire at this employment, and consequently the degree of pressure necessary to the complete contraction of this organ, instead of being steadily continued, becomes relaxed, and hæmorrhage occurs, or if it have been momentarily suspended, is renewed.

Figure 2 in the subjoined engraving represents a piece of mill-board, obtained from the stationer, seven inches by eight, padded on the inside with two layers of wadding, and covered with flannel or kerseymere. This plate has been previously divided down the middle, as seen in fig. 3; then united by pasting a strip of leather on each side, so as to form a joint; thus enabling it to be folded into half its compass, like a closed book; and, with the band, fig. 1, wrapped round it, to be conveniently put into the pocket. The band, fig. 1, which is made of variable length to suit the different dimensions of different females, is composed of webbing, three inches wide, is furnished with two buckles, and three sets of straps to regulate its pressure, and has four inches of India rubber web let into it, so as to combine a degree of elasticity with the force of its pressure. Fig. 3 shews the bandage duly applied; the band being under the crests of the ilia, and carried round the hollow of the back, just at the junction of the sacrum with the spinal column, by which it is prevented slipping upwards. This bandage, from its easy application, I use immediately after the birth of the child, directing the nurse, if there be hæmorrhage, to increase the pressure by buckling it tighter. This simple bandage answers well for every purpose proposed, is capable of exerting a great degree of pressure, and of thus facilitating or accelerating the complete contraction of the uterus.

When the patient is comfortably in bed, I usually apply what I call my sash bandage: were it applied previ-

ously it would probably become soiled. This is represented, fig. 7, applied under the crests of the ilia, and carried to the hollow of the back, just above the sacrum. Fig. 5 represents the exterior, and fig. 6 the interior of exactly the same plate, with the joint as



plied, folded in half; it is about 30 inches long, and is made of white jean doubled; it incloses the plate, fig. 2; it tapers from the width of the plate towards each end, where twelve inches of strong broad tape are attached for tying under the buttons, as seen in fig. 7.

described in fig. 2; but on each side, within two inches of the bottom, are two holes through which a piece of tape is seen passed from the inside, to attach a pearl button on the outside of the size of half-a-crown, as seen in fig. 4. Fig. 4 shows this bandage before it is ap-

The bandage is sloped downwards to fit the hollow above the sacrum, and in its posterior portion a slit is made, through which its opposite end is passed. By placing the plate over the region of the uterus, carrying the two ends of the bandage to the hollow

above the sacrum, and then bringing them round under the crests of the ilia, drawing them tightly over the plate, and tying the tapes in a firm manner under the buttons, a very effectual resistance is offered to the tendency which otherwise every bandage would have to slip upwards, and recede from the part which should receive pressure.

The specific advantage of the above plan, besides affording an extraordinary degree of comfort to the patient by the support it affords to the relaxed abdominal parietes, thus preserving the natural figure, is found by experience to be the prevention of uterine hæmorrhage and its dreadful consequences. Under the pressure which this bandage is capable of producing, even the formation of a coagulum of any size is almost impossible, and thus the accoucheur is enabled to leave his patient in a state of perfect security, which never can be the case if the uterus, though contracted at the time, be left without the support of some such pressure, which is therefore essential in every case in a greater or less degree. The natural expulsion of the placenta will be much accelerated by systematic pressure. Dr. Ruysch first, and afterwards Drs. Denman and Wm. Hunter, vainly imagined that the musculas orbicularis Ruyschii was self-sufficient for the expulsion of the placenta, and also for the complete contraction of the uterus; but even this muscular power is very greatly increased by the aid of pressure. I could give the detail of several cases of of retention of the placenta, which I have recently seen in consultation, where the uterus sympathizing with the general system, after a protracted labour, was in an atonic state. In these cases, as there was no hæmorrhage, I advised the continued permanent pressure by means of my bandage. The inherent muscular power of the uterus, thus assisted, safely expelled the placenta, in every one of these cases, without the introduction of the hand, and without any hæmorrhage. In one or two of these cases the placenta was retained three days and nights without any untoward sympton, and then securely and satisfactorily expelled by the natural efforts. A manifest mitigation of after-pains is acknowledged to be produced by means of these bandages by many females who have previously borne children, and have had no such assistance.

By securing the permanent contraction of the uterus, many cases of puerperal fever may be prevented, for it may be confidently asserted that puerperal fever is frequently produced by a congested state of the uterus. Nine-tenths of the diseases of the uterus, especially the chronic and acute engorgements, may be traced to the condition of that organ after parturition, and may be prevented by means of a proper pressure, such as the bandage described is calculated to make.

We have much reason to wish that those who have witnessed the accurate precision with which the various diseases of the uterus are discriminated and treated at the different institutions at Paris, where I am given to understand the most rapid progress has lately been made in this department of our art, would throw some light on this obscure subject. We might then be led to abandon the absurd system of fighting with the symptoms of the disease of the uterus, as though they were in themselves diseases, for the adoption of a rational system founded on accurate knowledge of the various alterations of structure which produce those symptoms.—Your obedient servant,

J. L. Fenner.

15, King's Row, Pentonville.
June 11, 1838.

ON INFLAMMATION,

AND THE MOTOR POWERS WHICH CAUSE AND REGULATE THE CIRCULATION.

By Professor Graves*.

The general laws which govern inflammatory action, and the relation which the vascular system bears to that process, constitute a most important subject, which has engaged the attention of the ablest pathologists and practitioners in this country for the last half century. Since the date of the great John Hunter's celebrated work, which gave the first impulse to this investigation, many British and Continental writers have applied their talents to the illustration of the changes the vascular

* This and the following papers are taken from Clinical Lectures delivered by Dr. Graves, in the session 1837-8.

system undergoes, during the progress of inflammation. Thompson, Hastings, W. Philip, James, Burns, and Marshall Hall, have performed numerous and interesting experiments, which throw light on its phenomena; and we have gained much by the assiduity and research they have displayed, in endeavouring to illustrate a matter of such acknowledged difficulty. Still, these authors appear to have adopted some erroneous views, and to have misunderstood or overlooked some points of peculiar importance. I shall first direct your attention to the opinions of Dr. Marshall Hall, as explained in his lectures, now in course of publication in the *Lancet*. Dr. Hall, possessing extensive acquirements, and high professional reputation, has cultivated the sciences of physiology and pathology with distinguished zeal, and made numerous experiments and microscopical observations, tending to illustrate the subject of inflammation; his opinions are, therefore, entitled to serious consideration.

In the last volume of the *Lancet**, page 76, Dr. Hall, speaking of the inflammatory process, observes — "I conclude that each cause of inflammation first induces such a physical effect upon the internal surface of the capillaries, as leads to the adherence of the globules of blood to it, and to their ultimate stagnation. This stagnation augments as the inflammation increases, and becomes more diffused, and seems to constitute the essential character of the disease." Here you perceive that the first step is the adherence of the globules of the blood to the internal surface of the capillaries, the consequence of which is, that the calibre of these vessels is considerably diminished, so that they become obstructed, and cause a stagnation of the blood, which Dr. Hall looks upon as the essential character of inflammation.

Further on he says—"I have never been able to detect any action in the capillaries themselves. It is, probably, by the partial obstruction to the circulation in the capillaries, that the minute arteries become enlarged." Now observe, according to this mode of explanation, the circulation being obstructed in the capillaries, in consequence of the adherence of the globules of blood to their sides, the arteries which supply them are propelling blood into obstructed vessels, and consequently become enlarged or dilated—and why? Dr. Hall says, "according to the well-known law, that muscular organs augment, with obstacles to their functions." Here I may in the first place observe, that Dr. Hall is not warranted in looking upon the minute arteries as muscular organs; but, waiving this point, how can the law alluded to explain the supposed increase in the capacity of the minute arteries? It might, indeed, explain the increase of thickness in their parietes, but is it not plain that this very addition to the thickness of the arterial walls, so far from increasing, must diminish their calibre?

Again, he observes—"It is probably by the fact of stagnation that inflammation differs from blushing, eruptions, &c." Here, you perceive, he introduces the qualifying term "probably." He continues—"It is generally asserted that there is a series of vessels which only circulate the serum of the blood, and exclude the globules. This I believe to be mere hypothesis. Vessels which only admit of single globules will appear colourless. In inflammation, the minute arteries which only admit single globules at a time, enlarge, and admit a greater number, and then the red colour becomes visible." He goes on then to say—"This enlargement of the blood-vessels is not confined to the minute arteries, for the larger vessels in the immediate vicinity of the inflamed part also become enlarged. * * * * * This is owing to the obstruction of the true capillaries." And he illustrates this, by instancing the application of a ligature to an arterial trunk, the consequence of which is, that the collateral arteries of the part become increased in size in consequence of the obstruction. We shall see, afterwards, how little this admits of being proved. He says—"It is not known how far this enlarged state of the arteries extends from the seat of the inflammation; but, in the case of an inflamed finger, the pulse at the wrist of the corresponding arm beats more strongly than it does on the opposite one."

Such are Dr. Marshall Hall's views of the causes of inflammation, and the part which the capillaries and minute

* This and the following lecture were delivered in the last week of November 1837. It will be seen that I have since added many references to books published in 1838.

arterial vessels play in that interesting process. You perceive, by the brief outline which I have given, that he attributes all the phenomena to adherence of the blood-globules to the sides of the capillaries, the consequent obstruction of these vessels, and the enlargement of the minute arteries to which that obstruction gives rise. In this view of the case, the vessels are regarded as passive, and are distended on purely mechanical principles; in fact, their enlargement is a mere dilatation.

Notwithstanding the respect I entertain for the learning, ability, and industry of Dr. Marshall Hall, I must say that I look upon his views as purely hypothetical, and am convinced that he has arrived at unsound conclusions with respect to the nature of inflammation. I shall not, however, take up your time by going over his positions *seriatim*, and shewing their untenable character; but shall proceed at once to lay before you the opinions to which observation and reflection have led me, and which have been taught for many years in my lectures on the Institutes of Medicine. I shall not, like Dr. Marshall Hall, attempt to explain the nature of inflammation, or determine its proximate cause, but shall content myself with endeavouring to arrange its phenomena, and point out their order, and the share which the capillaries have in the inflammatory process. Before entering on this subject, it may be necessary to premise a few observations on the circulation in general.

The human body is composed of various parts, differing in their ultimate structure, chemical composition, and vital functions. There is a very remarkable difference between muscle and cellular tissue, and between the latter and nervous tissue. If we examine these parts more closely, we find them differing, not only in their structural arrangements, but also in the ingredients or materials of which they are composed. In muscle we find a large quantity of fibrin and colouring matter; in cartilage, fibrous membrane, and tendinous substance, we find more or less of the *fibrous structure* of muscle, but we do not meet with *fibrin*, and there is not the slightest trace of colouring matter. The same blood furnishes materials for the growth and nutrition of all, and conveys the nutrient particles to red and white tissues alike; but the white parts require not red blood, and consequently receive none. Blood is a compound fluid, which contains, as it were, the raw material of all the tissues in a fluid state; it is, in fact, flesh in a state of fluidity, and destined to combine with and support the solid portions of the frame. It is conveyed by the arteries all over the body, supplying each tissue with its appropriate materials, and contributing to its growth, sustentation, and repair, in the amplest, and yet in the most economical manner. It does not enter the tissue of every organ, in that state which has been termed arterial, and in which it appears as a fluid of a bright red colour. This is an error of which nature is never guilty. It would be absurd if all parts of the blood were carried to all the different tissues indiscriminately; and it would moreover be a great waste of vital and mechanical power. The chief bulk of the blood is made up of a transparent fluid or lymph, holding in solution various salts, besides albumen and fibrin. The red globules are immersed, but not dissolved, in this fluid; and it appears from the observations of Mayer, that in the minute vessels, the red globules occupy the central part, surrounded by the transparent fluid. The colouring globules are necessary for the nutrition of muscular, mucous, and some other tissues; and are carried by the minute vessels wherever they are required. Every part of the blood is required in a muscle: fibrin and colouring matter for its essential fibre; albumen, fatty matter, &c. for its cellular and adipose membrane. The white tissues, as I have already observed, receive no red blood, because they require none; this is quite certain. Serous membrane, for instance, contains neither fibrin or colouring matter: at what point of the circulation does the separation of the albumen take place? is it an act of nutritive secretion which separates it from the whole mass of arterial blood, or are only the serous portions of the blood carried to the white tissues? "Serous vessels," says Müller, "that is, branches of the blood-vessels which are too minute to allow the passage of the red particles, and which are traversed, therefore, merely by the lymph of the blood, may possibly exist, but they have not been demonstrated."

It seems to me, however, that it is by

no means necessary for blood-vessels to be too minute to allow the passage of red globules, in order to make these vessels the vehicles of lymph alone. The entrance of the globules into them will be determined by other circumstances than their size. Already, as the blood approaches the capillary system, the microscope detects a tendency to a separation between its lymph and colouring globules; and no doubt their complete separation is effected by vital agencies, independent of mere calibre. Hence we may explain the fact, that no red blood seems to circulate in serous membranes during health; but the moment inflammation sets in, the natural play of vital energies is deranged, and the red globules, finding their way into unwonted channels, vessels innumerable, before filled with a transparent lymph, and therefore not visible, start suddenly into view, in consequence of their now containing an opaque and coloured fluid.

According to Hall, Müller, and other physiologists, all minute vessels contain red particles, which, however, are believed to exert no influence on their colour, so long as these particles are only admitted singly, and not several at a time. But when inflammation comes on, according to Hall these vessels are enlarged in consequence of obstruction, and then, admitting a greater proportion of red globules, become visible. Now, gentlemen, observe how suddenly, when the conjunctiva connected with the sclerotic is irritated, numerous vessels appear filled with red blood. Here is no time for the adhesion of globules to the internal surfaces of the vessels—no time for the gradual enlargement of vessels previously too small for the admission of the red globules; no, the vessels existed there, but they contained no red globules; they admitted none, because their admission would have proved unnecessary or injurious. I do not deny the sudden enlargement of minute vessels; on the contrary, I believe in it most firmly, and am persuaded that the minute and capillary arterial branches which admit, in health, only lymph, may suddenly expand and increase in size. I do not, for reasons hereafter to be detailed, consider this expansion as passive; and I believe that the red globules made little or no part of the fluid previously circulating in these vessels. Indeed, it seems rather illogical to argue that, because red globules might be present without imparting a perceptible red colour to this fluid, that, therefore, they are present. When the contents of a vessel are to the eye colourless, the *onus probandi* lies with him who asserts the presence of red colouring matter; and, until that is proved, in each particular case, the contained fluid must be regarded as colourless.

As to the idea that lymph vessels could not exist unless their diameter was smaller than that of the red globules, it is too mechanical to deserve serious attention. The entrance of animal matters into, and their propulsion along vessels, depend most assuredly on other conditions than mere size of particles. Indeed, Müller expressly says—"In the most minute capillaries which are not red, nor even yellow, but quite transparent, there is merely a single line of red particles, separated by unequal intervals, *and from time to time no red particles are seen in these colourless vessels;* but I have seen no canals through which red particles did not occasionally pass, and which, therefore, deserved the name of *vasa serosa*, and Wedemeyer, who says he has seen such *vasa serosa*, himself confesses that some of the red bodies traversed them from time to time. Here, then, we have my argument confirmed by observation, and the fact proved, that the *entrance and passage of the red particles does not depend on the mere size of the vessels.*

[To be continued.]

INTERESTING CASE OF INGUINAL HERNIA;

Operation followed by Hæmorrhage, Secondary Bleeding; Cure.

Communicated

By E. W. Tuson, F.R.S.

Surgeon to the Middlesex Hospital.

Thomas Guyner, æt. 32, residing at No. 1, Leicester Court, Castle Street, Leicester Square, a painter by trade, sent for Mr. Lane on the 27th September, on account of being very ill, with constant vomiting. Mr. Lane being confined to his bed, his assistant went, and found the patient had a strangulated hernia in the left inguinal region,

which he considered required the operation to be performed, and therefore sent for me. I found the man suffering from severe vomiting of fæcal matter, constipation of the bowels, a tense tumor in the left inguinal region, about the size of a hen's egg. The man complained of much pain upon pressure, which ran towards the umbilicus, and had much tenderness in the abdomen. He had already been placed in a warm-bath, and bled twice, and several attempts had been made to return the hernia, but without effect. The surgeon who had so treated him had impressed upon his mind the necessity of having the operation immediately performed, but he would not then consent to have it done. The patient stated that he had had the swelling as long as he could recollect; sometimes it would become as large as an egg, and oblige him to leave off work, when he would sit down and draw his thigh towards his body, and then the swelling would nearly disappear. He had never worn a truss, and the swelling had been nearly the same size for the last four days, but it might have got a little larger. He was suffering a great deal from pain, hiccough, and vomiting; he would not consent to be taken to the hospital, but entreated that something might be done for him at his own lodging. I urged the necessity of performing the operation, which he then consented to, and, in the presence of several of my pupils, I commenced by pinching up the skin and dividing it over the tumor, which occupied the seat of the inguinal canal. I cut through the fascia superficialis, when the external pubic artery was divided and secured. The tendon of the external oblique was then divided, by a director having been introduced through the external abdominal ring; and it was then found, by dividing another layer, that the hernia was behind the spermatic cord, which was drawn down by means of a blind hook. The hernial sac was then cut through, and about an ounce of clear serum escaped; the opening in the sac was enlarged to the extent of about three-quarters of an inch, when I introduced my finger and felt a small knuckle of intestine, which slipped into the abdomen by a little pressure, without the stricture having been divided. A portion of omentum was in front of the intestine, which was likewise returned into the abdomen. When I withdrew my finger I was surprised to find that it was followed by profuse bleeding, the blood being arterial, and coming out of the sac *per saltem*. Having inserted my finger in the sac, and endeavoured to find from whence the blood came, I found the sac full of coagulated blood, and brought out several clots, and also several pieces of coagulated lymph. Upon consideration whether, under these circumstances, to cut open the sac and look for the bleeding vessel, or whether to unite the edges of the sac by suture, as the finger remaining in the wound stopped the hæmorrhage; or again, whether to apply a graduated compress,—I resolved to adopt the latter plan; and in case that should fail, to lay the sac freely open and look for the vessel. I therefore withdrew my finger, and applied a graduated compress and a roller, when, finding that the bleeding had ceased, I left the man, having previously ordered him an aperient.

Upon visiting him in the afternoon I found that there was no appearance of blood, that the vomiting had in a great measure ceased, but that the hiccough still continued. The medicine had not acted. In the evening of the same day the patient was better, having slept an hour or so. The following morning further amendment had taken place; the bowels had not acted; the vomiting had ceased, and also the hiccough. Another dose of aperient medicine was ordered for him. In the evening the man was much better; the bowels had been freely open, he complained of no pain, and the abdomen was free from any tenderness upon pressure, except at the seat of the incision; and he appeared to be going on very well.

The next day, Dec. 29th, I examined the wound, and found a swelling about the size of a small walnut, situated behind the seat of the incision, which no doubt was coagulated blood; and upon pressing above the anterior superior spinous process of the ilium, and continuing the pressure downwards, there was some discharge of serum, followed by pus. The edges of the wound were brought together by adhesive plaster, and a compress and roller applied. There was no appearance of any disposition of the wound to heal, and I pressed my hand upon the swelling to observe if there was any pulsation in it; but not

the slightest was to be felt. The sickness had entirely disappeared, and the patient felt no pain, and stated that he was well, but wished for something to eat. He was allowed a cup of cool tea, and a piece of bread and butter. The following day he was going on well, complained of great hunger, and wished to eat some rabbit, which was allowed him. The wound had much the same appearance as on the preceding day, and was dressed in the same way.

Dec. 31.—The patient is going on well; instead of eating a piece of the rabbit, he eat the whole of it, and feels no inconvenience from having done so; bowels act freely, and tongue clean. From this day until the end of January he went on very well; the swelling gradually disappeared, suppuration took place, and the wound had nearly united, when he was advised to get up and sit on a chair by the side of the bed. This he attempted to do, but when he got upon his legs, he says that they bent under him, and that he fell upon the floor, when there was a little bleeding from that part of the wound that had not quite united. Upon examination, there was a small swelling situated in the same place as the former one, which I treated in the same way, and he went on well until the 18th of February, when a cat got upon the top of the bed, and before the man was aware of it, jumped upon the lower part of his abdomen, just upon the seat of the wound: bleeding again took place. The next day there was a tumor the size of a goose's egg; I introduced my finger, and removed some coagulated blood and lumps of coagulated lymph; there appeared a little disposition to bleed, but it immediately stopped upon the application of slight pressure: a compress was applied, and a roller. The next day there was a swelling as large as the day before: the compress and roller were continued. This swelling the following two days increased, and I again removed various pieces of coagulated lymph and blood, applying a compress and roller afterwards. The following day the swelling again appeared, and Mr. Mayo was requested to have the goodness to look at the case: he was of opinion that as there had been a swelling after the operation, which had gradually disappeared by the application of a graduated compress, the same plan should be persevered in, and this was in consequence followed, and the patient went on well until the 1st of March. On that day he had a fit of sneezing, and bleeding to a considerable extent took place. I was sent for, the messenger stating that the man was bleeding to death. Upon arriving at the patient's, I found a swelling of considerable extent, and a great quantity of blood issuing from the small wound. I inserted my finger into the aperture, and found a great mass of coagulated blood, and thought I felt the vessel pouring out the blood into the surrounding cellular tissue. I kept my finger on the vessel, and sent to request the favour of Mr. Mayo's or Mr. Arnott's assistance. Mr. Arnott arrived in about half an hour, and an incision was then made from the wound, extending upwards and outwards to the anterior superior spinous process of the ilium, and downwards to the pubes, which was enlarged in a crucial direction, so as to have a free view of the adjacent parts. The coagulum was now freely removed; it extended behind the pubes, running down within the pelvis. The fascia transversalis was brought into view to a considerable extent: the peritoneum and intestine were distinctly seen behind the fascia. The bleeding appeared to arise from the interior of the pelvis behind the pubes. Two or three small vessels, branches of the epigastric artery, were tied, but still the hæmorrhage continued. Pressure was then applied upon the external iliac artery, high up, but this did not command the bleeding: this vessel was compressed several times, but the pressure had no influence over the bleeding, which still continued. Fresh efforts were made to discover the bleeding vessels, by enlarging the wound, but without success; the man appeared exhausted; there was little or no pulse at the wrist; and as upwards of three hours had been lost in the endeavour to discover the bleeding vessels, and as the patient had not strength enough to bear the operation of tying the common iliac artery, it was resolved to insert a large sponge into the wound, and apply a graduated compress over it, which was done, and a roller increased the pressure. The next day, March 2d, the man had passed a restless night, from pain in the wound; the bleeding had ceased; the pulse was low and feeble, the tongue white and furred,

but on the whole he was much better than could have been expected under the circumstances.

3rd.—He is better this morning; slept well, owing to an anodyne draught which he took last evening. Tongue less furred; pulse a little stronger, but still very feeble. As the bowels had not acted, two pills of colocynth and calomel were ordered; no appearance or disposition of bleeding.

4th.—The patient is better; pulse still very weak; bowels not open; tongue a little cleaner. The roller was taken off, and the compress removed; the sponge was allowed to remain in the wound, and there was no disposition to bleed; the graduated compress and roller were re-applied.

5th.—The man is still going on well; he took three doses of the pills yesterday; the bowels have acted twice; complains that his mouth and gums are very sore; that he cannot swallow any thing, in consequence. The roller and compress were removed and re-applied. No appearance of any fresh bleeding.

6th.—The patient is doing well. The mouth and gums are very sore; pulse weak; tongue furred; the wound looking healthy.

7th.—Going on much in the same way.

8th.—The man is improving, but complains of the mouth and gums being very sore; bowels have been open, but he cannot make water. A catheter was therefore introduced, and the urine drawn off. The roller and compress were removed, and the piece of sponge was taken out of the wound with some little difficulty, it having now been in for a week: it was washed, and immediately introduced again, as there was a little disposition to bleed, which appeared to come from the granulations.

From this time the patient gradually improved; he was enabled to make water in about a week afterwards: the sponge was removed twice a week, and each time a small piece was torn off, so as to lessen its size, until at last nothing but the compress was put over the wound, which gradually healed; and on the 27th of May he got up, for the first time, having been in bed five months. The patient is now well, being enabled to walk: the wound healed, and he is perfectly recovered.

The question in this case is, What artery was it that the bleeding came from? Was it the epigastric? the obturator? the spermatic? or was it from the arteries of the omentum? adhesion having formed between the sac and omentum, the adhesions being broken down when the omentum was returned. The latter appears the most probable; but even if this had been the case, would there not have been a chance of bleeding in the cavity of the abdomen? which there certainly was not.

I do not believe that the hæmorrhage came from the spermatic artery, as that artery was seen in front of the hernial sac, and the bleeding came from the interior of the sac. If the stricture of the hernia had been divided, the epigastric might have been wounded; but no incision was made after the sac was cut through, and the serum discharged, the intestine and omentum being returned by the finger, and when the finger was withdrawn the blood immediately followed.

It is difficult to say what artery the bleeding came from: if it were from the obturator, that artery arising from the epigastric, its course would have been behind the sac, and not within it. Could it have been from the artery of the cord? I believe not; for I do not think that the quantity of blood could have come from such a small vessel. At the same time it is extremely satisfactory to find that the means adopted were such, that the cure of the patient was the result.

WATER-CUSHIONS.

To the Editor of the Medical Gazette.

Sir,

Allow me to remind or inform the professional public, that the air-cushions sold in the shops, made of waterproof cloth, become very convenient substitutes for the hydrostatic bed, when filled with water. A patient in this hospital has used one for the last three weeks with the best effect. The bed-sores on the hips and sacrum, from which she suffered dreadfully, are healing rapidly, although the sacrum was laid bare; and she expresses herself as receiving the greatest comfort from it. I provided her with a Macintosh neck-

pillow, which has very much the shape of a harness-collar. The loins and hips are raised, and it is of course necessary to elevate the shoulders and back to the same level by means of pillows.

A Macintosh sack, four feet deep and three and a half broad, moderately filled with water, and laid on a common mattrass, would afford the comforts derived from the hydrostatic bed, be much less expensive than the latter, and very convenient from its portability. It would support the whole trunk, and so prevent sores being formed on the shoulders. These water-cushions might be made extensively available in both hospital and private practice. It would, of course, be necessary to be very careful how they were used, after patients suffering from contagious diseases, especially erysipelas. I do not wish these remarks to be considered as offering any thing new to the public.

I am, sir,
Your obedient servant,
THOMAS LAYCOCK,
House Surgeon to the County Hospital.
10th June, 1838.

MEDICAL GAZETTE.

Saturday, June 23, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

ON THE PRESERVATION OF BODIES FOR DISSECTION.

ON Friday evening, a conversation took place in the House of Lords between Lord Brougham and Lord Radnor, on the occasion of the presentation by the former of a petition from a person named Roberts, praying the interference of the Legislature to enforce the use of a material which he says he has invented for the prevention of decomposition in animal bodies. We took occasion last October to notice the disgraceful attempts which this man was making to obstruct the study of anatomy, by writing to the Vestry Boards of different parishes, and placarding the populous poor neighbourhoods of the metropolis, with exaggerated and false assertions of the indecency with which the bodies of those consigned for dissection were treated; and of other evasions of the regulations of the Anatomy Bill. We scarcely expected that a person whose interested designs might have been learned by an hour's inquiry into his case, would have found two noblemen to support him in the House of Lords, or even to appear to give him the patronage of their names. Yet the Bishop of Exeter and Lord Brougham have on two separate occasions brought him before the House and the public. As this could scarcely have occurred if the facts of the case had been fully known, we shall now publish them—only prefacing them by a positive assurance that their truth is certainly and clearly known to us.

Mr. Roberts, who had been a dry-salter, found, as many others have done, that it is possible to prepare portions of the human body, as of all other flesh, so that they will not become putrid for some months; and imagining he had made an important discovery, he exhibited his preparations to some of our leading surgeons and physicians, who told him, what was quite true, that his preparations were very well made. Certainly as matters of curiosity they were interesting, and the plan seemed somewhat better than most of those before invented for embalming, or mummifying—so that if it were desirable to preserve the bodies of the dead in their coffins, so that they might retain somewhat of the form they had during life, the material supplied by Mr. Roberts would probably fulfil that purpose with sufficient certainty. None of them, however, gave him the least encouragement in his endeavour to make it subservient to the purpose of dissecting; nor indeed could any one who had ever been in

the habit of studying practical anatomy, imagine for a moment that it could be employed in that pursuit However, seeing no prospect of making money by preserving the bodies of the rich, for burial, Mr. Roberts made repeated and vain applications to the College of Surgeons, to induce the council to recommend his invention to be used in the schools. They, however, refused to do so, and he then made a general offer to the teachers of anatomy themselves, that he would supply all the schools with material sufficient for the preservation of all the bodies used for anatomical purposes, on condition that each pupil on entering to the right of dissecting *should pay him half-a-guinea*! This modest request to compel the pupils, whether they liked it or not, to provide Mr. Roberts with a sinecure of five or six hundred a year, was of course refused and laughed at. In revenge, he wrote to the boards of different parishes, urging them to refuse to give up the bodies of the unclaimed poor for dissection, and declaring that Dr. Somerville had entered into a league with the anatomical teachers, to permit the evasion of the regulations of the bill, which it was his duty to enforce. Dr. Somerville was in consequence requested to attend at the boards, when he pointed out the designs and motives of his accuser, and perfectly satisfied them of the utter groundlessness of his assertions. At the same time bills were printed and circulated among the poor of several parishes, to persuade them to prevent in every possible way the supply of subjects for dissection. However, all these means were perfectly futile; and, finding that during the last season he could make no impression on any of the parties concerned, Mr. Roberts has had the impudence to petition the House of Lords, to force the students to employ what would be both useless and mischievous in their studies, and, as we have seen, he has found two peers to support him with their names.

The whole scheme is so absurd, that had either of these noblemen been acquainted with the circumstances, they could not have entertained it for a moment. Had it been at all desirable to preserve the bodies for dissection longer than in the natural course of decomposition they can be conveniently used, numerous means (and probably the very one which Mr. Roberts employs) were known long since. A solution of nitre, for example, injected into the vessels and cavities, will preserve the whole body for a month or more, according to the dryness and heat of the weather; a solution of nitre, common salt, and muriate of ammonia, for a still longer period; sulphate of iron similarly employed in a strong solution will keep the body from putrefaction for five or six weeks; and a strong solution of corrosive sublimate will so completely preserve it, that it may be kept for years. Many so treated have been dried, and placed in glass cases. A M. Gannal, not long since, presented portions of the body completely preserved by the injection of a solution of pure acetate of alumina, to the French Academy of Sciences, in the hope that it also would be employed in dissections; but in vain. Lastly, the pyroligneous acid, used in a similar manner, will maintain the body, as it does hams, tongues, &c. without the least putrefaction, for many years. All these, and many others, have been frequently tried in anatomical schools, and all have failed; for although they completely preserve the body from putrefaction, they all alter the character of the tissues, and are accompanied by so many other inconveniences, that, except under particular circumstances, when it is desirable to keep a single body for an unusually long period, they are now

never employed. We notice them, however, to shew that it is not for want of the power to do it that bodies have not been preserved in all our schools; and we may add, that Mr. Roberts's method if it be not the same as one of those already known, is not at all superior to either the corrosive sublimate, the acetate of alumina, or the pyroligneous acid.

Dissecting (with which we suspect Lord Brougham is far less acquainted than he would have his compeers think) is not intended merely to teach the relative position of the various organs of the body, or of the different parts of a limb, but is even more necessary, in order that the student may acquire a knowledge of the sensible properties of each of the objects presented to him; their various degrees of hardness; their density, colour, vascularity, &c; — that when called on to operate or to make an examination, he may know each tissue at first sight, and on the instant it presents itself to him. Were it not for the necessity of learning these, anatomy might be studied well enough from drawings, or from *papier maché*, or wax figures, or from Mr. Roberts's or any other mummies. But no material has ever yet been discovered which will preserve the body from putrefaction, without at the same time so altering the texture of its parts as to make it nearly useless for dissection; and the present plan does this so remarkably, that no student who had endeavoured to learn his anatomy from Mr. Roberts's preparations, would be in the least likely to know an artery from a vein, or a tendon from a nerve, when he saw them in a living or fresh body. Every thing is converted into an almost homogeneous mass of hard, dry, ham-like substance, so little resembling the material of the human body, that, but for its form, one might doubt its origin.

However, the design of this man is so purely that of serving his own pocket, that we must be allowed to point out the absurdities which he urges as reasons why the legislature should interfere for him. He says that the bodies of the poor are indecently treated—at least he tells this to the Bishop of Exeter. Now this is a plain untruth; for if he were aware of it as a fact (and we know he was constantly prying into the dissecting-rooms), the law was open to him to have the penalties enforced. But supposing that it had been the case—supposing that parts of bodies were never buried, but were made preparations of — how could it prevent this, that they should be secured against putrefaction? It must be clear to the commonest perception, that the most likely mode of inducing students to retain portions of bodies, would be the adoption of any plan by which decomposition is resisted.

He says, again, that if his pickle were used, one-fourth of the present supply of subjects would suffice, which is another error (to use the mildest term). It will not be denied that it would be desirable that each student should dissect the whole body once in each year, during his studies in London; because without this, his knowledge of anatomy cannot be so certain or complete as to fit him for the emergencies of surgical practice. But there is not more than one student in ten who does this, or who can possibly, in the present state of things, effect it; and a majority of the students do not, in each year, dissect more than one-third of the whole human body. The number of subjects supplied last year was not so great as the number of new pupils who then commenced their studies; so that supposing the pupils of two years (which is below the probable total) to be dissecting each season, the number now annually supplied, including those required for lec-

turers and demonstrators), would not be sufficient to afford half a body for each pupil. It would certainly, therefore, be imprudent to diminish the supply.

It is said, again, that dissections might be carried on in the summer—forgetting that there are other studies for which the summer season is especially devoted, as botany, forensic medicine, midwifery, and too large a number of other minor subjects, for which there is no time in the winter.

Again, that the health and lives of the pupils would be preserved, as asserted, we should like it to have been first proved, that the material suggested would prevent illness or death among them; and next, that dissection is in itself an unhealthy occupation, or that its unhealthiness is the consequence of the putrid effluvia which the student is occasionally exposed to. We are ourselves prepared to prove, that the terrible results which sometimes, though rarely, arise from wounds received in dissection, are in no degree the consequences of the putrefaction that the body has undergone, but that the putrefaction is itself a preventive of those injuries, because, in proportion as the body is more advanced in decomposition, the less is the probability that inoculation of the fluids it contains will produce any serious consequences. Every one acquainted with medical schools or practice knows, that the persons affected by dissecting wounds, are they who occasionally open the perfectly fresh bodies which are subjected to post-mortem examinations; and not among the students, who are constantly dissecting bodies which have been dead from three to twenty days, such an occurrence is exceedingly rare. Those whose health suffers more gradually during their studies, can seldom if ever trace it to dissecting, nor, as a general rule, is the health of medical students at all worse than that of any class of young men who work equally hard. Indeed, the result of the experience of all engaged about dissecting rooms, is, that the occupation is not inimical to good health; and it is constantly observed, that no class of men is more generally robust, or less subject to disease of any kind, than those who exist surrounded by decomposed animal matter, as tallow-chandlers, tanners, knackers, &c.

What we have said is sufficient to shew that Mr. Roberts's scheme is useless and unsatisfactory for the anatomical schools; and an impudent attempt to obtain a sinecure of 500*l.* or 600*l.* a year for himself. Should the subject be again mooted, it may now be far more efficiently met than on the last occasion.

UNIVERSITY OF LONDON.

The motion of Mr. Gibson, on Thursday last, for laying before Parliament the minutes of the proceedings of the University of London, our readers are aware was refused by the Home Secretary. This refusal of information is glaringly inconsistent with those large promises which were made on the formation of this institution — that the utmost publicity should be given to the proceedings—and has naturally created a suspicion that some transactions have taken place which will not bear exposure. Can the rumour be true, that the disgust excited by the domineering spirit of a certain member of the senate has now risen to such a height as to threaten the very existence of this embryo University? or is the cause of the dissensions which exist, to be found in the conduct adopted with respect to the appointment of a gentleman to a seat in the senate, who, when candidate for an inferior office, had been almost unanimously rejected? However this

may be, we understand that nearly all the members of the Medical Faculty have resigned; while from the denial of this fact, and the endeavours made to induce them to resume office, it is evident that the patrons of the Metropolitan University are in great alarm.

It is strongly conjectured that had the production of the papers moved for by Mr. Gibson been ordered, the minutes of the proceedings would have conveyed to Parliament very unsatisfactory information regarding the real difficulties which have hitherto frustrated the success of the University.

PROGRESS OF ANIMAL MAGNETISM.

We are very glad to find that the public exhibitions at University College Hospital have been discontinued. The objections to the course adopted were so very palpable, that we are astonished they did not sooner lead to this result; but as we abstained, at the time when the proceedings alluded to were going on, so we shall not now indulge in any remarks which might be deemed invidious.

The subject of mesmerism meantime is undergoing private investigation by some gentlemen who have voluntarily undertaken the task, and who, we trust, will conduct the inquiry in a manner calculated to satisfy men of science, and without pandering to that passion for the marvellous by which this question has hitherto been so lamentably debased.

It has been stated in several of the newspapers, that a Committee, including Dr. Roget and Dr. Faraday, has been appointed by the Royal Society, to investigate the phenomena of animal magnetism. We find, on inquiry, that there is no truth in this report.

REPORT OF THE COUNCIL OF THE EASTERN BRANCH OF THE PROVINCIAL MEDICAL & SURGICAL ASSOCIATION.

June 12, 1838.

The following is a brief report to be laid before the general meeting of the Eastern Branch of the Association; and in preparing it there are presented the strongest grounds for congratulation on the results of the transactions of the year. The junction of the Eastern with the parent Association has been productive of that mutual benefit which arises from the associated interests and the increased numbers of its members. The expenses of the Eastern Branch have been kept considerably within the prescribed limit, and the balance in hand, at the end of the last year, has been transferred to the treasurer, at Worcester. It is difficult, in so wide a district as the Eastern counties embrace, to gather the list of our loss of members by death; but in one town, where we had six, a third of the number have thus been severed from us; an accession of new members is requisite to meet such unavoidable vacancies; and the new members to be proposed this day will evince that we have more than an equivalent supply.

The fame of the Provincial Medical and Surgical Association, of which the eastern branch now forms so considerable a part, is marked out for posterity, from its being the only medical body, not fixed in a metropolis, which has been able to publish a series of its Transactions. The sixth volume is now complete, and will soon be in the hands of every member, who must feel it to be his duty to reflect whether he cannot contribute to render each future volume richer in practical information than those already before the profession.

Whilst it is a source of disappointment to every zealous member in these eastern counties, that the great annual meeting of the Association continues to be held by invitation in the northern or western parts of the kingdom, (a circumstance attributable to the rapid march of improvement, and the more enlightened spirit abroad and active in newly-flourishing and populous towns), such an arrangement, precluding the attendance of most gentlemen in this district, must not be viewed with an unavailing regret, but be converted to benefit by fresh efforts being made to enhance the value of our local annual meetings.

Amongst the scientific business to be brought forward, besides a biographical memoir of the late Dr. Nathan Drake, of

Hadleigh, there are several papers, some calculated to excite discussion, which, courteously and temperately conducted, never fails to elicit useful information. The powerful public influence of a large association is made obvious by the ready channel it affords for communicating whatever bears upon the interests of the profession generally. There are also private effects or such as relate to individuals, which ought not to be overlooked; the renewal of old, and the formation of new acquaintances, are amongst the many advantages derived from our annual meetings, which have been successively held at Bury St. Edmund's, Ipswich, and Norwich. We are now assembled in a town where it is believed so many respectable members of the profession from several contiguous counties—Suffolk, Norfolk, Essex, Cambridgeshire—are for the first time collected; and it is trusted that the proceedings will be marked by that dignity and usefulness—that good-fellowship and liberality—which will secure new strength for future similar occasions, and contribute something to the interests and honour of our noble profession."

Nine new members were admitted into the Society, viz. Dr. Maclean, of Colchester; Mr. Harvey, of Castle Hedingham; Mr. Culledge, of March; Mr. Bullen, and Mr. Fison, of Newmarket; Mr. Hutchinson, of Waltham; Mr. Chater, of Norwich; Mr. Strowger, of Coggeshall; and Mr. Foaker, of Baddon.

SOUTHERN BRANCH OF THE PROVINCIAL MEDICAL ASSOCIATION.

The annual meeting of the southern branch of the Association was held at Salisbury on Thursday, the 14th inst.; and the attendance was commensurate with the magnitude of the objects contemplated in the establishment of these meetings. The leading members of the medical profession resident in Salisbury, and those of Warminster, Calne, Hindon, Chichester, Southampton, Winchester, Portsmouth, Portsea, Blandford, Bridport, and the other towns in Dorset, Wilts, Berks, and Hants, were present, to the number of between 60 and 70.

Shortly after eleven o'clock, the chair was taken at the Council Chamber, by the President for the year, Dr. Fowler, who briefly opened the proceedings by adverting to the important objects of the Association, and mentioning some of the matters which would be brought under the notice of the present meeting. He then called upon Mr. Wickham, of Winchester, (the permanent Secretary), to read the Report for the past year. After which, the report of the committee appointed to note the progress and position of medicine and medical literature during the past year, was read, of which the only portion we can give is that which related to small-pox:—

The prevalence and mortality of small-pox of late, both among ourselves and on the continent, has directed the attention of the profession in an especial manner to the subject. The frequent occurrence of this disease in individuals after vaccination, and the occasional fatal termination of it, has, I fear, caused many desertions from the Jennerian standard; and until a digest of faithful facts be obtained, opinions must of necessity be unsettled. Among the recent contributions to the history of small-pox and vaccination, those of Dr. Wendt, of Copenhagen, are the most valuable. Copenhagen having been visited with epidemic small-pox twice during the last six years, and Dr. Wendt having had the management of the small-pox hospitals in that city, his experience was considerable, and his opinions must therefore have great weight. In the first of these epidemics 1045 patients were received into the small-pox hospital, of whom 45 died—one in 24. Eight hundred and ninety-eight had been vaccinated; one hundred and forty-seven not vaccinated: of the former, ten died; of the latter, thirty four; making one death in four in unvaccinated, and one in ninety vaccinated.

In the second epidemic, of 1197 cases treated by our author, 1043 were vaccinated, 154 not vaccinated. Of the former, 47 died; of the latter, 51. No case of the disease occurred in re-vaccinated subjects. In those who had been vaccinated and not completed their fourteenth year, the disease was not seen in its genuine form; and of those who had been vaccinated, none had died before their twenty-third year. From these and similar facts, Dr. Wendt is of opinion that the protecting power of vaccination is weakened after many years: re-vaccinations he considers indispensable, and that the interval between each should be six years. Dr. Wendt thinks that neither inoculated or natural small-pox certainly protects the patient from a second attack.

The Report having been read, Dr Oke, of Southampton, moved that the thanks of the meeting be given to Dr. Forbes, Dr. Harris, and Dr. Bullar, for their excellent Report; which was seconded by Mr. Andrews, of Salisbury, and carried unanimously.

The President next called for the report of the committee on Surgery; and Mr. Sampson, of Salisbury, accordingly read an able report thereon. The first subject treated of is the comparative merit of lithotomy and lithotrity; and the latest improvements in the instruments connected with those operations. Various

new modes of treating hernia, with a view to permanent cure, were described; an interesting case of cure of aneurism by the application of ice was related. The report then detailed new plans of treatment for varicose veins, one of which plans consists in passing a pin through the enlarged vessel, and allowing it to remain a sufficient time. In erysipelas the application of raw cotton, well carded, had been found highly beneficial. In lacerated or contused wounds, the application of a solution of chloride of lime had speedily relieved the pain. A case was mentioned in which mortification was successfully treated by tight bandaging; but the treatment failed in some other cases in which it had been tried. Several new methods of cure of hydrocele had been discovered; and improved means of treatment for ophthalmia, by the external use of calomel, and in other cases by the application of small blisters to the eyelids, were mentioned.

Dr. Irvine, of Portsmouth, moved the thanks of the meeting to Mr. Sampson and Mr. Mayo, for this excellent report. In reference to the subject of varicose veins, Dr. I. related the circumstance of a medical officer attached to a regiment at Portsmouth, a young man, who was so troubled in this respect, that he was unable to attend to his duties; and who relieved himself by passing needles under the veins, and using ligatures which were twisted, to produce compression, somewhat similar to the operation for hare-lip. By this means he completely succeeded.

The motion was seconded by Mr. Corfe, of Southampton, and carried unanimously.

Various other reports were read, and followed by much interesting discussion, for which we cannot make room.

A vote of thanks having been passed to the chairman, and duly acknowledged, also a vote of thanks to the mayor, for his kindness in granting the use of the council chamber, the meeting separated.

MORTALITY IN PHILADELPHIA.

The deaths in the city and liberties of Philadelphia, during the year 1837, were 5202, of whom 2755 were males, and 2447 females. There are reported as having died from consumption of the lungs 748, nearly one-sixth of the whole number. Allowing for inaccuracy of report, and the probability that many diseases of the lungs not tuberculous are included under this head, the preponderance is still overwhelming, when we see the next highest in the list, convulsions, only 294. From scarlet fever 205 deaths occurred, from small-pox 79, from measles 49, from varioloid only 2. —*Philadelphia Medical Examiner.*

ON THE STRUCTURE OF ERECTILE TISSUES.

In our number for January 6th, 1836, we published a detailed account of the discoveries of Professor Müller in the minute structure of the penis, the substance of which was that there exists a separate series of minute arteries (which, from their form, he named *helicine*) projecting into the venous cells, and producing erection of the organ by the increased flow of blood through them, under circumstances of nervous excitement. His investigations were certainly of great importance, not only as establishing the minute structure of the organ described, but, in a general point of view, as relating to a distribution of vessels to which no similar arrangement had been presumed to exist in the animal body—viz. arteries terminating with free extremities, in cells communicating with veins. We have now, however, to notice a complete refutation of the views then advanced, and since almost universally received, which has been published in the last number of Müller's *Archiv für Anatomie und Physiologie.* It is from the pen of Professor Valentin, the well-known author of the Entwickelungsgeschichte, whose talent for minute observation is certainly not inferior to that of Müller himself.

He says, that the result of numerous examinations has convinced him that the so-called helicine arteries are by no means peculiar vessels, terminating with their extremities, and hanging free in the cells of the corpus cavernosum, but only minute arteries that have been divided or torn; and that, on the contrary, the real distribution of the vessels of the corpus cavernosum follows in every respect the most simple laws. In making the injections of the penis, different portions of it receive different quantities of injection; in general the posterior half is most injected, and of this the anterior fourth is best adapted for examination, because in it the injection will probably have exactly filled the minute arteries without passing into the venous cells. If a transverse section of a portion thus injected be made, one sees on its surface, together with arteries of various sizes running tortuously in the uninjured fibrous cord-like partitions* of the cells of the corpus cavernosum, the *helicine arteries*—that is, arteries which, to the naked eye, or with a lens, seem to terminate suddenly, either singly or in tufts, which lie partly on the fibrous cords, but principally hang loose in the cells, and

* *Balken*, beams: the fibrous cords or bands which bound and traverse the so-called cells of the spleen.

which when placed in water appear fixed at one end, while the other floats out in it. They all, even to the naked eye, appear completely inclosed by a membrane exactly like the tissue of the partitions; and if they are examined with a microscope, their ends appear sometimes rounded, sometimes obliquely or unevenly truncated, sometimes granular or even irregular; in a word, so inconstant is the form of their terminations, as at once to suggest the opinion that they are unnaturally formed.

If a cleanly cut transverse section be examined with a good lens, with which a view to some little depth may be obtained, it will at once appear that there are helicine arteries only at the surface, and in the cells lying near it; but that in those cells which lie deeper, no trace of them can be seen. At the same time it may be remarked, that every fibrous cord, without exception, contains an artery of proportionate size, which runs in it tortuously, or rather in the form of a cork-screw; and that these arteries, like the fibrous cords in which they lie, communicate together. If the surface of the section be examined under water, it will be seen that at the divided extremity of each fibrous partition, one or more helicine arteries seem to be given off, according as one or more smaller fibrous cords are given off together or separately from the chief one. These smaller fibres, when they were divided, had separated and contracted a little, and thus, and by their naturally winding course, the tendril-like or crozier-like terminations of the supposed helicine arteries were produced. Thus one sees how the helicine arteries are formed under one's own eyes. And wherever the minute arteries are filled with injection they may be made to appear helicine by dividing the fibrous cords in which they lie. In a longitudinal section the same thing may be observed, only that here still more partitions being divided, more helicine arteries are seen; and more still may be made by cutting the corpus cavernosum, as one would with a saw; or by washing out the injection from the cells into which it has run from the arteries, and so tearing a greater number of the extremely minute cords. By the careful examination of several of the arteries, and their fibrous cords supporting them, which are thus divided, a sufficient proof may be obtained that the apparent enlargement of their extremities, the closeness of their orifices, and their tortuous or tendril-like course, depend merely on the mode in which the section has been made, or on some artificial means employed in the examination.

In the posterior part of the corpus cavernosum in man the cells are large, and the fibrous cords traversing them very delicate, so that as all the minute arteries run tortuously on them, the helicine arteries seem to be very abundantly and evidently present. But more anteriorly, where the cells assume a more honeycomb appearance, and the fibrous partitions are more band-like than cord like, and the arteries running on them are proportionally much smaller than the membranes surrounding them, the helicine appearance cannot be demonstrated. The most easy refutation of the presence of the so-called helicine arteries is found in the human species (in which, it will be remembered, Müller said they could be most easily demonstrated,) and next to it, in that portion of the corpus spongiosum urethræ of the horse and ass, which immediately surrounds the urethra.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *June* 19, 1838.

Abscess	2	Heart, diseased	2
Age and Debility	18	Hooping Cough	1
Apoplexy	5	Inflammation	4
Asthma	6	Bowels & Stomach	5
Childbirth	1	Brain	4
Consumption	29	Lungs and Pleura	4
Constipation of the Bowels	2	Influenza	1
Convulsions	11	Liver, diseased	2
Croup	2	Mortification	2
Dentition or Teething	2	Paralysis	1
Dropsy	8	Small-pox	16
Dropsy in the Brain	3	Sore Throat and Quinsey	1
Dropsy in the Chest	2	Spasms	1
Erysipelas	1	Tumor	1
Fever	12	Unknown Causes	256
Fever, Scarlet	1		
Fever, Typhus	7	Casualties	7
Gout	1		

Increase of Burials, as compared with the preceding week . . . 160

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

June.	THERMOMETER.		BAROMETER.	
Thursday . 14	from 48	to 63	29·64	to 29·68
Friday. . . 15	52	66	29·70	29·68
Saturday . 16	50	72	29 71	29·76
Sunday . . 17	58	72	29·80	29·80
Monday . . 18	57	71	29·62	29·55
Tuesday . 19	51	69	29·72	29 80
Wednesday 20	52	63	29·78	29·64

Prevailing wind, S.W.

Generally cloudy, except the 17th and 19th, with frequent showers of rain. A very heavy storm of thunder and lightning, accompanied with rain, passed from the south to the north on the morning of the 18th.

Rain fallen, ·65 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JUNE 30, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton-Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXIII.

Diseases of the Parenchyma of the Lung (concluded).—Pulmonary Consumption, Physical Signs (continued).—Signs of Softening and Evacuation of Tubercles.—Signs of Caverns, Pectoriloquy, &c.—Expectoration in the latter stages.—Signs of Intercurrent Diseases.—Processes of Cure of Phthisical Lesions.—Treatment of Pulmonary Consumption.—First Stage; Antiphlogistic Remedies.—Sorbefacient Alteratives, Iodine, &c.—Constitutional Treatment.—Diet and Regimen.—Treatment of Symptoms.—Latter Stages; External Counter-Irritation, &c.—Constitutional Treatment.—Treatment of Symptoms.

The conversion of the semi-transparent, grey, or dark consolidation of the lung, into crude yellow tubercle, is a point only deduced from anatomical examinations. We have no signs of this change during life. I have before observed, that there is sometimes an abatement of the symptoms of irritation, which may perhaps be associated with this change; there may be at the same time an increase in the expectoration, and the submucous and mucous rhonchi become more marked. But the change to yellow tubercle can hardly take place without some augmentation of the consolidation; the indurations increase in extent, and some yellow tubercle may perhaps be deposited or secreted in other parts. Hence there is a fuller development of the signs of an increased density in the lung; the partial dulness on percussion becomes more marked; the respiration becomes more obscure or more bronchial, and it may be accompanied by a permanent fine crepitation. The vocal resonance may have also increased in degree and extent, and altogether the signs have become more localized, and therefore less equivocal.

But it is the softening and evacuation of tuberculous matter that produce the most remarkable and cognizable changes in the physical signs; and these too often give to the expectoration something of the precision of a physical sign. The sputa before may have been sometimes opaque and muco-purulent, as in bronchitis; but they now become decidedly purulent, often sink in water, and, if narrowly examined, may sometimes be found to contain particles of a curdy or clotted matter, like cheese softened in water, which is tuberculous; they are not fœtid, like the similar concretions from the tonsils. There may also be little streaks or even clots of blood; but this is uncertain: there is generally, besides, more or less mucus, which gives tenacity to parts of the expectorated matter; but on close examination it may be seen that some sputa are opaque purulent clots, almost without mucus; and it is these that come directly from the cavities. Wherever these changes take place, generally under one of the clavicles, or above the spine of one of the scapulæ, there you may hear a clicking or bubbling sound, which is coarser, and gives you the idea of its being produced in a larger space than any of the common sounds of these parts. And here you will see that this sign is more conclusive, the finer and more completely vesicular is the natural structure of the lung in the part in which

it is best heard. In listening for this sound, you must seek for it in a long inspiration, or during a cough, which makes the air enter more fully and suddenly; and at first you may hear no more than one or two *clicks*, from the entry of single bubbles; but as the evacuation of the softened matter proceeds, and there is more room for the entrance of air, there is then a more continued gurgling or bubbling sound; and this will be coarse and distinct in proportion to the extent of the vomica, and its communication with the air-tubes. This gurgling or *cavernous rhonchus* will also vary somewhat, according to the quantity and liquidity of the contents of the cavity; becoming less crackling, and more whiffing as these diminish. When it is heard over an extended space, there are probably several cavities communicating with each other, and all containing more or less liquid. But I think it is not necessary to detail to you all the varieties that circumstances may produce; when you know the principles of the phenomena, you can foresee and understand their varieties.

The softening and evacuation of the vomica being complete, there is left an ulcerous cavity or cavern, which becomes the seat of further phenomena. Even before all the liquid is evacuated we sometimes hear, in the corresponding part of the chest, with the gurgling a hollow whiffing or blowing sound; and when the patient speaks, a sort of *snuffling* voice, interrupted, broken up by the gurgling. When the cavern is empty, these pass into *cavernous respiration* and *pectoriloquy*. Cavernous respiration is something like what you hear on applying the stethoscope to the front of the neck, in the course of the windpipe; but it is more circumscribed, and does not give you the same impression of a rush of air. You may get a better imitation of it by blowing into thimbles or shells of different sizes. It may present considerable variety, according to the size and shape of the cavity, and the freedom with which the air passes into and out of it from the bronchi. When of very large extent, the sound becomes amphoric, like that produced by blowing into an empty phial, and precisely on the same principle. All these phenomena are better obtained with quick forcible respiration, or slight coughing, which increases the force and velocity of the passing air, and exaggerates all the sounds.

Pectoriloquy is another very striking sign of a cavity in the lungs. Its value was perhaps overrated by Laennec; but I think it has been neither appreciated nor understood by subsequent writers. I formerly explained to you that the voice, although formed in the larynx, vibrates in full strength through the windpipe and its branches, until it becomes broken up and muffled in the smaller tubes and porous tissue of the lung. But if a cavity be formed in this parenchyma, communicating freely with the tubes in which the voice is strong, it will form a part of those tubes, and the vibrations will be continued in *system* from them to it; and there may thus be heard near the surface of the lung a voice from the chest like that heard over the trachea; its distinctness and intensity, however, being more or less perfect according as the cavity is adapted to receive the vocal resonance from the tubes, and to transmit it to the walls of the chest. Laennec made an artificial distinction between the degrees of pectoriloquy, according to whether the voice does or does not give the impression of passing up the stethoscope with the stopper to the ear. In the *perfect* kind, the words are so distinct that it seems as if the patient had his mouth to the tube: where this impression is not produced, the pectoriloquy is *imperfect*. But this is only a difference of degree, the sound most perfectly transmitted *engrossing* the air in the tube with its vibrations; whereas a less perfect transmission affects it more partially. I consider the *character of the sound*, and its *circumscribed position*, a better distinction. The sound is not a mere vocal resonance, like the bronchophony from consolidation, which is often as loud or louder, and may pass up the tube; but it is an articulate although indistinct speaking, and sometimes accompanies a loud whisper as well as vocal utterance. There is in it another feature which is characteristic, and distinguishes it from bronchophony; it is accompanied or followed either by whiffs of cavernous respiration, which give the pectoriloquy a *snuffling* character, or by a hollow or fistular resonance, like that which you can produce on speaking at the mouth the tube of a pan-pipe, the pipe of a large key, a shell, or any such hollow body. This accompaniment is sometimes observed when the pectoriloquy, or transmission of the articulate voice, is very imperfect; but I have found it to be more distinctive of a cavity than the loudest vocal sound without it. I suppose that it depends on the same physical cause as the phenomena of the hollow bodies to which I have compared it; the cavity in the lungs being in the same relation to the bronchial voice as they are to the oral voice. When the cavity is very large the accompaniment is more amphoric or bottle-like; and if the communication with the bronchi be at the same time narrow, the voice may scarcely be transmitted to it, but only excite in it a tinkling echo, a metallic tinkling, as in pneumo-thorax.

All these hollow, fistular, or tinkling characters, may be also perceived in the breathing and cough, especially in the latter, but not in a proportionate degree; and I have known them to be scarcely perceptible but with the voice. These differences must depend on the relations of the cavity to the air-tubes communicating with it; if this open into them so as to catch the current of air passing in them, its interior will be thrown into vibration; otherwise the air within it may only receive the stronger and more pervading vibrations of the voice. So also, if there be much consolidation about and beyond the cavity, there may be very little passage of air in the tubes, and therefore but little cavernous breathing.

The circumscription of pectoriloquy is another of its peculiar characters, and by this it may generally be distinguished from the loud bronchophony of condensed lungs, which is diffused over some extent of surface. To observe these differences, it is quite necessary to limit the point of examination, by using the stopper in the stethoscope. By this means you can trace the precise boundaries of the pectoriloquy of a cavity; but when you try to find where the resonance of bronchophony ceases, you will fail to find any exact limits; it gradually loses force as the tubes become smaller, or the superjacent lung more porous. Pectoriloquy is most characteristic when it forms a *little island of voice* under a clavicle, and little or no sound is transmitted nearer the sternum. The pectoriloquous bronchophony of a lung consolidated by inflammation, or compressed against the walls of the chest by a liquid effusion, never has this isolated character, but is generally louder in proportion to the size of the tubes involved in the condensation. It is, however, true that sometimes, in phthisis, the pectoriloquy is not thus circumscribed; for besides the cavities, there may be general consolidation of the lung, and consequently free transmission of the voice over an extent of surface. Even in this case, a practised auscultator can distinguish the peculiar phenomena of cavities, in the snuffling, blowing, or tinkling and more articulate voice that certain spots present, or a coarser gurgling if liquid be present. You will readily perceive that all these phenomena are liable to be interrupted or modified by the accumulation of the matter secreted by the cavities and adjoining tubes. So also as, in time, the disease advances, the excavations become extended, and the gurgling first, and pectoriloquy afterwards, are heard in new spots.

You might at first suppose that the excavation of tubercles and the formation of cavities containing air, would remove the dulness on percussion, that accompanies the two first stages of phthisis: but this is seldom the case; for although there is more air than there was, yet there is much solid deposit about the walls of the cavities, and the irregular density and flaccidity of the parts, as well as the defect of air in the peripheral structure of the lung, still tend to check and muffle the vibrations of the walls of the chest, and prevent them from yielding a clear sound. Even where the cavity is so large as to be the seat of a tinkling echo, or metallic tinkling, the resonance on percussion is irregular and imperfect; and thus may this case of metallic tinkling be distinguished from that of pneumothorax, in which some part of the chest must have an unnaturally clear sound. Sometimes the sound is clearer, in consequence of a general dilatation of the superficial cells; and as this is commonly of the flaccid kind, it may be accompanied by a sharp puerile kind of respiratory murmur: both these circumstances may disguise the phthisical signs, but only partially, for there will still be some decided irregularities in the sound on percussion, and enough of the signs of the subjacent caverns to declare the case to the wary observer. Now and then we find a case in which there is a hollow or bottle-like sound, on percussion over a cavity; this is when it is pretty large, and the intervening tissue is pretty uniformly condensed. More commonly there is an opposite condition; the walls of the cavity are loose and yielding, and, if it be large, percussion may sometimes cause a motion of its contents and a gurgling or tinkling expulsion of air from it, which produces a muffled metallic sound, like that of money in the nearly closed hands, or more nearly like the imitation of that noise with which we amuse children in striking the hands, hollow and closed, upon the knee. Laennec compared the sound to that emitted by a cracked jar when it is struck.

With the irregular and deficient sound on percussion, generally most obvious under the clavicles or in other parts of the upper regions of the chest, there is very commonly associated a collapse or sinking in of the walls of the chest, forming under the clavicles a remarkable hollow, generally more conspicuous on one side than on the other. There is very commonly, also, some defect and irregularity in the movements of the chest; the upper part of one side being but little raised, and the lower parts altogether exhibiting the most motion. But there is seldom that complete fixing of one side that we see in chronic pleurisy, in which case, too, the upper part is generally more mobile than the lower.

We might class with the physical signs the characters of the sputa in the third stage of consumption, if they came only from the cavities which are peculiar to it. The expectoration of distinct portions of tubercle, or of pulmonary tissue, which have been seen in a few instances, constitutes a physical sign of the clearest character: they must come from cavities. If patients could save all their expectoration, and if this were inspected daily, this unequivocal sign might be more frequently met with. But the inflamed air-tubes are in great measure the source of the expectorated matter, which therefore presents much of the same aspect as in chronic bronchitis. The large size, and almost perfectly purulent character of the masses sometimes expectorated, which are like irregular balls of flock or wool, and of a yellowish or greenish colour, sinking and breaking down in water, go far to prove the existence of cavities in the lungs. A dirty yellowish brown or greenish matter, occasionally streaked or fringed with blood, flattening like a piece of money when separate, and in masses forming a smooth sluggish purilage, are more characteristic of phthisis, and commonly occur in the most advanced stage. The general pulmonary congestion which frequently precedes death, is often announced by the darker reddish or green hue of the purulent sputa. Profuse hæmoptysis does not happen often in the advanced stage of consumption; for the vessels soon become plugged with fibrin and obliterated in the diseased portions of lung, and the mass of blood is reduced to the capacity of those that remain free.

I think I have said enough of the most distinguishing signs of consumption to make it unnecessary to pursue them further into the details of differential diagnosis, or the distinctions between particular diseases. There is only one kind of lesion which, even in its physical signs, may be mistaken for tuberculous excavations; I mean dilated bronchi. These, I have told you, may be the seat of a coarse gurgling rhonchus, cavernous breathing, and pectoriloquy; and the accompanying chronic bronchitis often furnishes also a purulent expectoration. The situation, greater extent, and more stationary character of these lesions, may serve to distinguish them: they most commonly occupy the scapular, mammary, and lateral regions, and not the infra-clavian; they usually extend over a considerable space, and they do not tend to spread as tubercular cavities do. Again, if they arise from disease in the bronchi only, they do not impair the sound on percussion so much as phthisis does: and if they originate in pleuro-pneumonia, the dulness is much more complete, and is confined to one side, and accompanied by a more marked contraction than that which occurs in phthisis. But you should take the general symptoms also into account. There is seldom, with dilated bronchi, the degree of emaciation that occurs in phthisis; and when they arise from condensation of the lung, there is very often œdema and general dropsy, which are not common in simple phthisis.

It is not at all uncommon for phthisis to become complicated with other diseases of the chest; especially bronchitis, pneumonia, and pleurisy, and the attacks of these additional lesions sometimes prove fatal even when the phthisical changes are not extensive. Thus it is very common to find general pneumonia attacking a lung in which there are miliary tubercles, which must have existed prior to the inflammation, and would probably not have run their course for many months. This complication greatly increases the danger of pneumonia, which, unless it be stopped in its very onset, commonly proves fatal. In some instances, especially in the young, we see a reason of its intractability in the tuberculous character of the hepatization, which has the greyish or boiled-liver aspect, with considerable softening, instead of the redder deposit of common hepatization; on the pleura there is sometimes seen, at the same time, the friable opaque lymph which borders so closely on real tuberculous matter: and in other instances, where the disease has not advanced far enough to present these appearances, we can still understand that there may be in the deposit enough of that defect of vitality which renders tuberculous matter so difficult of absorption. The supervention of the signs of an extensive pneumonia, crepitation, with increasing dulness on percussion, affecting the posterior lobes of one or both lungs, together with the increased febrile disturbance, increased heat, and the rusty tinge in the sputa, must be looked on as indicative of extreme danger to patients with any extent of phthisical disease; for if it do not itself prove fatal, as it very commonly does, the inflammatory attack will not fail to hasten and increase the phthisical disease. It is otherwise with the circumscribed pneumonia that not unfrequently attacks portions of the lung in the progress of tuberculous disease; these come on without much disturbance, and subside without causing much mischief, being probably the result of mere local obstruction or irritation. The same remark applies to the slight pleuritic attacks so common in phthisis, the effects of which are seen in the adhesions of the pleura so generally found in phthisical subjects. I have often heard

a sound of friction in different parts of the chest, continuing for some days; then ceasing, and the chest when examined long after, has exhibited adhesions at these points. I believe that the inequalities occasioned by the deposits in the lung are sufficient to cause a local inflammation in the pleura, by their continual friction against it. Liquid effusions are less common, and arise from a more extensive cause, such as the bursting of a vomica into the pleura. If the vomica also communicate with the bronchi, there will be pneumothorax as well as liquid effusion. In either case the pleurisy is a serious and untractable addition to the consumptive disease, and may prove fatal in a few hours. Pulmonary hæmorrhage is another grave accident, occurring especially in the early stages of phthisis. It may prove fatal by loss of blood, or by suffocation; or the effusion of blood may break up the tissue of the lung to a great extent, and the patient may sink from the sloughy suppuration that ensues. Or, after the hæmorrhage has ceased, inflammation may arise in and about the hæmorrhagic consolidation, and involve the lung in a destructive suppuration, which may be more or less of a tuberculous character. On all these accounts it is of the greatest consequence to guard against or to arrest pulmonary hæmorrhage, and when it has taken place, to use means to promote the absorption of blood from the tissue, and to prevent the rising of inflammation, which under these circumstances may prove so pernicious.

Destructive as phthisical lesions are, both by their own tendencies, and by the manner in which they affect the system, it is nevertheless now well ascertained that they do occasionally admit of cure. I think we are warranted in supposing that this may take place at any stage; but the mode of cure which has been most completely traced, is that by the expectoration of the tuberculous matter, and the lining of the cavity with a complete false membrane, which commonly is of a fibrous or fibro-cartilaginous texture, tending to contract and ultimately to obliterate the cavity, but is sometimes thin and more like mucous membrane, without any obvious tendency to such a contraction. Here are drawings representing different cavities lined with both kinds of false membrane; they are all pretty smooth in their interior; some contained friable caseous matter, the debris of tubercles; but the lining was most smooth and uniform in this cavity which you see here, and it was so thin that you can discern the pulmonary structure through it, and the bronchial tubes which open into it at the two points where the bits of whalebone are inserted. Although it is rare to meet with such complete specimens of lined cavities as these drawings represent, it is not uncommon to find in the lungs of those who have long laboured under symptoms of pulmonary consumption, some of the cavities with a lining more or less perfect, and at the apex of the lung especially there may be now and then found a cavity contracted almost to obliteration; and sometimes a mere cicatrix perhaps inclosing a little friable caseous matter. All these instances evince a natural effort towards the healing of ulcerous cavities in the lungs; and where the disease is very limited in extent, and fresh tuberculous deposits do not take place in other parts, this healing of the cavities may amount to a cure of the consumption. The symptoms which may lead us to hope for such an unusual event, are a gradual diminution of the cough and purulent expectoration, a cessation of the fever, and quickness of pulse, and a decided improvement in flesh and strength. The signs that countenance this expectation are, the diminution of the pectoriloquy and cavernous respiration, and the restoration of some vesicular respiration, and a better sound on percussion in the part, whilst in the rest of the lungs the sounds are natural.

I cannot bring morbid anatomy to prove the possibility of a cure in the earlier stages of phthisis; but I have the history of several cases in which the signs render it extremely probable that some of the depositions which form the first stage of consumption of the lungs, had been removed by absorption. I have not time to give you the detail of these cases; but their general features are these: there have been cough, some shortness of breath, in two instances hæmoptysis, in some pain under the clavicle, quickened pulse, some evening fever; and these symptoms, instead of taking the course of an ordinary cold, have remained permanent for two months and upwards, with some loss of flesh and strength. On physical examination there have been found some dulness on percussion, on or under one of the clavicles or scapular ridges, with bronchial respiration, or a slight permanent rhonchus, and increased resonance of the voice. Under treatment and favourable circumstances of air and climate, these symptoms and signs have been gradually removed, in some cases entirely, in others partially, but to such an extent that the patient's health has been considered in great degree restored. It may perhaps be said that we have no certain proof that these cases were phthisical, or if they were so, that the phthisical lesions were entirely removed. I admit this, but maintain that the existence of phthisis was as much proved as it ever can be in

its early stage,—that precisely similar cases when neglected commonly run a consumptive carcer,—and we are not aware that such a combination of signs and symptoms can be produced but by lesions which essentially have a phthisical tendency.

Time will not permit me to dwell long on the treatment of pulmonary consumption, for if I were to enter into dctails they might well occupy two or threc lectures. I must refer you to the recent work of Sir James Clark, which contains much important matter, espccially on this subject; and I shall now give an outline of the best plan of treatment, in accordance with the views to which the study of the disease has led us, with a few remarks suggested by my own experience on the effect of particular classes of remedial agents in the prevention and treatment of pulmonary consumption.

We have been led to conclude that the most important elements in the production of phthisical lesions, are a state of constitutional weakness, and a local vascular irritation or congestion: these elements predominate in various proportions in different cases, and will require a corresponding variation in the treatment; but in almost every case both the constitutional and the local causes must be duly investigated and treated, or success will be only a matter of the most incalculable and irrational chance.

In the last lecture I divided cases of consumption into three classes; 1. Those originating from local disease; 2. Those originating from constitutional disorder, or hereditary predisposition, without any previous local disease; and 3. Those arising from local diseasc in a subject of hereditary or acquired scrofulous or phthisical constitution. I had occasion to observe, with rcgard to the first class, that the local disease might act, not only by developing in the lungs lesions which tend to run a phthisical course, but also by injuring the functions generally, and thus a constitutional cause becomes added to the local one. Therefore I believe in no case should we exclude constitutional treatment from a prominent place in the management of consumptive patients; and as far as a plan including many variations can be described by words, that best suited for consumptive cases generally may be designated as consisting in constitutional tonics, and local counter-irritants or evacuants. It is, however, where local disease has been the chief cause of the mischief, that we have the best chance of curing consumption; and the more so, in proportion as the local lesions are limited, and the constitutional powers little impaired.

The symptoms of the first stage of phthisis, that of the indurations, are those especially of vascular irritation, and obstruction; and this is the period in which antiphlogistic and counter-irritant remedies avail most. Small general bleedings in the plethoric, and in those in whom there are symptoms of considerable congestion, or pulmonary hæmorrhage, or repeated moderate leeching below the clavicles for the less robust, may often be practised with considerable advantage in the early stage of consumption; especially when an increase of pain, cough, a bloody tinge in the sputa, dulness on percussion, and irregular respiration under the clavicles, indicate a congested state of the lung about the suspected indurations. In cases of greater debility, or where there appears to be a defect of blood in the system, blisters or counter-irritants are more suitable. I generally prefer a saturated solution of tartar emetic, rubbed in below the clavicles twice a day, until the skin begins to be elevated in little papulæ. If it be continued longer, the pimples may become pustules, and go on to suppuration, which is not desirable in this stage. When the papular eruption has died away, there being only a few pimples that have come to a head, the application may be renewed according as the symptoms may require. Issues and setons commonly produce too much irritation of the system to be useful at this stage. A more moderate and general counter-irritation may be produced by sponging or rubbing the whole chest once or twice a day with a stimulating liniment, such as the volatile liniment, or one composed of strong vinegar, with a little almond oil and some pungent volatile oil, such as that of cajeput or lemon.

The efficacy of internal sedative medicines is more doubtful, except so far as they tend to diminish the irritation of the cough and pain. Thus digitalis, colchicum, and hydrocyanic acid, may now and then subdue temporary vascular excitement, and thus quiet symptoms; but I doubt the utility of continuing them long with the view to permanently reduce the pulse: they may thus do more damage to the constitution than they give relief to the irritations. The narcotic remedies, such as opium, conium, hyoscyamus, and belladonna, are also occasionally useful in allaying cough and pain, especially when these symptoms are associated with high nervous sensibility or a tendency to spasm; but they have no permanent influence on phthisical lesions, nor on the congested vessels about them; and unless given judiciously, they may disorder the gastric and alvine functions, and thus injure the state of the constitution.

But are there no remedies which will promote the removal of the indurations

themselves? Of this we can only speak doubtingly; but if we may be guided by analogy, we might almost be led to hope that certain medicines may influence the processes of absorption and renewal of the matter of textures, so as to facilitate the removal of morbid deposits. Thus we see tumors of various kinds, enlarged glands, and depositions in the joints, occasionally reduced under the influence of mercury, of alkalies, or of iodine; and although there are many forms of deposit on which these remedies exercise no influence, and others in which their power is very equivocal, yet the limits of their action are not so well defined as to authorize us to say that all the kinds of induration which precede tuberculous deposit are quite beyond their reach. The influence of these remedies in promoting the absorption of the simpler products of acute inflammation is scarcely to be questioned; and arising, as the lesions of phthisis occasionally do, from acute inflammation, and presenting various gradations which remove them only step by step from its products, it would be unreasonable to assert, without sufficient evidence to prove it, that these agents can never exert some such operation on the deposits that form the first stage of phthisical lesions. Evidence on the other side is also wanted; and all that I can say on this point is, that in the cases of apparently arrested consumption to which I have before alluded, some of these remedies have formed part of the treatment. I cannot say much of *mercury* in such cases, except as an occasional aperient: it may, perhaps, be useful where the pulmonary lesion originates in acute inflammation; but whatever its influence on this may be, its operation has generally appeared to me to be so injurious to the constitution, that I have been deterred from employing it to affect the system in more than two or three instances: in these the disease was certainly not retarded by its use.

Nor can I speak from experience on the effect of frequent *antimonial emetics*, which were formerly much used in the early stage of phthisis, and have been lately again recommended on theoretical grounds. Before I can advise the use of so violent and disagreeable a remedy, I must have better testimony in its favour than that of old writers, who, we know, did not possess the means of distinguishing phthisis from chronic bronchitis.

In *iodine*, combined with alkalies, I rest more hope; and I have been in the habit of giving this medicine in incipient cases of consumption for the last ten years. The form that generally agrees best is the hydriodate of potash, in doses of two or three grains, three times a day, with twenty or thirty minims of liquor potassæ, in decoction of sarsaparilla, infusion of calumbo, or distilled water, according to the state of the system, adding a little tincture of henbane, ipecacuanha wine, or any other medicine that the predominant symptoms may indicate, and always drinking or eating something farinaceous after it. Where there is a tendency to feverish irritation it may be given in a nitre draught; and where there is more vascular debility it may be combined with mild tonic infusions. In chlorotic and in exsanguine scrofulous subjects, the iodide of iron is a suitable form; and where this is borne, not causing headache and fever, or increase of cough, it rarely fails to improve the state of the general health; but it should always be combined with occasional local depletion or external counter-irritation of the chest. When iodine agrees, (and by varying its form and combinations it may generally be made to agree,) it increases all the secretions, and seems to give increased activity to the whole capillary system. In cases of gastric irritation, with pain in the stomach, or florid tipped tongue and thirst, it should be suspended, and a dose or two of hydrargyrum cum cretâ administered, followed by a few small daily doses of castor oil, or some other mild aperient; and after a few days the hydriodate of potash may be resumed, guarded by the frequent use of a farinaceous diluent.

But even in the cases in which the phthisical lesions are most limited, we are never to forget that it is not these lesions alone that we hope to remove. Their very presence in the system, or the operation of the cause that produces them, may lead to the formation of more; and in our treatment we should ever endeavour to remove those low degrees of vascular irritation, or that unhealthy condition of the nutrient matter of the blood, which, singly or combined, occasion the deposition of tuberculous indurations. We have already alluded to the local means of preventing vascular irritation and congestion of the lungs; we must now advert to the constitutional treatment. Here we are to seek for all those circumstances and agents that may best promote the due action and balance of all the functions. The purest air and the most suitable climate for regular and ample exercise in it—the most nutritious food that the digestive organs can easily assimilate, and that the vascular system can bear without excitement—such remedial agents as give at once tone to the system and maintain the free action of all the secreting organs, together with friction, exercise, and proper clothing, to promote the activity of the superficial circulation—these are the means which are rationally indicated to fulfil the object of improve-

ment of the general health. But how vague is this statement! how little can it guide us in particular cases! And so it must be: the means must be varied and adapted to the diversified forms of particular subjects, and it is in the study of individual cases, and in the power of discovering their conditions and of adapting means to them, that the ability of the practitioner is displayed. I can only give you the more general results of my observation in the employment of these various hygeian agents.

Pure country air is almost indispensable to give any chance to the consumptive. If the disease be limited and chronic, and his circumstances prevent him from giving up his employment in town, he should at least sleep in the country, and take every opportunity of longer absence. I have no hesitation in saying that I have known the lives of several consumptive patients prolonged by adopting this practice. But the country must be dry, and not too much exposed to the east and north, or there may be only a change of evils, from cachexia to inflammation. There is no air which is so truly an antidote to the poisonous effects of a town residence, as that of a dry sea coast; and the more open this is for the summer, and the milder and more sheltered in the winter, the better for the consumptive. The benefit that patients often quickly experience from the change is most striking, even in the more advanced stages of consumption. To profit fully by the influence of pure air, the patient should be as much out of doors as the weather will permit, and use as much gentle exercise, both by walking and riding on horseback, as the state of the strength will allow, without inducing much fatigue.

The diet in the early stage of consumption should generally be of a very mild unstimulating character; consisting chiefly of milky and farinaceous food. Sometimes white fish and chicken may be allowed; and a state of vascular debility, or previous habits in some cases make the plainer kinds of meat necessary; but this is especially the period of irritation and congestion, and more mischief is likely to result from repletion than from moderation. For the same reason, fermented liquors are seldom admissible at this period.

Of remedial measures, those already named in relation to the local lesions and particular symptoms, may be combined or modified so as to act favourably on the functions at large. This is especially the case with iodine. Occasional mercurial or saline aperients will be generally needed to prevent internal congestions, and to ensure the sufficient action of the abdominal viscera; but they should not be carried to excess, and their operation should be aided by due attention to the diet; so, also, the functions of the kidneys and the skin may, in particular cases, be ameliorated by aid of medicines; but the more that can be done by clothing, diet, and regimen in general, the better. Clothing, especially, should be most carefully attended to; we have in it the means of affecting, often powerfully, the whole vascular system; and if so regulated as to maintain a permanently warm and supple, but not relaxed state of the whole surface and extremities, it would prevent many of those fresh colds and exacerbations that are the greatest bane of phthisical invalids. In case of these aggravations, which commonly consist in an increase of bronchitis, but sometimes of pneumonia or pleurisy, the remedies for these diseases must be cautiously resorted to; always limited by the reflection that we are treating a subject already debilitated with disease, and in whom a permanent source of irritation will prevent that complete relief that antiphlogistic measures may give in simple inflammations. In case of hæmoptysis, much care is required to remove the congestion or vascular fulness, which occasioned it, before attempts be made to arrest it by styptics; or the congestions may pass into inflammations, which, occurring in a lung tuberculated, and consolidated by hæmorrhage, is particularly destructive. Moderate repeated bleedings from the arm, or by cupping, and the use of tartar emetic in small doses, not sufficient to cause vomiting, joined with digitalis and nitre, a little morphia in case of vascular irritation, are the measures which I have found of most avail. If, in spite of these, the hæmoptysis continue to any amount, the superacetate of lead, in doses of two or three grains, with half a grain or less of the aqueous extract of opium, should be given every two hours, or as often as the urgency of the case may require.

In the second and third stage of phthisis, the chief modifications of the treatment are indicated by the greater degree of constitutional debility, whilst the symptoms of irritation may have diminished, or at least have not increased in proportion. Here depletions are less needed and worse borne; and a somewhat tonic plan of treatment and more generous diet may sometimes be ventured on with advantage. Still counter-irritation generally proves useful; and now that accompanied by purulent or sero-purulent discharge, will commonly produce the greatest benefit. In fact, the same abatement of irritation that I have before described as accompanying free purulent expectoration, will in some degree follow from this ex-

ternal suppuration, without the wasting and harassing effect of such a discharge from the lungs. With this external outlet, as a sort of safety-valve, strengthening medicine, and nourishment, may be borne; and there is less risk in restraining any excessive secretion which may take place from the lungs, the bowels, or the skin. Much attention is necessary to keep up the discharge, whether it be by the formation of successive crops of pustules, by tartar emetic solution, or some similar suppurating liniment, or by a seton or issue. If it be suddenly checked, there will, in all probability, be an increase of pulmonary irritation, probably attended by the deposition of more tubercuIous matter. In slighter cases, or where the weakness or irritability forbids these measures, occasional blisters, or the frequent use of milder liniments, containing tartar emetic and hydriodate of potass, or acetic acid and oil of turpentine, are often productive of some benefit.

In the tuberculous and ulcerative stages of phthisis, the constitutional powers especially need support; and it is then, more particularly even than in the first stage, that the general measures are required. But unless the disease be limited in extent, there is, for the same reason, less hope of their success. The disease has existed longer, and passed into a stage in which it is more likely to have tainted the system. The preparations of iodine, and other alteratives, should be used more freely, and the general health supported by all the medicinal and hygeian circumstances that can be brought to bear on it. There are vomicæ to be evacuated, and the object is to get them emptied, and to promote the healing of the fistulous cavities which they leave. Can this be aided by local applications, by inhalations of chlorine, iodine, or other vapours? Some who have used these means speak highly of them; too highly to gain our confidence. I have seen them used, and do much harm; but I do not deny that, under very judicious management, they may sometimes contribute to induce a healthy healing action in the interior of ulcerated lungs. But we must look more to an improved state of the constitution for such a healthy action, and for what is of more immediate moment, a cessation of that disposition to deposit more tuberculous matter in other parts, which too commonly prevails during the softening and evacuation of the tubercles.

In very many cases, alas! no means will stay the progress of consumptive disease; and the utmost that we can do is to give temporary relief to troublesome symptoms; to the cough, by various forms of opiates; to pains in the side, by a blister or a mustard poultice; to the dyspnœa, by æther and ammonia, or by the tincture of lobelia; to the hectic heats, by sponging with vinegar; to the sweats, and to excessive expectoration, by acid mixtures; to the diarrhœa, by astringents, preceded by a mercurial aperient; and so forth. But in not a very small number of cases we may considerably prolong life by watchfulness and care. Consumption may run its course in a few weeks; but it may exist in a limited and chronic form for many years; and it is these long cases that may reward us for our attention and judicious treatment, if not by permanent recovery, at least by temporary restoration of a moderate share of health and strength compatible with the enjoyment of life, and the fulfilment of important duties in society.

LECTURES ON BLOOD-LETTING,

Delivered from time to time,
At the General Dispensary, Aldersgate Street,

By Henry Clutterbuck, M.D.

Formerly one of the Physicians to that Institution.

Lecture VIII.

On the Use of Blood-letting as a Remedy for Inflammation in general.

Inflammation and the employment of blood-letting are often so strongly associated in the minds of practitioners, that the presence of the one is apt immediately to suggest the idea of a necessity for the other. From the same cause it is, that in diseases the nature of which is obscure and questionable, it has been sometimes argued that the disease was not inflammation, merely because it did not yield to blood-letting—as if this were a universal remedy for inflammation, and applicable at all times and on all occasions. Instead of this being the case, however, I must repeat to you that there are a great number of inflammations which blood-letting will not cure; many also in which, though useful, it may be safely and properly dispensed with; and not a few in which it proves injurious rather than beneficial. In no case, indeed, is blood-letting a certain remedy for inflammation; nor, where it really proves successful, does it appear to act directly upon the disease, but rather by a kind of counter-impression on the system, by which the disease is influenced in a secondary way, as before observed. Were we to suppose it a certain remedy for inflammation in any case, and possessed of directly-curative powers, we might be often led to employ it to an injurious extent, as is now sometimes done in apoplectic seizures, where blood is often drawn to an enormous amount, as if it

were only required to carry the evacuation far enough, in order to ensure success.

Different circumstances appear to influence the result, in the application of blood-letting as a remedy for inflammation, and which therefore require to be considered. The following are the chief:—

1. The degree of the disease.
2. The stage of it.
3. The part in which it is seated; and
4. The nature of the inflammation itself.

1. *Cæteris paribus*, the more violent the disease, the greater would seem to be the call for blood-letting, as the most likely means of averting present danger. Acute inflammation, doubtless, requires greater promptitude and activity in the use of remedies, than when the disease is of a milder character, or of a chronic form. But, though it be apparently, or even really mild, the disease, when it happens to be seated in organs of importance to life, is not to be lightly treated on that account; for its tendency and power to injure the structure of the part still exist, and must not be disregarded. In parts, too, which naturally possess but little sensibility, such as the lungs, it is not always easy to judge of the real violence of the disease present. If the inflammation be of limited extent, it may be really violent, so as quickly to disorganize the part, without giving much pain, and without materially impeding the function of respiration. The same reasoning applies to the brain, the texture of which is often found partially destroyed by inflammation, the existence of which was not even suspected during life.

2. The *stage* of the disease is of still greater moment in determining both the employment of blood-letting, and the extent to which it is proper to carry it. Generally speaking, the earlier the remedy is resorted to, the safer and more effectual it will be found. A single bleeding, employed on the first or second day of an inflammation, will often put an immediate stop to it; whereas, if delayed to a later period, the remedy is not only less certain in its effect, but often requires to be repeated, possibly for many times, before the object can be accomplished. This is, perhaps, to be accounted for by the change that has taken place in the structure and living powers of the part, a change that is always progressive, and hourly increasing as the disease proceeds.

Another circumstance regarding the stage of inflammation, merits notice: namely, that the quantity of blood required to be drawn in order to afford relief, lessens as the disease advances; so that where a pound or more of blood may be safely and properly drawn in the first days of any inflammation, the quantity of a few ounces comes at length to be sufficient, when the same disease has reached the second or third week. And the same is the case where, either from error or neglect, no blood has been drawn at the commencement. This is to be ascribed, probably, to the increasing weakness induced by the disease, owing, in part, to the interruption or disorder of the natural functions, and partly to the excess of action that is taking place in those cases by which the vital power is exhausted.

3. The organ or part affected in inflammation, has also an influence on the use of blood-letting, and that so considerable as to demand particular notice. Every part seems to possess peculiarities in this respect. *Thoracic* inflammation, for example, appears to be more under the influence of blood-letting than any other. It seldom fails to cure here, if promptly and actively administered; and further, it may be safely and usefully resorted to, at almost any period of the disease; due regard, of course, being had to quantity and the other circumstances before pointed out. Whereas, in the case of *ligamentous* inflammation (acute rheumatism more especially), blood-letting, though often urgently called for by all the signs usually considered as requiring the use of this remedy, fails, nevertheless, in numerous instances, to produce the desired effect. Other organs appear to hold an intermediate station in this respect, between the two extremes. Thus erysipelas, or inflammation on the skin, is less benefited by blood-letting than the active state of the disease would lead one to expect. So also, in the case of what is called idiopathic fever, where the brain is the suffering organ, blood-letting is very generally safe, and often decidedly curative, provided it be employed at the outset of the disease; as within a day or two of the attack. But the same remedy becomes less and less effectual as the disease advances, and at length is decidedly prejudicial.

4th. The *nature* of the inflammation present has an important influence on the use of blood-letting; as we see in the case of those termed *specific*, such as the variolous, morbillous, scrofulous, venereal, and others; all of which, though possessing the ordinary characters of inflammation, stand on peculiar ground in respect to treatment. Blood-letting has no power of curing these, though it may be occasionally useful as a palliative measure.

So that, upon the whole, you see that blood-letting is not to be considered in the light of a general remedy for inflammation, nor to be used indiscriminately; but requires that attention should be paid to a variety of circumstances, without which its employment may be both useless and dangerous.

In the application of blood-letting as a

remedy for inflammation, it is by no means necessary, under ordinary circumstances, to use it in a manner, or to such an extent, as to produce any immediate or striking effect, either on the general system or on the disease itself. Upon the abstraction of a moderate quantity of blood, as, for example, ten or twelve ounces, (which experience has proved to be fully adequate to the purpose on numerous occasions,) the disease often begins to decline, though gradually and imperceptibly; and at length goes off altogether, without the production of any sensible disorder, or inconvenience of any kind. It is only in cases of great urgency, therefore, and where parts of immediate importance to life are concerned, that it is either necessary or proper to induce syncope, or even an approach to it.

Of the repeated use of Blood-letting in the treatment of inflammation.

Many cases of inflammation will yield to a single bleeding. But in order to this taking place, it is generally requisite to bleed very early in the disease, and with as much freedom as the circumstances warrant. If the operation be delayed but for a few days, the probability is that a repetition of the operation will be necessary. Some inflammations are so violent, and so indisposed to yield, that a repetition of blood-letting is indispensable, and that for several times. In these cases, when the inflammation is seated in an organ of great importance to life, the intervals should be short, not exceeding six or eight hours. In less urgent cases, a day or more may often be allowed to intervene. The full effect of any bleeding may not take place for many hours, and therefore it is not requisite (unless in very urgent cases) to repeat the operation the same day.

Many chronic inflammations of the lungs, heart, brain, &c., though of long standing, will yield to small and repeated bleedings. In such cases, the quantity of blood drawn at one time should seldom exceed five or six ounces, with intervals of a week or more; and if the patient be inclined and allowed to take ordinary food, according to the appetite, the general strength will be preserved; so that the treatment may thus be borne for a great length of time.

The appearance of the blood drawn has often been relied upon as shewing the propriety of further bleeding, or the contrary. But this, as a general rule, is very equivocal. The blood first drawn, provided it be taken very early in the disease, will often exhibit no inflammatory character, even where the inflammation is violent, while the subsequent bleedings will be strongly marked in this respect. And, indeed, in all such cases we are to be guided rather by symptoms, and the general condition of the patient, than merely by the appearance of the blood.

A buffy or sizy appearance of the blood is only a proof that the inflammation is going on; but it is not always a reason for further bleeding.

The blood will often present the appearance mentioned in the highest degree, where the strength is so much reduced as not to allow of further loss of blood. This is seen at the latest period of phthisis, and when life is almost extinct; on the other hand, a loose state of the coagulum, (the opposite of the former) and also a great excess of the serous portion, are neither of them always sufficient to prohibit blood-letting, as where the inflammation is proceeding with violence, and threatening danger to life. Such states of the blood, however, always render great reserve and caution necessary, in regard to the quantity of blood to be taken away.

There is a period, no doubt, when it becomes necessary to desist from further loss of blood on account of the weakness of the patient, although the disease may not have given way. It is not easy, however, to define this point with any exactness. Faintness is not of itself a sufficient guide, for this is often temporary, going off entirely after a short time, especially if the patient be put into the horizontal posture. The pulse upon the whole, perhaps, affords the best guide on these occasions. If the pulse continues small and feeble, it will hardly be justifiable to carry the bleeding farther; and we must resort of necessity to such other means as may present themselves.

I have only further to remark here, that it is by no means necessary, generally speaking, to continue to draw blood from day to day till the disease has entirely gone off; it is sufficient, in most cases, to have checked the progress of the disease, so as to put it on the declining side; for, provided no cause of aggravation be applied, it will then gradually and wholly subside. Should it, however, prove otherwise, the disease beginning again to advance, or even if it remain stationary, an immediate recurrence must be again had to the remedy.

Of Auxiliaries to, and Substitutes for, Blood-letting in the Treatment of Inflammation.

In regard to substitutes for blood-letting in general, in the treatment of inflammation, I wish to observe that there are few possessed of much importance, or which can be trusted to in dangerous forms of the disease. Still, the practice of blood-letting is always to be considered as objectionable in itself, and only to be justified on the

plea of necessity. This necessity, however, is of very frequent occurrence, because it is seldom that a substitute can be found that is at once sufficiently safe and effectual. The evils that may ensue from an intemperate or injudicious use of it, are not to be taken into account as furnishing any real objection to the practice; for these, we must presume, are not necessary consequences of it.

The substitutes for blood-letting may be almost all comprised under the heads of *regimen* and *counter-irritation;* and these are also the best auxiliaries. The former includes whatever regards food and clothing —air, exercise, and rest, both of body and mind—sleep and watching,—all, in short, which the ancients designated by the term non-naturals—a strange expression, considering that there is no one of the articles mentioned that is not consonant with our natural wants, and which is not, indeed, in constant and universal use. Attention to regimen, in the enlarged sense of the term, is no doubt of considerable importance, whether as auxiliary to blood-letting in the treatment of inflammation, or as a substitute for it on certain occasions. Without such attention the good effects of blood letting, or other active remedies, may be altogether frustrated. The article of food, however, is by no means the most important part of what comes under the general name of regimen. In most cases, indeed, the laying down any very positive rules on the subject is superfluous; the appetite, when not vitiated by habitual indulgence, or stimulated by variety, being a sufficient guide. In acute diseases, in which the taking food might be injurious, there is rarely much desire for it, but, more commonly, a loathing of it; and in those of a milder or chronic form, a desire for food of a simple and ordinary kind may generally be indulged with impunity. Many instances, no doubt, are recorded of the relief, or even the entire removal, of chronic inflammation of various kinds, by a diet restricted to the simplest articles of food. Pulmonary consumption, for example, if any credit is to be given to medical testimony, has been in many instances cured by a milk diet; and I have myself, more than once, witnessed the great advantage derived from the avoidance of animal food in chronic inflammations, especially schirrus or cancer, as inculcated by my much respected and able colleague, Dr. Lambe. But I speak of abstinence here in relation to blood-letting chiefly, either as an auxiliary or as a substitute for this powerful remedy.

It is thought by many, that the withholding of food altogether is a matter of vital importance in the treatment of active inflammation, and that there is an inconsistency in allowing food to be taken where blood-letting is called for; as if abstinence and blood-letting were, as remedies, identical in their nature. Accordingly, one often sees a patient strictly enjoined to take only water, or at best a little thin gruel, and this to a very limited extent, for days together. All this, however, appears to me without foundation, resting on an assumption that is far from just. The notion is, that as abstinence and blood-letting both induce weakness, they are similar or identical in operation, and, on that account, fit and adequate substitutes for one another.

There is here, however, as I conceive, much misapprehension. It is assumed that blood-letting, in the cure of inflammation, effects its purpose simply by weakening the system; but this, I have before shewn you, is far from being the case. The single circumstance that the curative power of the remedy often depends as much, or even more, upon the manner of drawing, than upon the quantity of blood drawn, is decisive against such an opinion. The comparative effect of these two agents on the general system are exceedingly different. Privation of food, where the appetite calls for it, occasions much disorder in the system at large, as well as great emaciation after a time; whereas the operation of blood-letting, when confined within moderate limits, is exerted chiefly on the sanguiferous system; and, if slowly and quietly practised, may weaken in some degree, but occasions no general disturbance in the system. Instead of this it is often found to communicate fresh vigour to the organs of nutrition; exciting the appetite, and improving digestion; so that the body, instead of becoming emaciated, acquires an increase of bulk.

But even allowing that abstinence and blood-letting act in a similar way, the cure of inflammation, when violent, cannot safely be trusted to abstinence, however rigorous. This is by far too slow in operation to be relied upon in such cases; the disease will have made too great progress to be afterwards arrested, before the remedy begins to take effect.

Upon the whole, admitting, as must be done, that attention to diet is generally proper on these occasions, no uniform rule on the subject can be laid down. In acute diseases, and in the early stage more especially, abstinence, no doubt, is to be inculcated; though, generally speaking, such a direction is rendered superfluous by the want of appetite on the part of the patient. But in less active diseases, especially as occurring in weak subjects, and also in the advanced stages of such as are acute, no such restriction is required.

Judging from observation, I should say, that blood-letting is not only better borne, but proves more useful, and is more conducive to its purpose—that of inclining the disease to go off—when aided by a proper supply of food; regulated always, of course, by the desire of the patient, and limited to such as is of a mild and ordinary description. The good effect of this, depends, probably, upon its equalizing the circulation, which is always disproportionately determined towards the suffering part.

Another branch of the so-called *non-naturals*, which particularly requires attention, as auxiliary to blood-letting, in the treatment of inflammation, is that which regards rest and exercise. Generally speaking, it is desirable that all exertion, both of mind and body, should be avoided as far as possible. In all diseases of an inflammatory nature, where the circulation is much increased, and which, in consequence, may require blood-letting for their relief, repose is highly necessary; because all exertion tends to accelerate still more the circulation. In inflammation of the heart and lungs, this is of the greatest importance. Upon the same principle, in all brain affections, much attention is to be paid to the sensorial functions. In phrenitis, and in what is called idiopathic fever, for instance, it is highly injurious to make any powerful impressions on the organs of sense, or to exert much the voluntary power; because this cannot be done without a simultaneous exertion of the brain itself—or to employ actively the powers of the mind; and that for a similar reason. Inattention to these points is a frequent cause of the fatality of fevers, where they might otherwise have terminated favourably.

Of Counter-Irritation as a substitute for Blood-letting in the Treatment of Inflammation.

The principle of *counter-irritation* is of so extensive a nature, as to include almost all our most effective medicinal agents. Even blood-letting, as I formerly remarked, may be considered as coming within its range. The means of producing counter-irritation are very various. Every thing, in fact, that is capable of making a powerful impression on any part of the frame, may act in this way. Even the mind may be made a medium of thus impressing the body.

The kinds of counter-irritation chiefly relied upon, either as auxiliary to blood-letting or as substitutes for it, are vomiting, purging, sweating, and blistering, or any equivalent operation on the skin. To these may be added the effects produced on the system by antimony and mercury, which may be considered as acting on a similar principle.

For the practice of exhibiting the tartarized antimony in large doses, as a substitute for blood-letting in the treatment of acute inflammation, we are indebted, I believe, to the Italian physicians. It is remarkable of this medicine, that although it usually excites vomiting in the dose of a quarter of a grain, or even less than this, it may be exhibited in doses of several grains, often without any sensible effect. I know not how to explain this apparent anomaly. I have frequently administered this remedy, in the way here alluded to, in the practice of this Dispensary, but have not found it in general an adequate substitute for blood-letting in the cure of acute inflammation; nor should I feel myself justified in relying upon it exclusively in such cases.

The employment of mercury as a substitute for blood-letting requires greater notice. The peculiar and specific power of this remedy over the venereal inflammation, is too well known to dwell upon here. But the use of it has been transferred of late, and with scarcely less confidence, to other inflammations, and to those of the liver more especially. That there is much exaggeration in all this, I have not the least doubt. In the first place, I may remark, that mercury is so frequently used in combination with other means, that the conclusion come to in its favour is often questionable on this ground alone; and I know, from considerable experience, that a cure of those diseases may in general be accomplished by the ordinary antiphlogistic means. Without, however, denying altogether the utility of mercury in the cases mentioned, I am disposed to question its superiority over blood-letting, or other anti-inflammatory remedies; while it must be admitted that the inconveniences, and even injurious consequences, that often result from its too free administration, are of a very serious and distressing kind.

A combination of opium with calomel, was strongly recommended several years ago, by the late Dr. Hamilton, of Lynn, as a remedy capable of superseding nearly altogether the use of the lancet in the most acute inflammatory diseases. The idea is, that these two medical substances mutually influence each other, so as to form a compound with new properties—a *tertium quid*, and that this effects what neither of them singly could accomplish. There seems no great probability in this, if we compare the obvious properties of those two substances together—the one, opium, amongst the quickest acting of drugs—the other, calomel, requiring, as usually administered, many days to produce any marked effect. The operation

of opium, one would think, must be at an end long before the mercury commences its operation on the general system. It is, however, altogether a question of experience. I have myself often watched the operation of this combination, and I am bound to state to you that I have not found it an adequate substitute for other and simpler means, while the opium has, in many instances, had the same unpleasant and injurious effects, which this drug is generally allowed to produce, when exhibited singly, in cases of active inflammation.

With regard to counter-irritation in general, however produced, as a substitute for blood-letting, I may observe that, although it forms a useful auxiliary in many cases, and may be frequently relied upon alone in such as are slight, and where blood-letting is, from any cause, objectionable, it is to be considered, upon the whole, as far less effective in the removal of inflammation; and in cases where organs of importance to life are concerned, and where the known tendency of the disease is to disorganization, ought not to be relied upon, to the exclusion of blood-letting.

There is one other remedy which merits notice, as an auxiliary to, rather than as a substitute for, blood letting, namely, the *digitalis*. The power of this drug to diminish the action of the sanguiferous system, is very remarkable. It abates the frequency of the pulse, in a manner, and to a degree, that I believe no other substance does; and, at the same time, cools the skin, and lessens febrile action altogether; thus seeming to do much that blood-letting itself effects. It is not, however, to be relied upon, in active forms of inflammation, but requires the aid of blood-letting, to which it is rather an auxiliary than a substitute.

Of the use of blood-letting as a remedy for the consequences of inflammation.—The consequences of inflammation are sometimes the subject of treatment by blood letting, as well as the inflammation itself from which they spring. They vary with the part affected, and consist either in a change in the organization of the part, or in an increased and, for the most part, an altered secretion from it. Thus, with respect to the solids, these commonly swell or increase in bulk under inflammation, become indurated in their texture, and incapable of performing perfectly their function, whatever this may be. Further changes take place, according to the degree and duration of the disorganizing process, but which it is not necessary now to dwell upon.

When surfaces are attacked by inflammation, the secretion furnished by them is changed, both in quantity and quality. In internal surfaces that have no communication with the atmosphere; the fluid thus formed in excess, if not again absorbed, accumulates, and thus dropsies are produced; either in the large cavities, as the abdomen, chest, and others, or in the minute interstices of the cellular membrane. Sometimes, instead of fluid thus accumulating in the cavities or interstices mentioned, the inflamed surfaces coming in contact, adhere together by a vital union.

When the surfaces lining the canals or passages of the body (inlets as well as outlets) that communicate with the atmosphere, are attacked by inflammation, the membrane swells or thickens, so as to lessen the diameter of the passage; but the inflamed surfaces have no tendency to grow together as in the former case. If the inflammation be very active, the surface is hot and dry. Soon, however, the secretion returns, and is at first thin, transparent, and acrimonious; but gradually it becomes thicker, opaque, and mild in nature; and after a still further, though uncertain period, the secretion reverts gradually to its natural state. These cases constitute the *profluvia*, or increased discharges, of nosologists, and take the names respectively of coryza, catarrh, diarrhœa, dysentery, &c. according to the particular canal affected.

This swelling or thickening of the membrane, by narrowing the passage, becomes important at times, by impeding more or less the function of the organ to which it leads. Thus, with regard to the nostrils, breathing through these channels becomes difficult or impossible for a time. As, however, there is another channel for the passage of air into the lungs, this is comparatively of little moment. But in the narrower parts of the air-tubes, the obstruction to the entrance of air into the chest is sometimes so great as to endanger life itself.

Now to shew you the connexion of these affections with the use of blood-letting, I must remark, that as inflammation in all these cases is the primary disease, and as it frequently continues in greater or less degree with the altered condition of parts here described, the removal of the inflammation (supposing it still to be going on) is the primary and most important object of attention. In ordinary cases the inflammation, after running its course, subsides of itself, and requires little if any aid from art. But if, on the contrary, the inflammation lingers on, interference becomes necessary, and blood-letting may in such case be required. The propriety and necessity of this, however, will be determined by all the circumstances that

govern the use of this remedy, as before explained. And you are not to be deterred from using it merely because some have said that it is not adapted to inflammation when seated in the mucous membrane; for the contrary is the fact. It is true that blood-letting is not required in ordinary cases of this description, because the discharge itself soon takes off the inflammation; or perhaps it is more correct to say, the return of the discharge is the consequence rather than the cause of the subsidence of the inflammation, and that this takes place spontaneously. But in either way of considering the subject, the utility and propriety of blood-letting is unquestionable. Whenever the inflammation, therefore, proves obstinate, whether under the name of catarrh, diarrhœa, dysentery, or any other, but more especially when it interferes materially with a vital function, such as respiration, the remedy becomes indispensable.

Strictly speaking, blood-letting is not employed for the purpose of removing the consequences of inflammation; whether these consist in enlargement or induration of the solids (tumors), accumulations of fluid in the different cavities or interstices of the body (dropsies), contraction of canals or passages (strictures, as commonly termed), or increased discharges from the mucous membrane lining those canals (catarrhs, diarrhœas, dysenteries, &c.); but to take off any inflammation that still remains. When this object is fully and satisfactorily attained, opportunity is given for those internal changes in the part, by which the natural structure may be restored. When this happily occurs, swellings disappear, indurations are softened, contractions cease, and preternatural discharges no longer continue. All this is the work of nature, as we express it; and which art no further contributes to, than by removing, where practicable, the original cause of such morbid states.

ON INFLAMMATION,

AND THE MOTOR POWERS WHICH CAUSE AND REGULATE THE CIRCULATION.

By Professor Graves.

[Continued from page 533.]

If we take an accurate view of the general circulation, we shall find, then, that there is a great circulation of red fluid containing the raw material of all the tissues; which fluid, in its integral state, is destined chiefly for the muscles of voluntary and involuntary motion, into every part of which red vessels penetrate, and from which red blood returns. In fact, red blood forms, as it were, a separate circulation, sweeping by the white tissues, to which it merely detaches its uncoloured lymph, while the red blood enters the capillaries of the red tissues. When the minute arteries arrive at the parts where red blood is no longer necessary, they send off smaller vessels which contain only white blood, mixed with comparatively few, if any, red globules, while the branches which carry red blood proceed to join the corresponding veins.

I dissent from the common notion that the circulation of the blood goes on very rapidly. It has been computed that the heart expels from two to four ounces at each stroke of the left ventricle; and if we compute the quantity of blood in the body to be from twenty to thirty pounds, we shall be led to conclude that the whole mass of the blood passes through the heart in a very short space of time. This, however, is only taking a partial view of the matter. It is true that there is a rapid central current of red blood which accomplishes its circle through the body in a very short time, but a large proportion of the juices of the body circulates very slowly through the tissues it supplies, being detained in the capillary system for a considerable period before it is returned to the general mass of the circulation. If you compare the relative circulations of different classes of animals, you will find that they differ considerably in the composition of their blood, as well as the rate at which it travels through the system. Some animals have only white blood and a capillary circulation, without any distinct arteries or veins. Others possess vessels corresponding to arteries and veins, but still no distinct organ like the heart. Finally, we arrive at a higher class, which have not only distinct arteries and veins, but also a heart. In each of these classes the circulation differs, not only in the properties of the circulated fluid, but also in the velocity with which it travels. It is much slower, much more sluggish, in the lower than in the upper classes of animals. In the same way, blood does not circulate so rapidly in tissues of a low degree of organization (as bone, cellular and fibrous membrane) as in the red parts of the body. It is, therefore, not unreasonable to suppose that bone lives at one rate, fibre at another, muscle at

another, and nervous matter differently from all. These views are of importance when brought to bear on the subject of inflammation, and tend to explain the slow progress it makes in certain tissues.

You must have perceived that, from the very beginning, I have rejected the idea that the blood is propelled through the system by the *vis a tergo* alone. If that were the case, the current, though diminishing in velocity as it receded from the heart, would be equable in vessels of the same size throughout the whole system. But, in my opinion, the current of circulation has many different rates, which depend not on the *vis a tergo* alone, or the distance from the heart and size of the vessels*, but on the vital energy of the vessels themselves. Hear what Müller says on this subject:—"Wedemeyer's description of the course of the blood in the anastomosing capillaries agrees perfectly with what I have observed. Sometimes, he says, the red particles flow rapidly from one current into another, as if by attraction. In other cases the current which they join is very rapid, *but they are arrested, as it were, in the collateral current, and only from time to time find means of entering.* Sometimes a red particle is even thrown back out of the rapid current into a weaker stream, and is then again repelled. I have also remarked, that the same anastomosing branch between two currents sometimes receives the blood in one direction, and sometimes in the other, and that variations of pressure and position, and motions of the animal, are always the causes of these changes."

Such is Müller's testimony concerning the circulation in the capillaries, and it bears me out in the assertion, that a very great portion of the blood (using that word in its most comprehensive sense, and meaning thereby *nutritive fluid*) is comparatively stagnant in the capillary system; but I must confess that I felt much astonishment at Müller's assertion, that "all these variations in the capillary currents are, just as in currents of water in irrigated land, merely the result of mechanical causes."

Having made these preliminary observations, we are now better prepared to speak of the forces by means of which the circulation of blood is accomplished. Most authors, and with them Müller, have stated that the motion of the blood in the capillaries is wholly dependent on the heart's action. Now these vessels are mere simple membranous tubes, and there is no doubt that their membranous parietes must exert a strong power of endosmosis and exosmosis, as shewn by Dr. Rogers, in the American Journal of Medical Science. This power must necessarily have a great influence on the motions of the blood contained in the capillaries, causing a mutual interchange of contents between vessels in contact with each other, and between the vessels and surrounding parenchyma of the organs. Again, it has been proved by Dr. Draper, in the same journal, that in capillary tubes and organic pores a motion of the contents must result when the contained fluid possesses certain physical properties, from its mere contact with the internal surface of vessels so minute.

Here, then, are two sources of motive power, quite independent of the heart's action, and which must necessarily influence, in a most important manner, the capillary circulation; but this is not all, for there resides in the small vessels connected with the capillaries, whether minute arteries or minute veins, a vital sensibility which enables them, by suddenly or gradually changing their calibre, to increase or diminish the quantity of fluid in any particular organ or tissue.

Facts in abundance may be brought forward in proof of this assertion. When a fatty or a fleshy tumor arises on any part of the body, we have new vessels, as it were, created; and there is no reason to attribute their formation to any thing like a dilating *vis a tergo*. But the formation of the vascular system in the fœtus affords the strongest proofs. Here the smaller and more minute parts are formed first, the development commencing with the capillaries, and extending to the minute arteries and veins, and then to the larger trunks; until, at last, the heart is superadded, at first of an elementary, afterwards of a complicated structure. The best account of the development of the vascular system

* The blood's velocity in its progress from the heart is diminished chiefly by two physical causes, viz. increase of friction, and the increasing capacity (considered as a whole) of the vessels which contain it.

in the fœtus, is contained in Von Baer's work, published in 1837, in Konigsberg *. He says (Part II. p. 126), that there is no doubt that the blood is formed before the vessels. The formation of blood goes on in every part of the body, and, when formed, it is put in motion by the agency of some unknown cause which impels it in the proper direction, until it at length reaches the central formation of blood, around which is developed a tubular canal, afterwards to be further modified and changed into the heart. In truth, the first motions of the blood are towards the heart, *and consequently the first vessels formed, the veins;* a fact in itself sufficient to disprove the hypothesis that this motive power which presides over the circulation resides exclusively in the ventricles of the heart. What do we find occurring in the case of pseudo-membranes resulting from pleuritic inflammation? Exactly what takes place in the development of the fœtus. A large quantity of lymph is effused, which at first has no vascular connexion whatever with the parietes of the chest. After some time, however, the effused lymph becomes organized, *and vessels begin to form in its substance;* these extend gradually, and join the vessels of the tissue with which the lymph lies in contact. Of this formation of vessels in effused lymph there can be no doubt; I have often examined it with admiration, and it is likewise attested by Andral. When a mass of lymph, effused into the pleural cavity, is about to organize itself, and become vascular, a vast number of red points make their appearance throughout the mass, and are connected with very minute streaks, having a vascular distribution. In this lymph, then, red blood is manufactured, as in the fœtal body at an early period of development, and vessels are formed; and sanguineous circulation, no doubt, exists.

These facts, I say, bear strongly on the question before us, proving beyond a doubt that the vital properties of living matter are capable of forming vessels, and of rapidly increasing their size when formed. To account for the sudden increase in the size of vessels belonging to an inflamed part, we must look to this fact, and not rely solely on increased *vis a tergo*, aided by obstruction.

Now the whole of Dr. Marshall Hall's explanation depends on these two causes—*vis a tergo*, and obstruction. But I say, that vessels may be formed, multiplied, and enlarged, independently of these causes, and in consequence of an altered vital action of the parts in which the process occurs. Let me refer to the case of the impregnated uterus. In the unimpregnated state the womb is a small organ, with vessels and nerves so small as scarcely to admit of being satisfactorily traced. What takes place after conception? It has now new and important functions to perform, and it becomes proportionally increased in magnitude and vital activity; its arteries and veins become elongated and enlarged; its walls become thickened, and its nerves increased in size. And yet we are told that this increase in the size of its vessels depends on obstruction. Where does the obstruction exist? What proof have we that there is any increased *vis a tergo*? Will any of these principles account for the augmented size of its nerves? Tiedemann has proved beyond contradiction that the nervous matter of the womb is augmented to a very remarkable degree, during the impregnated state, and that minute nervous filaments, scarcely discoverable with the aid of a microscope, enlarge into bands visible to the naked eye. The same thing occurs with respect to the minute arteries and veins; from being but barely perceptible, they become large tortuous vessels, carrying an abundant supply of blood, and performing their functions with extraordinary activity. I do not pretend to offer any explanation of these facts; I merely place them before you, and shew you the analogy which exists between the vascular and nervous development.

The vessels increase in size and capacity; so do the nerves; and the augmented size and capacity of both depend on the same unknown cause. The nerves are developed in the same order as the vessels, and, like the latter, they increase from the circumference to the centre. Nay, I am persuaded, that, did our means of investigating the matter possess the same advantages as those we enjoy in the examination of the vessels, we should find that, in inflamed parts, the nervous matter increases in many

* Uber Entwickelungsgeschichte der Thière, &c. &c.

cases, as rapidly, and to as considerable an extent, as the vascular.

So far, gentlemen, I have endeavoured to lay before you proofs of the independence of the capillary circulation, a fact which I have long since brought forward in my public lectures, and of which I have written somewhat in detail, in my review of Dr. Joerg's work on Atelektasis of New-born infants. These views, I am happy to state, have been further confirmed by Dr. Houston, in a paper published in the 29th number of the Dublin Journal. In that paper, which I recommend to the attentive perusal of every student, Dr. Houston gives an account of an extraordinary case of twins born of a healthy young woman, between the seventh and eighth month of her pregnancy. One of the children was, to all outward appearance, perfect in every particular, and of the full growth of its age; the other, a female, and the subject of Dr. Houston's communication, was a monster, of somewhat smaller size than its companion. Both were alive at the time of delivery, but died almost immediately after. There was a separate cord, and a separate set of membranes, for each fœtus. The abnormal one had neither brain, heart, lungs, or liver; the kidneys were of enormous size, nearly filling the abdomen, and extending to the apex of the cavity formed by the ribs.

The umbilical vein, after quitting the cord, descended between the abdominal muscles and peritoneum as far as Poupart's ligament, and there opened into the external iliac vein, which became enlarged in size at this point. From this vein all the veins of the body were derived; large branches passed to the pelvis, thighs, and kidneys, and smaller ones to the intercostal spaces, and the tumor which constituted the head. These veins were devoid of valves, and terminated in the capillaries. From the latter, the arteries began by fine roots, and gradually coalescing, united into a sort of aorta on the fore part of the spine, which descending, divided into the iliac and hypogastric arteries in the usual way. No communication existed between the arteries and veins, except at their capillary terminations.

Such is the history of this very remarkable case, as given by Dr. Houston. I have not time, at present, to enter into his arguments, but I think he has satisfactorily proved, that in this instance the circulation was carried on without the aid of the heart of the other twin (as supposed by Sir Astley Cooper), or of the heart of the mother, and that it depended solely on the vital energy of the capillary and other vessels.

Another case of a monster without a heart, is related in the American Journal of Medical Science, for February 1838, by Dr. Jackson, of Boston. This was likewise a twin; and there can be but little doubt that its circulation was quite independent of any assistance derived from the heart of its fellow.

[To be continued.]

CASES OF INJURY OF THE HEAD.

To the Editor of the Medical Gazette.

Sir,

After the very excellent article in the 14th volume of the Medico-Chirurgical Transactions, by Sir Benjamin Brodie, on Injuries of the Brain and its Membranes, and the very ample detail of symptoms indicating different lesions of this important part of the body, it seems scarcely requisite to offer any further observations upon a subject which has already received such a large share of attention from surgeons of celebrity, in this as well as in other countries. Nevertheless, I feel it a matter of importance to accumulate as large a quantity of evidence, afforded by the actual observation of cases, as possible, on a subject of such vast importance, in order that we may thereby be strengthened in the opinions we have already formed upon the subject; or may be enabled to clear up any doubts existing in our minds upon the propriety of the employment of any surgical means ordinarily recommended. In this paper it will be my principal object to adduce a variety of cases and dissections, illustrating the known phenomena of injuries of the head, and to endeavour to form an acurate diagnosis from a particular train of symptoms of the seat of injury. It is unnecessary to insist upon the necessity of the strictest attention to what appear at first the most trifling accidents about the head, as the extent of mischief is in very many instances by no means indicated by the existing symptoms. In

proof of which I will adduce two cases, which came under my notice at the London Hospital.

Case.—A man, some time ago, was admitted on account of a wound in the forehead, unaccompanied with any particular symptom. He was discharged nearly well; but he returned in a fortnight, and was readmitted on account of a sloughing sore on the forehead in the seat of the original wound. He complained of intense pain in the head. A large portion of the frontal bone became exposed, and he died. In consequence of the urgency of the symptoms indicating suppuration under the bone, a portion of bone was removed with the trephine, and about an ounce of fœtid glairy fluid escaped, accompanied with some blood; it seemed to come from the substance of the brain. During the operation he complained of great pain, and was perfectly sensible. He got gradually worse, and died four or five days after the operation.

Post-mortem.—The fluid which had escaped at the time of the operation was clearly above the dura mater; but beneath the latter membrane, and in the substance of the hemisphere, there was a large extravasation of blood, and a distinct abscess. The case also beautifully illustrates the sympathy existing between the scalp and the dura mater.

Case.—A man, in apparently good health, a groom of Sir Robert Sheffield, whilst assisting in the landing of some horses from a steam vessel just arrived from Scotland, received a severe blow from the fore foot of a horse, upon the vertex, by which a large flap of scalp was turned back. It was accurately replaced, and under the usual treatment had healed to within scarcely more than a single line's breadth of granulation. He imprudently walked out of the ward into the garden during the prevalence of the late cold winds, and he had a violent attack of erysipelas. The inflammation commenced in the head, and gradually extended down the face and neck; the wound looked very well, and no cerebral symptom existed, beyond, at one time, a slight thickness in his speech. The inflammation had receded from the head, and more especially attacked the face and neck; and after some days he suddenly changed and died, as if from suffocation.

Post-mortem.—The neck was examined, and the mucous membrane of the larynx exhibited marks of inflammation. The calvarium was removed and examined; a slight depression of the external table, of a very small piece of the right parietal bone, was seen close to the coronal suture, and beneath this the inner table was fissured to the smallest possible extent. The portion of bone had evidently lost its vitality, and was in progress of exfoliation. There was a slight puriform secretion on the dura mater, which was ulcerated; and beneath this membrane a distinct layer of fluid and inspissated pus was remarked.

In reference to the first case, it appears evident that notwithstanding the effusion of a large quantity of blood had taken place in the substance of the cerebral hemisphere, yet this man was so well as to be able to resume his occupation; and it is possible that he might have still remained in health, had not a sudden attack of inflammation, leading to purulent deposit, taken place.

In the second case, although the two are by no means parallel, unless for the accidental occurrence of the erysipelas at that time prevalent in the wards, the small portion of bone thus deprived of its vitality would, in all probability, have become exfoliated without the least constitutional disturbance. It becomes, therefore, a matter of paramount importance to watch carefully every case, until even the slightest vestige of an apparently trivial injury remains unrepaired.

In the first case we have the disease commencing within and extending to the outside, whilst in the second the reverse is found to be the fact. The next case to which I wish to direct your attention is one as nearly parallel to the last as possible; but we have here presented a slight modification in the condition of the brain.

Case.—A man, 58 years of age, was admitted into the hospital in consequence of a contused wound of the scalp, which he had received from a fall. He had, at some former period, suffered from fracture of the skull on the opposite side, and had evidently lost a large portion of bone, as the pulsations of the brain were distinctly perceptible. No fracture could be discovered in the situation of the recent injury. The bone, however, died beneath, and exfoliation was proceeding favourably, when he was seized with

erysipelas, which at first was confined to the scalp, spread afterwards down the face and shoulders, and was accompanied with great constitutional disturbance. He was perfectly sensible; but subject, at intervals, to twitchings of the muscles of his arm and other parts of the body. He died about ten days after the occurrence of the erysipelas, being distressed at last with constant hiccup.

Post-mortem.—The viscera of the chest and abdomen were healthy. A complete separation was found to have taken place between the living and dead bone through the outer, and at one point through the inner table. Beneath the recently dead bone there was a small collection of matter, and the dura mater was in a sloughing state. A large quantity of fluid was effused from the arachnoid and in the ventricles; and at one part the hemisphere had undergone a curious change, being tougher than usual, giving rise, on section, to the sensation as if it were being cut with a blunt knife. Where the former accident had occurred, the dura mater was firmly adherent to the bone, and the arachnoid adhered, also, to the dura mater.

From the perusal of this case will be seen, independent of other considerations, the mode adopted by nature for the relief of the brain from the effects of the effusion of matter upon it; I allude, of course, to the perforation of the bone by one or more small apertures, by which a free exit may be provided for.

In the consideration of fractures of the cranium it is needless almost to remark, that a most important element is the position of the fracture, and the mode of its occurrence, as it is well understood that a variety of these accidents are in themselves of a trivial character, in consequence of the implication of a part of the cranium which is not so immediately concerned in the defence of the brain; I allude to those accidents incidental to the supra-ciliary ridges and the outer table of the frontal sinuses. Are we not almost justified in ranging these under the head of fractures of the face? for although the injury may be followed by symptoms of concussion of the brain, yet when these subside, no further mischief to this organ is to be dreaded. I am, however, free to admit, that in all cases it is better to keep a watchful eye upon them, and to follow up a strictly antiphlogistic plan of treatment.

Case of Fracture over Frontal Sinus.

A boy was admitted, having received a kick from a horse. There was a wound of two inches in extent, and a fracture of the external table over the frontal sinus. A portion of bone being detached, was removed, and the internal table was seen without any appearance of fissure in it. Under the anti-inflammatory treatment he recovered without an unfavourable symptom. In another instance, however, the result was not so favourable.

A groom was admitted with a kick from a horse, which detached a portion of the supra-ciliary ridge; it was accompanied with considerable injury to the right eye. Under ordinary treatment he recovered, but the eye suppurated.

On the other hand, those fractures which occur at the basis of the skull are, from their numerous complications, of the most serious character. Surgeons of the present day seem to question the possibility of the occurrence of fracture by *contre-coup*, and no doubt with great justice, as most cases supposed to be of this description admit of a very different explanation. I can, however, here adduce a single instance of this description of case, explicable upon no other grounds, as no direct force could possibly have been applied in this situation; although I am aware that it may not be altogether reducible in its circumstances to the mathematical explanation of these cases which has been offered.

A man, 55 years old, was brought to the hospital after a fall into a ship's hold. He died some few hours after admission. On examination, blood was found extravasated extensively upon the dura mater, compressing the posterior lobe of the left hemisphere. There was a considerable effusion under the dura mater. A fracture had traversed the left side of the occipital and petrous portion of the temporal bone. *A circular portion of bone, of about half an inch in diameter, in the centre of the right orbitar process of the frontal bone, was remarked driven into the frontal sinus, which was unusually large. No bruise or any trace of direct violence was observed externally near this spot.* Over this piece of detached

bone the anterior lobe of the right hemisphere of the brain was lacerated.

If, however, we are justified now in dissenting from the supposition that the fracture by *contre-coup* is so frequent in its occurrence as was formerly supposed, there seems every reason for believing that the substance of the brain itself may at any rate be injured in that manner, and, as Sir Benjamin Brodie has remarked, that the laceration of its substance generally occurs at a point opposite to that on which the violence was inflicted, and that generally such laceration occurs at the basis; nay, I remember a single instance in which considerable laceration took place totally independent of a fracture, there being here very evident predisposing causes to this affection. These lacerations usually occur at the surface, and are in this respect different from the effects of an apoplectic attack, which ordinarily occurs nearer to the central parts of the encephalon.

A man, 52 years of age, short in stature, with a thick short neck, and fat, was admitted in consequence of a blow on the head, received in a scuffle when drunk. When first admitted he appeared insensible from the effects of the liquor. The next day he complained of slight pain in the head, and was ordered some purgative, by which he was relieved. The day following the pain recurred, and on the fourth day he was suddenly seized with convulsions and stertorous breathing. About sixteen ounces of blood were removed from the temporal artery; the blood was dark and venous as to its colour. He died shortly after.

Post-mortem. — Generally increased vascularity of the brain. On the side opposite to the point where he had received the blow there was a large quantity of blood extravasated into its substance.

The next three cases illustrate the laceration by *contre-coup*, accompanied with fracture.

A man, 60 years old, in a state of intoxication, received a blow from the handle of a truck, by which he was knocked down, and he struck the back of his head against some steps. He was admitted in a state of insensibility. From this state he somewhat recovered. He, however, laboured afterwards under symptoms of pressure upon the brain, but did not perfectly lose his sensibility until just before his death.

Post-mortem.—When the calvarium was removed, some blood escaped from the surface of the dura mater, and there were two or three round patches of extravasation on that membrane. Beneath the dura mater at least half a pint of blood was effused. The anterior lobes of the brain were lacerated, and blood was extravasated into them. A fracture was perceived extending from the great transverse ridge of the occipital bone to the foramen magnum; it traversed the condyloid process, and passing over the petrous portion of the temporal bone, terminated in the anterior fossa of the cranium.

A man, 51 years old, fell from a stage whilst repairing a ship. He was brought to the hospital in a state of insensibility. The symptoms indicative of pressure on the brain supervened, and increased gradually. The head was carefully examined; there appeared considerable ecchymosis over the right temporal region, and at the back of the head. I made an incision over the temple, but could not detect any fracture. He died thirty-eight hours after the accident.

Post-mortem. — A separation of the lambdoidal suture on the right side, with a fracture extending upwards from it; effusion of blood upon the surface of the brain, from laceration of the longitudinal sinus. The anterior lobe of the right hemisphere lacerated, and hæmorrhage into its substance.

A man was admitted in consequence of a fall from the third story of a house. He had been doing well, and was sitting up one morning taking his tea, when he was suddenly seized with symptoms of pressure upon the brain. His cranium was closely examined, and a wound was found over the anterior part of the left os parietale. An incision was made, and after some search a fracture was discovered. Three pieces of bone were removed with the trephine. A small quantity of blood was found on the dura mater; this membrane was slightly lacerated, and bruised cerebral substance escaped. The symptoms were unrelieved, and he died soon after the operation.

Post-mortem. — Extensive fracture across the petrous portion of the left os temporis. A small quantity of blood effused above and beneath the dura

mater. Right hemisphere of brain extensively lacerated, and blood extravasated into it; no fracture in its vicinity.

It seems, however, needless to multiply examples of this fact; every day's experience teaches us that the brain is in very many instances, I would almost say generally, injured more or less on the side opposite to the part where the principal mischief to the bone is observed; and if such be the case, it will still further remove from us any expectation of a successful application of the trephine. The relation of cases will, perhaps, more forcibly illustrate this position.

A man, 73 years old, was admitted, having fallen down a ship's hold; he was in a state of partial insensibility, with a wound on the left, and another on the right side of the head. It was difficult to ascertain on which side he had pitched in his descent. On examining the wound on the left side, a fracture was discovered, with slight depression of the bone. The fracture was observed on the side and upper part of the os frontis, just anterior to the coronal suture. It seemed to be the termination of a fracture which must probably have traversed the base of the skull. The symptoms indicated pressure upon the brain, and as they continued unrelieved, on the following day a further examination was made; and although the situation of the fracture appeared anterior to the position of the spinous artery, the trephine was applied, and a portion of bone removed, including a large piece of that which was depressed. A quantity of coagulated blood was visible on the dura mater. Another piece of bone was removed, which clearly defined the extent of the extravasation, as the dura mater could be distinctly seen at one part. The first portion was traversed by a groove of at least a large branch of the spinous artery. There was a slight hæmorrhage from the artery, but it soon ceased. He appeared somewhat relieved after the operation, but he died three or four days after.

Post-mortem.—The effusion which had taken place on the left side evidently came from the spinous artery, but the middle lobe of the opposite side was lacerated.

A man fell into a ship's hold, the distance of 12 feet, and was supposed to have struck his head against some projecting body. He was admitted in a state of insensibility. My attention was directed to a swelling of the scalp at the upper part and left side of the cranium: this apparently indicated a mere effusion of blood beneath the scalp. His symptoms might, it was imagined, possibly merely indicate a violent degree of concussion. He was therefore left for eight or nine hours, until the evening; his symptoms had undoubtedly increased, and decided paralysis of the right arm and right side of the face had supervened. All the other symptoms now proved the existence of pressure on the brain, and a swelling had taken place over the left temporal region. It was agreed, in consultation, to make a further examination. An incision was made, and a distinct fissure was observed, without depression. The termination of the fracture corresponded to the point where the effusion of blood under the scalp had been first remarked. A portion of bone was removed, and blood was seen upon the dura mater beneath. He appeared slightly relieved by the operation, but he soon relapsed, and he died two days after the injury.

Post-mortem.—A fracture was found extending across the left petrous portion of the temporal bone; this traversed the groove of the spinous artery. A large quantity of blood was effused upon the dura mater. The substance of the brain was lacerated on the corresponding side, but the base of the opposite hemisphere was most extensively lacerated.

Joseph Maynard, æt. 57, was brought to the hospital, having fallen into a cellar in the Docks, and pitched upon his head. He was in a state of partial stupor, in which state he continued for some time, without any other decisive marks of pressure on the brain. There was no external wound, but a puffed condition of the scalp near the vertex, towards the right side. As no improvement in his symptoms occurred, an incision was made over the contused part of the scalp; and after a most minute inspection, a slight fissure was discovered. This was traced down towards the temporal region, and was found to be of considerable extent. The trephine was applied, and two portions of bone were removed, and an angular piece

was sawn off by Hey's saw. An effusion of blood had taken place upon the dura mater. But transient signs of relief were perceptible, and in two days he sunk.

Post-mortem.—A fracture was seen traversing the anterior inferior angles of the right parietal bone, and the adjacent part of the squamous part of the temporal bone. In the course of the fracture, blood was extravasated on the dura mater, as also under this membrane. The substance of the right hemisphere was softened, and a slight laceration of the base of the right middle lobe was visible; but a most extensive laceration of the under part of the middle lobe of the *left* side had occurred, and on this side no evidence of injury to the bones existed.

I feel convinced, in my own mind, that we can readily account for the want of success in the treatment of injuries of the head by the operation of trephining, by reflecting upon the extensive lesion that must almost necessarily occur, when violence is applied sufficient to fracture that part of the cranium traversed by the middle meningeal artery. I have little doubt but that, in the greater number of instances where this accident has taken place, the fracture thus implicating this important blood-vessel but rarely occurs from the direct application of force to the vicinity of the artery; but that the fissure which traverses the groove of the anterior inferior angle of the parietal bone is but a continuation of a fracture which has commenced at the basis of the skull, and which necessarily implies a very extensive degree of violence. But are we to discard the operation because it is generally unsuccessful? I think not; for I cannot conceive that we place our patient in a worse position by the mere removal of the bone (although, I admit, even this cannot be effected without much violence) than when we leave him with an extensive effusion of blood upon the brain, with no outlet for its escape. Nature seems especially to have provided for the security of this artery, by defending it with the thick soft temporal muscle, and its its firm aponeurosis, but I need not say that it does occasionally become injured by direct force, as many cases testify to that fact, and one of which I have recorded in a former number of the Medical Gazette, and where the trephine was successfully applied; but I think that if we have in view the circumstances I have just hinted at, we may by possibility form an opinion as to the probability of a successful issue to the operation. Much doubt will occasionally exist as to the proper situation for the application of the trephine, in cases, from the usual train of symptoms, evidently indicating the effusion of blood upon the brain; but if we adopt the principle that in by far the greater number of instances the blood is most likely poured out from one or other of the spinous arteries, we should be particular in making a careful examination of both temporal regions, and where there is the slightest tension there let an opening be made.

There are some occasional exceptions to what we may almost term a general law—I mean, that the effusion of blood is the consequence of rupture of the spinous artery. Thus I will here relate a case where the effect depended upon an escape of blood from the lateral sinus.

A man, ætat. 50, was thrown from a horse, and pitched upon his head. He was taken up senseless, and was bled to 16 ounces. He remained in the same insensible condition the whole night. On the following day he was brought to the hospital in a state of insensibility. All the usual symptoms of pressure upon the brain were strongly marked. He was bled to 40 ounces. He died shortly after the bleeding.

Post-mortem.—On removing the scalp, a quantity of effused blood was observed at the back part. The calvarium was raised, and about three ounces of dark blood found upon the dura mater. It seemed to have escaped from a laceration in the left lateral sinus. The occipital bone was fractured, and the lambdoidal suture on the left side was separated. On raising the dura mater, a small quantity of blood was discovered upon the anterior lobe of the left hemisphere. The left side of the brain was much flattened at the upper and outer part. I will also relate another instance of effusion, and in this case it is probable that the blood came from the vessels of the pia mater, as no fracture or laceration of the dura mater was found after death.

A youth, 16 years of age, was engaged in a pitched battle, in which he was knocked down several times; about

the fourth round he fell into a small pit. He was removed from this, and fought three more rounds, when he was taken home insensible, having complained of great pain in the head. He was brought to the hospital on the following morning, in a perfectly comatose state. He was somewhat relieved by a large bleeding, but he died in the evening. On removing the scalp, there were many marks of bruises, especially at the back part. When the calvarium was removed, there were found many bloody points of extravasation upon the dura mater. On raising the latter membrane, about four ounces of dark blood was found pressing on the right hemisphere, some of it extending between the falx and the hemisphere. The blood-vessels were excessively gorged with blood. In the right centrum ovale there was a slight extravasation.

Of the diagnostic marks afforded by the condition of the Irides.

The extreme sensibility of the iris in obedience to the functions of the retina, and the rapidity with which its power of contraction is affected under injuries and diseases of the brain, have induced the surgeon to pay the utmost attention to the condition of the pupils, in forming his diagnosis of the state of parts within the cranium. In estimating this diagnostic element it is important to bear in mind the anatomical structure and physiological associations of this membrane. It is scarcely requisite here to canvass the existing opinions, as to its muscular or its vascular arrangement; suffice it to say, that physiologically it has all the properties inherent in muscular fibre, and anatomically, according to the most recent German microscopist, it is strictly identical in structure. But by whatever mechanism its condition is affected, we know that it is generally regulated by the state of the retina, and that in some rare instances its irritability remains, when the susceptibility of the retina to the impression of the ray of light is lost, and also that it is to a certain degree under the influence of the motor nerve of the eye, as is sufficiently proved by the experiment of Mayo on the cat and pigeon. We may therefore conveniently refer the derangement of its actions to two heads; either as they are affected by the state of the retina and brain, or the motor nerve. I am well aware that in the present day many surgeons have almost discarded the evidence afforded by the pupils, in estimating the effects produced upon the brain by accident, observing that in cases of concussion and compression they are at one time in a contracted state, in another in a dilated condition, under both these affections: and I certainly cannot myself attach indiscriminate importance to the symptom, so long as both irides act in unison. Yet from some observations made in many severe cases, I have arrived at the conclusion, that organic mischief to the brain or nerves almost always, if not invariably, is indicated by a want of accordance of these membranes. The active state of the iris is of course clearly witnessed when the pupil contracts, and *vice versâ;* and therefore when permanently dilated, we suppose it to be paralysed, and consequently incapable of contraction: can we not, therefore, form a diagnostic mark as to which hemisphere is the seat of injury, from observing on which side the dilatation occurs? There is this difficulty in the way of the inquiry. Each retina is connected to the brain by nervous filaments, some of which go to one side, and some to the other; or may be said to receive its nervous supply from the two sides of the brain; and it is well proved that pinching the optic nerve on one side causes contraction of the opposite pupil. But it receives its power of contraction from the third pair of nerves; and if this nerve be irritated on one side, the corresponding pupil is affected. It is this complication which renders the enigma so difficult of solution. We are therefore to found our opinions upon the evidence afforded by actual observation, and in this manner to endeavour to draw the conclusion. Now from the generality of cases I shall refer to, it will be found that the dilated pupil indicates the side on which the principal mischief is situated.

In proof of the position I have advanced, that a discordance of the pupils is indicative of some organic mischief, I may adduce the testimony afforded by the cases recorded by Sir Benjamin Brodie, in the 14th volume of the Medico-Chirurgical Transactions, pages 345, 353, 362, 373, and 375, in which it will be found, that in some of these cases the dilated pupil corresponded with the side affected; in others it was

the reverse. Now many of these cases were not followed by a fatal result, but still the evidence of pressure upon the brain is clearly evinced in most of them.

There are, however, some cases which have fallen under my own observation, which, from the great extent of injury, were necessarily fatal, and to these I beg to direct attention. A man fell down a ship's hold the distance of nine feet: he was brought to the hospital in a state of insensibility: his skin was cold, pulse slow, breathing stertorous; the irides do not correspond, the right pupil being contracted and slightly sensible, the left dilated and quite insensible. He had a wound over the left eye, and florid blood oozed out of the left ear. The sealp on the left side was exceedingly tumefied; an incision was made, and a fracture diseovered in the neighbourhood of the coronal suture; a portion of bone was removed with the trephine, and blood was found extravasated upon the dura mater. There was no relief to the symptoms; he died in about 40 hours after the accident.

Post-mortem. — The skull-cap was removed, and a large effusion of blood was found upon the left hemisphere from the left spinous artery. The fracture extended across the groove of this vessel, traversed the base of the skull to the opposite side, through the petrous portion of the right parietal.

A man was brought into the hospital in an insensible condition, having fallen from a scaffolding the distance of 40 feet. He was labouring under the ordinary symptoms of compression. The pupils were at first both eontraeted, but afterwards the right became permanently dilated. He soon died.

Post-mortem.—A large quantity of dark coagulated blood was found effused upon the dura mater of the right side. There was an evident fissure in the track of the spinous artery of this side. At the basis an evident laceration of cerebral substance had occurred.

A man fell from a cart in a state of drunkenness, and pitched upon his head: he was brought to the hospital quite insensible. With the ordinary symptoms indicative of pressure, the irides were very inactive, the left contraeting more freely than the right. The man died, and on examination extensive fracture on the base of the skull was found.

A similar diseordance of the irides was remarked in one of the fatal cases I have detailed, illustrative of the laceration of the cerebral hemisphere by *contre-coup*, in which there was a slight effusion upon one side, namely, that which corresponded to the diminished pupil; but on the other there was extensive laceration of the cerebral hemisphere, and effusion of blood on the same side.

The next case, although not attended with a fatal result, nevertheless proves that some serious injury must have been inflicted on either the brain itself or on the nerves at its basis, and is complicated with a paralytie condition of the portia dura on one side. A labourer in the West India Docks was raising a sugar hogshead by a crane, which gave way, and the hogshead struck him on the head. He was removed to the hospital, perfectly insensible. He had hæmorrhage from both ears. The left eye was elosed. His right pupil was in a mid state between dilatation and contraction; his left was dilated. He recovered slightly, and afterwards relapsed. I shall not describe the treatment, which was strictly antiphlogistic, and he was put under the influence of mercury, under which he gradually recovered, except that the left eye remained closed, and the right portia dura remained paralytic, as shewn by the inability to close the eye-lids, and the immobility of those parts influenced by the muscles supplied by this nerve.

In the Medical Gazette for May 26, 1838, are two cases related as having been admitted into Guy's Hospital, which also bear most distinctly upon this point.

Before we ean draw any general conclusions, it is requisite that we should aecumulate as large a collection of eases as possible, to afford us the greatest abundance of indisputable evidence. From the few which I have detailed, however, and from those to which I have alluded as mentioned by Sir Benjamin Brodie and others, I trust the position I attempted to support is maintained; and the praetical inference to be drawn from this position is, that a guarded opinion as to the result of a case is to be given where the phenomenon of a want of accordance of the pupils persists.

I shall conclude these remarks with

the relation of two cases of laceration of the brain from instruments penetrating the orbitar processes of the frontal bone.

A man was heaving coals into a basket in the hold of a collier, and the hook by which the basket was drawn up slipping, caught him on the orbit: he was drawn up some distance before his situation was discovered. The wound was examined, and was found to penetrate a considerable distance, but the eye and brain appeared to have escaped. He was in a state of collapse, and had a shivering fit. He was bled, and took some calomel. He was perfectly sensible until the following afternoon, when suddenly symptoms of pressure upon the brain supervened. Cerebral matter, mixed with blood, oozed through the wound, and he died about ten hours after.

Post-mortem.—There was found an extensive fracture through the orbitar plate of the frontal bone, with laceration of the anterior lobe, and copious effusion of blood into its substance.

A man, 40 years of age, whilst filling a dung-cart, received a blow from the fork of another man, in the left orbit, by which he was stunned. There was a large effusion of blood into the palpebra, but the eye was apparently but little injured. When admitted, he was quite sensible, and was much chilled over the surface of his body. In a few hours he became suddenly insensbile; his breathing became stertorous; the muscles of the superior extremities were convulsed; he was immediately bled to 20 ounces, by which he was much relieved; he was again bled; his fæces and urine passed involuntarily; and he died on the following day.

Post-mortem.—There was an extensive effusion of blood under the dura mater, but this was observed principally on the side opposite to that on which the injury was received. There was a small opening in the orbitar plate of the frontal bone, through which the prong of the fork had entered the anterior lobe of the brain, as far as the lateral ventricle.

JOHN ADAMS,
Assistant-Surgeon to the London Hospital.

31, New Broad Street, June 1838.

THE MORBID ANATOMY OF PHTHISIS*.

BY F. W. GRANT CALDER,
Assistant-Surgeon, 2d Regiment of Life Guards.

TUBERCLES have now been found in almost every organ of the human body; the lungs, however, may be considered as their chief, and, in the majority of instances, their primary seat, for M. Louis, whose authority on this subject may be considered as paramount, tells us that, with one single exception†, he never found tubercles in any other organ without their existing in the lungs at the same time; and, indeed, he considers their presence in the lungs as essential to their being in other organs: This fact he considers as proved by their being always much further advanced in the lungs than in any other organ. I shall presently, however, relate some interesting cases which my limited experience has presented me with, and which tend very much to shake the validity of this sweeping statement of M. Louis.

Dr. Baillie considered that tubercles were most common immediately before the completion of the growth; they, however, appear to belong more to middle age, for they are confessedly very rare both in the fœtus and in the odentulous subject, being seldom seen under five years, or after fifty.

According to Andral, tubercles occur in other organs, without there being any in the lungs, more frequently in children than in adults. They have also a tendency to affect a greater number of organs at once, and the organs which are most frequently affected in adults are not those which are so in children. He gives a table from M. Lombard in proof of this statement. The organs in which they have been most frequently seen, are the lungs, cellular tissue, under the pleura pulmonalis and costalis, in adhesions connecting these latter; in the subserous and submucous cellular tissues of the intes-

* This may be considered as a continuation of the article on the Morbid Anatomy of Scrofulous Formations, which appeared in the thirty-third and thirty-fourth numbers of the present volume.

† The exception occurred in a case of typhus. No tubercles existed in the lungs, and yet there was a small quantity of tuberculous matter in the mesenteric glands.

tinal canal, subserous tissues of the peritoneum generally, spleen, ureters, prostate gland, testicles, uterus, brain and its membranes, cranial bones, vertebræ, ribs, &c. &c. They have likewise been found in the extremities of the cylindrical bones, and in the bones which compose the tarsus and metatarsus. This fact has been well attested by Sir B. Brodie, in his valuable work on the Diseases of the Joints. They have likewise been said to be very common in the bronchial, cervical, and mesenteric glands; and they are moreover said to occur in the liver, kidneys, epididymis, vasa deferentia, and vesicules seminales; but although I have met with abundance of tuberculous disease in these parts, yet I am not aware of ever having seen a *tubercle* in either of them. From what I have already said, it will be seen that I consider the tuberculous matter which is met with in these latter organs to be in its nature a pure inorganized secretion from the first, and not to have its origin after the manner of tubercles, as met with in the lungs, or in the submucous or subserous cellular tissue of the intestinal canal.

Out of seventy cases of phthisis, the post-mortems of which I had a hand in while stationed at Chatham, two were under twenty years of age, ten were between twenty and thirty-five, twenty-two were between twenty-five and thirty, seventeen were between thirty and thirty-five, eight were between thirty-five and forty, four were between forty and forty-five, three were between forty-five and fifty, one was between fifty and fifty-five, one was aged fifty-six, one was aged fifty-eight, and one was aged sixty-eight.

These results tend to give some information of the average age when soldiers break down from this disease, and I regret exceedingly that my removal from that station prevented me from carrying this point further out: owing, however, to the annual mortality from this disease in the army, and from the very excellent arrangements and orders of the present enlightened Director General (and which orders I need not say must be scrupulously carried into execution), much may yet be expected by the profession on this interesting head.

Out of the seventy cases alluded to, fifty of their brains were very minutely examined, and the following is the only case in which that organ was found affected. It is likewise the only instance where I ever met with an encysted tubercle:—

Case.—James Hermen, æt. 31, 10th regiment of foot, was sent home, from the Mediterranean, labouring under phthisis; and on his admission into the General Hospital he became my patient. He was evidently far advanced in consumption, and in addition to a profuse muco-purulent expectoration, under which he laboured, the stethoscope pointed out very distinctly the existence of a large cavity in the superior lobe of each lung. Little was being done for him, but it was observed that his manner was peevish and discontented: he answered questions hastily, and appeared irritated at being interrogated. After about a month's residence in the house, he was suddenly seized with dilated pupils, stertorous breathing, and partial loss of sense and voluntary motion. He passed his urine and fæces involuntarily, and it was observed that during all this time his pulse remained full and labouring. In this state he continued for a few days; constant low muttering delirium succeeded, and ultimately the most complete coma, and death. On dissection, his lungs were found minutely studded with tubercles in various stages, and the existence of a cavity in the superior lobe of each lung was verified. The upper parts of the brain were generally healthy, but the whole base was covered with recently effused lymph and serum. Imbedded in a circular depression on the inferior surface of the right lobe of the cerebellum, and external to the arachnoid, a tubercle the size of a hazel-nut was found, generally firm, but broken up at one part. The remainder was enveloped in a proper membranous capsule, and there was every reason to believe that its partial rupture, and the consequent extravasation of the tuberculous matter into the cavity of the arachnoid, with which it communicated at this part, had produced the cerebral symptoms which preceded his death. The tubercular matter was intimately adherent to its capsule all round, and there is little doubt but that this was furnished by the pia mater, amidst the meshes of which it became primarily developed.

In sixty-eight of the cases tubercles were found in both lungs. In the fol-

lowing one, miliary tubercles were pretty generally scattered throughout the substance of the right lung, at the same time that none were observed in the left one :—

Case.—Private Ralph Barlow, 80th regiment, was admitted into the General Hospital, from detachment at Harwich, with lumbar abscess. He had been ill for two years, but had only been off duty for the two preceding months. He gradually sank, apparently under the more prominent symptom — the discharge of pus from the right groin. He, however, had complained of cough and expectoration for months before this event took place. In addition to the above-mentioned fact regarding the lungs, it may be noticed that numerous small circumscribed deposits of pus were situated towards the lower part of the ilium, immediately beneath the mucous membrane. They were of the size of pin-heads, and were complicated with ulceration of the mucous tissue of the ascending colon and lower third of the ilium. I have mentioned these purulent depôts, not that I consider them as having any thing tuberculous in their nature, but as tending to corroborate an opinion of M. Louis', who has noticed their frequent coincidence with tubercles of the lungs.

In the following case, the reverse was observed, for the left lung was found affected, while the right one remained sound. *Case.*—Edward Flannigan, aged 31, 1st Foot, was admitted into the General Hospital on his arrival from the West Indies, under the care of my esteemed friend, Dr. Anderson, now of the 82d regiment, but then an Assistant Staff Surgeon. He had served nine years and upwards in the West Indies, and while there, had suffered greatly at various times, from hepatic and splenetic affections; likewise from intermittent and remittent fevers : latterly he had been affected with dysentery, for which disease he was invalided. On his admission, his symptoms were those of a well-marked case of chronic dysentery, under which he gradually sank. He had likewise a slight cough, attended with a scanty muco-purulent expectoration, and by means of the stethoscope a cavity was found to exist under the left clavicle: on the *post-mortem* examination, in addition to an abscess in the liver, complicated with an ulcerated and rotten colour, and the other attendants on dysenteric diseases, we found a small tuberculous excavation in the superior lobe of the left lung, attended with tubercles in various stages of progress; tubercles were likewise found in the inferior lobe, together with a hard cartilaginous body, of the size of a walnut. The right lung adhered firmly to the walls of the chest, but was healthy throughout; no tubercles being met with in it.

By a majority of eight cases, the right lung contained more tuberculous diseases than the left one; but this latter lung again, by a majority of two, exceeded the right one in the frequency and magnitude of the excavations in its superior lobe.* In four cases only were excavations found in the middle lobe of the right lung, unconnected with like cavities in the superior lobe; and in only one case did a tuberculous vomica exist in the inferior lobe, while the superior and middle remained free. And in only three cases out of the whole number did the inferior lobe of the left lung present tuberculous cavities, and then they were complicated with similar excavations in the superior lobe. Excavations of small size were pretty frequently met with in the middle lobe of the right lung, connected with like cavities in the superior one; and this again was observed to exist much less frequently in the inferior lobe.

In almost every case, adhesions to a greater or less extent, of the parietal and visceral layers of the pleura existed; but in thirty-six of the cases these adhesions extended over the whole surface of each lung, and consisted of a firm kind of filamentous tissue, dry in its nature, and differing in these particulars from those adhesions which are generally described as being the result of previous inflammatory actions. The cases, however, in which tubercles exist in these adhesions are confessedly very rare. None of the cases were complicated with empyema of both sides, but in one case it existed in the right side; and in another it existed in the left. In the former, no communication with the lung could be detected, but in the latter there was a tuberculous excavation in the top of the corresponding lung, which communicated very freely

* Forty-eight times, one or more excavations were met with in the superior lobe of the right lung; while fifty times, the same was observed in the upper lobes of the left lung.

both with the bronchial tubes and bag of the pleura; and yet no air existed in the latter cavity. The lung was very much condensed, and bound down by adhesions to the side, and as it was made into a preparation, it may now be seen in the third division of the Class Respiration.

Those cases of empyema occurring in phthisical subjects, are for the most part dependent upon the partial discharge of scrofulous matter, situated immediately under the pleura, into the bag of the latter membrane; and this acting as a foreign body gives rise to the diseases in question. Should the whole of the tuberculous matter become thus discharged, the probability is, that we should then have a communication opened with a bronchial tube, air extravasated, and pneumothorax produced.

There is reason, however, to believe that deposits of tuberculous matter, equal in size to a garden pea, become discharged through ulcerated openings in the pleura, and without air being subsequently emitted: for I have repeatedly examined these openings, and I have found that the tissues which had been in immediate contact with the tuberculous matter were rendered firm and impervious to air. More frequently, however, these deposits, when met with so circumstanced, are only partially discharged.

In no case was hydro-pneumatothorax of both sides met with, but in three cases it existed in the right side; and in an equal number it occurred in the left. In the right side, the fistulous opening by which the air escaped, in two of the cases communicated with tuberculous excavations in the superior lobe, and in the remaining one the fistulous tube occurred in the middle lobe.

On the left side, again, in each of the three cases, the fistulous tube communicated with an excavation in the superior lobe. The results already mentioned regarding the very frequent occurrence of advanced tuberculous disease in the upper lobe of this lung, in preference to the lower, explain how this might *à priori* have been looked for.

These fistulous tubes were generally of good size, for the most part equal in diameter to a crow-quill, and always communicated very directly both with the tuberculous excavation and cavity of the pleura. In no instance did it appear as if the lung had been ruptured; for, on the contrary, the tube had a patent mouth on the surface of the pleura, round, and clearly indicating a complete loss of substance. The whole tract, which varied in length, and which in only one case extended for a space of two inches, invariably presented firm and gristly parietes, and its lining membrane had generally the same kind of structure as that lining the excavation with which it communicated. Moreover, the portion of lung around its exterior had invariably that condensed and solidified appearance which it generally presents in the immediate neighbourhood of an ordinary excavation, from the softening of tuberculous matter. This circumstance has led me to the belief that in some of these cases the tuberculous deposit had extended in a finger-of-glove-like manner, from the central nucleus to the cellular surface of the pleura, and that afterwards, on softening taking place in it, the pleura covering it had ulcerated, and thus formed a ready exit for the disorganized matter into the cavity of the chest, which in the first place gave rise to inflammation and its consequences, either the effusion of serum or of pus, or of both; and this again sooner or later was followed by the extravasation of air from the ulcerated opening in the lung, communicating with one or more bronchiæ. How it is, that in some cases the pleura covering a tuberculous mass should thus readily assume the ulcerative process, and without presenting the slightest degree of re-action, either as characterized by vascularity or effusion of lymph upon its surface, or into its substance, while in other cases, either in the opposite lung, or even in the same lung, such strong evidences to the contrary are to be met with, (a deposit of tuberculous matter being in one example covered by a gristly pleura, while in another, coagulable lymph, in various degrees of organization, exist on its surface) is somewhat difficult to explain; such, however, is the fact.

In only one case was pneumo-thorax met with; the case terminated fatally very soon after the attack, and to the suddenness of this event may be attributed the circumstance of air alone being found in the cavity of the pleura.

The disease occupied the right side of the chest, and the opening in the lung was in the superior lobe. It occurred in the person of a young soldier of the 79th Highlanders, who had for some time been my patient, but who lately was under the care of my excellent friend, Dr. Campbell, now of the 1st Life Guards. We had therefore, in all, seven cases, where air was met with in either cavity of the chest, and depending upon tuberculous diseases of the lungs; in four of which it was extravasated into the right cavity of the pleura, and in three of which it occurred in the left side.

In no case were tubercles found either in the larynx, trachea, or bronchi. In seventeen of the cases the chordes vocales were very extensively ulcerated: sometimes the ulceration was confined to one, but for the most part both were so affected; and in two cases the posterior aspect of the epiglottis was very much destroyed by a similar process. Sometimes these ulcers of the larynx were found covered with a kind of curdy-looking matter, the real nature of which, however, was at best but equivocal. Not unfrequently many small ulcerated-looking points existed in various parts of the trachea, and in eight cases there were distinct and large ulcerations, some of which had laid bare its cartilaginous rings, while others again had not only destroyed these, but had even extended to the subcutaneous cellular tissue. Now five of these eight cases of well-marked ulceration of the trachea were situated on its anterior aspect, while the remaining three occupied its posterior surface. I mention the fact, as it is at variance with a statement of M. Louis, who considers that ulcers of the trachea are much more common on its posterior than its anterior aspect, a circumstance which he attributes to the irritating nature of the sputa of phthisical patients, and the facility which in them is offered for its producing disease, in consequence of the supine position which individuals so affected for the most part assume.

In only five cases were the primary divisions of both bronchi ulcerated, and in two of these the right one was chiefly so. If, according to M. Louis, the expectorated matter had any thing to do with the production of these ulcerations, there is reason to suppose that we should have this occurrence more frequently; at least, according to his theory, it is somewhat difficult to account for how it should be so seldom met with.

[To be continued.]

TREATMENT OF RHEUMATIC GOUT.

To the Editor of the Medical Gazette.

Sir,

Knowing with what readiness you at all times lend your pages to serve the profession, and, through them, the public at large, I venture to trouble you with the accompanying account of a valuable remedy for that most troublesome of all complaints, "rheumatic gout." In addition to the testimony of my much respected friend, which is founded on the most extensive and well-digested experience, I also beg to add mine, as I have tried it with perfect success.

I am, sir,
Your obedient servant,
John Propert.

New Cavendish Street,
20th June, 1838.

My dear sir,—In compliance with your wish for a fuller account of my mode of treating rheumatic gout, I proceed to give you all the details which are necessary to insure success.

Let me premise, that of all the names given to this malady, I deliberately prefer the one just stated. The other terms which have been adopted seem either imperfect, or else imply more or less of theory; while that which I have selected simply expresses the combination of the symptoms of two well-known diseases. My own opinion is, that it belongs to neither of them, but that it is completely *sui generis*. On this, however, I do not dogmatize. It may not be uninteresting to state that in early life I lost by this terrible disease (in the usual form of metastasis to the heart) a young friend, to whom I was most sincerely attached. My attention was turned to the subject earnestly, and I must own that the numerous victims to the same malady whom I have subsequently known, among the most promising youth of both sexes, have not

allowed that interest to diminish. The utter futility of bleeding in cases which seemed so obviously and so imperatively to require depletion, first tended to convince me that I was (as the sailors say) "on the wrong tack." I was then largely engaged in practice in a bleak open country, and the cases which came under my care were extremely numerous. I read every thing which could be procured on the subject, and went through a course of experiments under the guidance of the many writers who had treated of it. It appeared to me that all the modes recommended were equally inefficacious, or if in one or two cases the disease seemed to be alleviated and the duration shortened by the adoption of a certain plan, the next case precisely similar in appearance seemed utterly uninfluenced or perhaps aggravated by it. I had charge for many years of the poor of a very large parish in a well-managed workhouse, liberally supplied with every thing that could aid my efforts, and under my absolute control. In this establishment I first began a series of experiments—of conscientious experiments, I believe—though, as a young man, I was of course often misled by vague analogies and defective experience, and the imperfect medical education of that period. I speak of 30 years ago, when the sole object of a student was to make a figure in surgery, and medical knowledge was expected to come of itself. These experiments, as well as those in a large private practice, ended in total disappointment; and twenty years of subsequent active employment, of which fifteen were passed in London, only served to convince me that, except to alleviate a few urgent symptoms, or control casual complications, medical attendance was in almost all cases useless. I have also seen the disease treated by many of the most eminent practitioners in Europe, in the hospitals of many different nations, with a similar conviction of the importance of medicine. Out of the innumerable and contradictory remedies to which I resorted, those which alone seemed to have any decidedly beneficial effect on this disease, were three—vegetable alkali, camphor, and colchicum.

The *first* I adopted in consequence of observing the good effects of a popular cure for rheumatic gout, used in the western and south western counties of England, viz., the ashes of the bean stalk: large doses of potassa certainly tended to shorten the duration of the malady, and materially to hinder the formation of earthy deposits in the joints: in fact, the first time the remedy was mentioned I was constantly reminded of a fact which I had often noticed, *the intense acidity of the perspiration* in the disease, and, indeed, of other secretions, as the urine, and even the saliva. I had often observed, that scissars, and other articles of steel, and the fire-irons in the bed room even, were rusted in a short time, as if they had been subjected to the steam of vinegar. Bearing this in mind, it seemed natural and reasonable to expect benefit from alkalies. I am not aware that any writer on this subject has made a similar observation. I have forgotten from what strange analogy I was induced to try camphor, which, however, I gave largely, and with great relief to the pain, but with no permanent benefit to the patient. All the preparations of colchicum I used extensively, but their uncertainty, the impossibility of ascertaining beforehand the proper dose in each case, the formidable symptoms to which it occasionally gave rise, and the *real danger* to one person from a quantity which had no effect on another in a case strictly analogous, made me ultimately but reluctantly lose all confidence in this drug; and about ten years ago I had gone back in despair to my old plan of vegetable alkali and camphor. When I, soon afterwards, fixed my residence at Brighton, an opportunity was afforded me of putting my remedies to the test of a still more extended experience. Having built a large room at the back of my house, for the purpose of receiving patients gratuitously, I continued during many years to admit from a hundred and fifty to two hundred weekly. The whole was conducted with a system and regularity not exceeded in the best-managed public dispensary, and of every case a record was kept. Although in my printed papers I professed not to attend patients at their own houses, I made a special exception in favour of rheumatic gout, and as the sea-side is generally obnoxious to this ailment, I had thus a more abundant experience than falls to the lot of most men. The

statement of the result, then, surely deserves some confidence. The object of this detail of the mode in which my present satisfactory conviction became established, is solely to shew that my plan is not a mere lucky guess, nor hastily put forth on the accidental coincidence of a few successful cases (a common error with young practitioners), but that it is the conclusion formed after a long experience, under a great variety of circumstances. Some of my trials were made with very complicated remedies, too tedious to enumerate, but of which colchicum, in some form, was often a part. From the multifarious compound which was ultimately found to be most effectual, I gradually discarded one ingredient after another, till it was reduced to the simple and effectual remedy now offered to the public. The quantity and mode of administration have been varied in every way, and I rest satisfied that no further change is necessary or advantageous. On no one of the many occasions on which it has been used, have I seen the slightest injurious consequence, and I do not now hesitate to pronounce it the *most easily managed, the most universally applicable, the safest*, and the most *certain specific*, in the whole compass of our opulent Pharmacopœia; the mode of exhibition being an effectual regulator of its influence, adapting it accurately to the varying temperaments of different patients. Administered in the way I prescribe, and with the limitations stated, I feel quite confident that no one will be disappointed in the results to be expected from it. No doubt there may be complications of this disease, which may render other measures advisable: cases of plethora, or of inflammatory action, so decidedly marked as to leave no doubt of the propriety of taking away blood. It may be so; I have seen none such; at least none in which I have thought it necessary to use the lancet, though I see no disadvantage in erring on the safe side, and just relieving the tension of the blood-vessels by moderate depletion. *Large bleedings I believe to be always injurious;* they seem to me to aggravate the tendency to metastasis. The cases which require bleeding must be extremely rare, since not one has presented itself to me during the last six years. I am sorry to say, that when this disorder has gone on for some time, when it has been allowed to spend its violence on the articulations, and especially those of the vertebræ (the most frightful form of the disease), during a fortnight or three weeks, the remedy is by no means so effectual. Still it has a great influence in alleviating the pain and in shortening the duration of the malady, although *the transition from intense suffering to perfect ease* is less sudden and decided. I have uniformly remarked, that the more violent the attack the greater the number of articulations under its influence; the higher the fever, and the more general the disturbance, the more speedy and the more perfect is the cure. Were I to give details of some of the cases, I should not obtain belief, so much would the statement resemble a quack-doctor's advertisement. I will, however, venture to assert, that on many occasions I have seen the patient one day unable to turn in bed, with ankles and wrists so swollen and inflamed, and even the vertebræ affected to such a degree, that he has entreated me to walk gently, as the mere shaking of the bed produced an agony of suffering;—I have seen, I say, a patient in this state, and thirty-six hours afterwards not only perfectly free from pain, but able to walk across the room without assistance.

Treatment.—The powdered root of colchicum is, then, the specific on which I depend. It is an old remedy, but the mode of administration is new, and entirely my own. If the bowels be loaded, I begin by an enema of decoct. aloës, but this does not delay the use of the colchicum. The dose, eight grains every hour, taken in the medium most acceptable to the patient—plain water, sugar and water, apple tea, ginger tea, or other analogous fluids, changing them from time to time, even with the successive doses, if necessary, according as the stomach is more or less irritable, or the palate more or less capricious. The point of saturation, as I call it, (for want of a better term) is very uncertain, varying so much with different individuals, that although the usual quantity is eight or ten doses, I have known some take fourteen, and others unable to bear more than five. In every case it is to be repeated till active vomiting, profuse purging, or abundant respiration take place; or at least till the sto-

mach can bear no more. If a slight nausea comes on after three or four doses (I have never seen it so soon as the fourth), a quarter of an hour's delay may be allowed. A lump of sugar, dipped in brandy or eau de Cologne, a wine-glass of soda-water, or any thing else the patient wishes for, in small quantity, may be given. Sometimes a small slice of lemon kept in the mouth will turn away the nausea, and enable him to bear a few more doses; the main object in all cases being to get into the stomach the largest quantity that it can be induced to receive. Even two doses taken at once would be rejected by a patient, who will thus gradually bear a dozen. The most usual course of things is this. At the end of the sixth or seventh dose a slight nausea comes on. By keeping quite still, turning away the thoughts by conversation, or listening to an amusing book, coaxing the palate with a slice of lemon, a clove, or some such thing, three or four more doses can be received, when the disgust becomes perhaps unconquerable. After this there is generally sound sleep, with occasional nausea on waking. The pain ceases, but the more active effects of the colchicum do not take place for some hours after the last dose. Distressing as is the state of the patient when under the full influence of the medicine, it still does not exceed an ordinary sea-sickness; and when this has been endured for a few hours, it is succeeded by the Elysium. The inflammation of the joints subsides, and they resume their natural size with miraculous rapidity. The acidity of the perspiration ceases, as well as the peculiar odour, which enables the experienced practitioner to recognize the disease on entering the room, before he has asked a single question. As soon as a cup of souchong tea can be retained, a sound sleep comes on, from which the patient awakes perfectly well. When circumstances will admit of it, I prefer to give a breakfast of bread and butter and tea only, very early in the morning, and two hours afterwards commence the colchicum. No more food will be required that day, but tea may be given abundantly, with bread sopped in it, if required. It will be well to indulge the returning appetite very sparingly on the day following, on which, however, we may allow a small snap of devilled meat, and rice boiled plain as for curry, which will generally be the things most acceptable to the stomach. A small quantity of good curry itself is not objectionable to those who have been accustomed to that luxury. In the subsequent treatment I have no reason to think that any precaution is necessary. The patient may resume his ordinary diet as soon as the appetite dictates. I have never seen a relapse.

The colchicum, it is obvious, should be preserved with care. The best mode, I believe, is to grind it, at the proper season, with twice or thrice its weight of fine sugar, into an impalpable powder, when, should it accidentally become damp, it is safe from injury.

It is better, if possible, that the patient should not be aware of the direct effects expected until they take place, in order that the imagination may not anticipate and interfere with the process.

It would not be difficult to lay down a plausible theory on the nature of the disease, and *rationale* of treatment, quite as satisfactory as many others which have successively occupied attention, inspired confidence, and fallen into oblivion. Being, however, certain that my practice is right, I am unwilling to connect it with a theoretical explanation, which may be wrong; I might fail to convince by argument some who may be inclined to put faith in the simple assertion of the benefits I have witnessed from it. It would seem to most men to waste time to reason on a disease which is only cured by a specific remedy, whose mode of action is unknown. Many persons as well as myself have observed, that when the progress of discovery or of reasoning proves a theory to be false, the practice founded on it, or explained by it, falls into desuetude. Now it is obvious that the theory may be absurd, and the practice excellent, of which every man's personal experience must afford many examples; and as most men engaged in practice are anxious rather for results than for discussions, I will not run the risk of wearying attention.

I am, dear sir,
Yours very truly,
A. L. Wigan.

Brighton, June 1838.

CASE OF PTYALISM DURING PREGNANCY.

To the Editor of the Medical Gazette.

SIR,

I SHALL feel greatly obliged by the insertion of the following case in your valuable journal.

I am, sir,
Your most obedient servant,
JOHN GORHAM, M.R.C.S.L.
Fellow of the Physical Society
Guy's Hospital, &c.

225, High Street, Borough,
June, 1838.

Mrs. Davis, a lady residing in the neighbourhood of Horselydown, æt. 37. has generally enjoyed tolerably good health. She is the mother of three children, and with each pregnancy sick headache and salivation have troubled her. She states, that with her first child, after being pregnant about one month, she became affected with headache, and a large quantity of clear fluid, like saliva, was continually running into her mouth, so that sometimes two or three quarts were spat out during the day. At the expiration of the fourth month, that is to say after she had quickened, the salivation left entirely. During the second pregnancy precisely the same series of symptoms presented themselves, the secretion stopping immediately after quickening. The bowels were generally costive, and great thirst was complained of. No medicines were taken, for sickness prevented her retaining most things on her stomach. During this last gestation her old complaint had troubled her more than ever; it first appeared about a month after conception. Some days she spat out as much as *four quarts;* never so little as *two quarts.* The quantity averages, indeed, somewhere about *three quarts* daily. After this quickening a diminution took place: no complete cessation, however, was observed, and even during her labour a pocket-handkerchief was constantly used to absorb the fluid. Immediately after the child was born, the salivation ceased; no vestige of it remains, and she is now quite well in every respect.

I have thought it right to send this account to you, as some of your readers may be interested by it. The points which struck me as peculiar were the following:—The manifest connexion between the condition of the uterus and the salivary apparatus, as shewn by the presence of the salivation directly after pregnancy had become established, by the complete cessation of this symptom immediately on the termination of the fourth month, in two successive gestations, and by the same circumstance happening directly after the third labour.

The salivation was not produced by any therapeutical agent. The gums were not spongy, neither was the breath offensive. Lastly, the quantity of saliva excreted was excessive.

I may mention that I gave Mrs. Davis a mixture, consisting chiefly of magnesia and lime-water, when she was about four months gone with her last child, but not the slightest change was effected. I felt a strong inclination to administer small doses of mercury, according to the plan laid down by Dr. Blundell, but my patient was rather averse to medicine; and as a reasonable hope remained that her complaint would not last certainly longer than nine months, nothing further was done.

June 1838.

A CASE OF PLEURITIC EFFUSION; WITH REMARKS.

To the Editor of the Medical Gazette.

SIR,

I SEND you an account of a case of copious pleuritic effusion, which recovered without puncture, or any very active remedial measures, thinking you may perhaps consider it of sufficient interest for insertion in your useful periodical.—I am, sir,

Your obedient servant,
THEOPH. THOMPSON, M.D.
Physician to the Northern Dispensary, &c.

Keppel-street,
June 13, 1838.

W. R., a delicate boy, aged seven years, was brought to me on the 29th of August, 1837, with oppressed breathing, anxious countenance, and scarcely able to stand. On removing the dress, I found the left side of the chest remarkably prominent, and traversed by dilated veins, the shoulder of that side elevated, and the lower ribs apparently

motionless. The sound of the heart was heard more extensively than natural on the right side, and its pulsations were visible in the epigastrium. The respiratory murmur was inaudible; the sound, on percussion, dull over the lower two-thirds of the left side: œgophony was heard towards the upper third. At the apex of the right lung the respiration was puerile. At the upper part of the left, mucous rhonchus and bronchophony were observable. The pulse was quick and weak, the skin very dry, with troublesome cough; and occasional delirium.

On making close inquiry, it was ascertained that five weeks previously the boy had received a blow on the left side from a brick, but had not complained of any particular uneasiness from the injury; and until lately, when the breathing became impeded, his parents had not imagined that there was any thing wrong about the chest. An intelligent practitioner, who saw him five days since, ordered three leeches to the side, a dose of calomel and antimonial powder, and subsequently small doses of sulphate of magnesia, and antimonial wine. This medicine had produced some diarrhœa, and the tongue was very red. Under these circumstances I did not feel authorized to prescribe active measures, and supposing an operation inevitable, was satisfied with endeavouring to get him into a more favourable state for encountering it. I ordered ten drops of spirit of nitric ether, and about a drop of prussic acid (Scheele's preparation) three times a day, and directed fomentations to the chest.

Three days afterwards I was agreeably surprised to find not only that his general appearance was improved, the skin moist, and the quantity of urine increased, but that the sound of respiration was audible over a greater extent of the chest. The mixture was directed to be continued, and a blister applied to the left side. An ointment of iodide of mercury was also employed.

On the 7th of September, my excellent colleague, Dr. Stroud, saw the case during my absence, and adopted a plan which had previously been agreed upon if any more active measures should seem expedient; having had recourse to a mixture containing the acetate and hydriodate of potash, tincture of squills, and spirit of nitric ether. The decoction of wintergreen was likewise administered, and the blister was repeated.

Under the use of these more active diuretics, the urine became thicker and less abundant, the patient weaker and more irritable, the pulse quicker, and the sound of respiration more extensively dull. Dr. Stroud consequently substituted mucilaginous mixture and linseed-tea; and in a few days the boy was again better. A quart of urine was passed daily; the sound of respiration was more extensively heard, and œgophony was no longer perceptible. The mild plan of treatment was therefore continued, with occasional slight modifications, such as the employment of castor oil, the repetition of blistering, the addition of spirit of nitric ether to the mixture, and subsequently of carbonate of soda, on account of a scalding state of urine.

On the 29th of September, his strength being considerably improved, but some effusion still remaining, I adopted the suggestion of my colleague, Mr. Bishop, and ventured on the use of calomel, of which he took a grain twice a day for some time.

On the 16th of October, Dr. Edwin Harrison, who was kind enough to join me in examining the patient, agreed that the effusion was entirely removed. The ribs of the left side now moved freely; the measure of that side, from the sternum to the spine, was full two inches less than it had been six weeks previously, and the respiratory murmur was heard over the whole chest.

The following are some of the points which this case appears to me to illustrate:—

I. It shews how insidiously extensive effusion may take place into the pleural sac, and exhibits the necessity of vigilance in cases of injury to the chest.

II. It affords an illustration of the remarkable relation subsisting between the bowels and kidneys, through the influence of which active diuretics may sometimes frustrate the object for which they are given, and by irritating the mucous membrane, impair the strength, as well as diminish the quantity of urine.

III. It shows, that if the constitutional powers are husbanded, the absorbents, more especially in childhood, are capable of removing considerable quantities of effused liquid, independently of much assistance from medicine.

This rule is, perhaps, most applicable to cases of effusion from mechanical injury.

IV. It furnishes a suggestion, that although the effusion may be considerable, yet, if it is observed to diminish, and the strength is not much impaired, it is desirable to avoid recurring hastily to the operation of puncture; which is always hazardous, and may perhaps be superseded by the efforts of nature. If, however, the effusion is decidedly gaining ground, and active remedies are not well supported, doubtless the sooner the operation is performed the better.

Keppel Street, June 13, 1838.

REMARKS ON THE ACTION OF THE HEART AND ARTERIES.

To the Editor of the Medical Gazette.

Sir,

If you think the following observations of sufficient importance to claim a place in your valuable periodical, I shall feel obliged by their insertion at your earliest convenience.

I have the honour to be, sir,
Your obedient servant,
W. H. Madden, M.D.

Penicuik, Midlothian,
June 8, 1838.

It requires but little acquaintance with the phenomena presented by the animal body, either in the state of health or disease, to become convinced of the melancholy fact that there are innumerable circumstances, inseparable from the very nature of the subject, which have hitherto frustrated, and in all probability will continue to obstruct and render abortive, all our endeavours to reduce medicine to its just rank among the exact sciences. But there are also other causes which have exerted no small influence, and which might have been easily and effectually guarded against by a more strict adherence to the ordinary laws which regulate inquiries in other sciences, and a more rigid observance of the rules of logical deduction.

Among these retarding forces, if I may be allowed the expression, these heavy drags upon the wheels of progressive improvement, may, with the greatest propriety, be reckoned the common, nay, almost universal, employment of certain indefinite terms, which, founded originally upon some vague, perhaps utterly groundless, hypothesis, have yet, by frequent use, come to be considered as explanatory of some of the most complicated and difficult questions that concern our art.

Of these peculiar and very convenient phrases, the well-hackneyed expressions "vital principles," "sympathy," "efforts of nature," "lost balance of the circulation," "disturbed equilibrium *," &c. are notable examples.

It would be an endless task, one at least that could never be accomplished within the ordinary limits of a communication, to examine in detail the countless host of these expressions, and endeavour to analyse their true meaning, where such exists; and I shall, therefore, in the following paper, confine my remarks to the consideration of one only, "the action of the heart and arteries," a term to be found in almost every page of some of our most valuable practical works—a term which is used indiscriminately upon all occasions, which is constantly enumerated among the means of diagnosis, is often appealed to as a regulator of practice, and not unfrequently quoted in explanation of the nature of disease. We might be naturally led to imagine that an expression of so great importance, of so wide a range of application, should at least have some definite and fixed meaning—one upon which all men are agreed, and the full extent and bearing of which might be easily comprehended by all who meet with it. But in the progress of our inquiry we shall find that such is far from being the case; that those who are most conversant with the subject differ the most widely in their opinions, and that uniformity of ideas on this matter is only entertained by those who have never fairly looked upon the question as in its true bearings. Let us suppose a student, with notions hitherto unbiassed in any one particular direction, to com-

* Professor Hamilton, in endeavouring to explain the *modus operandi* of his very admirable plan for inducing premature labour, gives it as his opinion that it acts by destroying "the equilibrium between the contents and the containing parts of the uterus." (Vide Pract. Observations, Part II. p. 178.) What the learned Doctor means by this expression, or how, even supposing the existence of such an equilibrium to be established, the mere separation of a small portion of the membranes from the cervix uteri could destroy this equilibrium, I am at a loss to understand.

mence the perusal of a work on the practice of medicine or surgery: before proceeding far, he will meet with instances of disease which are said to depend upon, or be characterized by, "increased action of the heart and arteries;" and forthwith divers means will be recommended to remove this morbid condition. Before proceeding much further, he will be made acquainted with other maladies no less distinctly marked by "diminished or enfeebled action of the heart and arteries;" and again, with others whose distinguishing trait consists in "irregular action of the heart and arteries." Seeing all this, and struck with the great importance of these organs, he will naturally, if at all gifted with an inquiring mind, be led to ask, "In what consists their actions?" The question, he will find, is far more easily put than answered, or at least the answers he will receive will be so vague and contradictory, that he will experience no slight difficulty in arriving at a satisfactory conclusion; and unless of a naturally persevering and sceptical disposition, will, in all probability, be led into the dangerous error of taking mere expressions as an explanation of facts—an error so fatal to all chances of improvement. It is with the hope of being able in some measure to remove this obstruction, and to give greater certainty to our notions upon this subject, that I have been induced to submit the following remarks to the notice of the readers of the MEDICAL GAZETTE.

The opinions of physiologists concerning the forces engaged in circulating the vital fluid* may be conveniently divided into the following:—1st. That the heart is the sole moving power. 2d. That the small arteries and capillaries are the prime agents, the heart exercising merely a subordinate influence. 3d. A modification of these two preceding. It will be our endeavour to ascertain, as far as possible, in which theory the truth lies, or whether it exists in any; and for this purpose it will be necessary to examine particularly the actions of these separate organs.

* By the term vital fluid, I mean one which is necessary for the continuance of those phenomena to which we give the name life; not one possessed of any independent vitality, as some have imagined.

The distance to which the heart extends the influence of its contractions will be noticed hereafter: I shall in this place merely allude, in a few words, to its *active* power of dilatation, of which many have spoken, and which some have believed to accelerate in no small degree the venous circulation. I am not aware that either experiment or observation will warrant the opinion that the relaxation of muscular fibre is any thing more than a mere passive change, a return to the natural state of rest, or that it can in any case become a generator of power; and in regard to the organ now under consideration, the ingenious experiment of Oesterreicher* is perfectly conclusive; I allude to the one in which he laid upon the heart of a frog removed from the body a weight sufficient to press it flat, and yet not conceal its motions, and found the weight raised during the systole, but unmoved by the diastole. Indeed, even in the absence of such evidence, the extreme thinness of the auricular parietes would have led to the same inference.

But direct experiments have been quoted in proof that such a power does exist, or, at least, that the walls of the heart are endowed with a species of elasticity by which a tendency to the formation of a vacuum is produced; and of these, perhaps the most important are those performed by Wedemeyer and Guenther, who introduced a curved tube into the jugular vein of a horse, between a ligature placed round that vessel and the heart, and observed coloured water, into which the opposite extremity of the tube was plunged, to be raised two or more inches at each pulsation†; because in this case care appears to have been taken to distinguish between the effects of respiration and those which might be attributed to the heart alone—two circumstances which have been frequently confounded. If this result were uniform and invariable, it would undoubtedly shew that the heart does indeed possess a suction power (to what extent this might operate in the living body is another question); but it will be seen immediately, that the same or analogous experiments have altogether failed in the hands of other inquirers. Thus,

* Oesterreicher. Lehre vom Kreislauf des Blutes. Nürnb. 1826. P. 33.

† Vid. Müller's Physiology, by Baly, p. 234.

Messrs. Ellerby and Davies * have shewn that when a tube of this nature was introduced for two or three inches only into the jugular vein, no rise of fluid was produced; and Macfadyen has borne witness to the same fact †. Dr. Williams compressed the venæ cavæ, the inferior as near the diaphragm as possible, the superior a little above its entrance into the auricle, but no effect indicating a sudden extraction of the blood from this isolated portion could be perceived ‡. Messrs. Ellerby and Davies § have confirmed this observation, and shown further, that when part of the vena cava in the immediate vicinity of the chest was emptied, and pressure then applied close to the heart, it rapidly filled from below, although, of course, no suction power could operate in this case. These facts, and they might be greatly multiplied, are I apprehend quite sufficient to shew that this suction power is either a chimera, or, if it does exist, can exercise a scarcely appreciable influence over the circulation. In fact, all known principles of hydraulics are quite opposed to the idea that a fluid could be thus drawn through any thing but rigid pipes, and from such the veins most essentially differ.

Dismissing the heart, therefore, in the belief that it acts only as a forcing-pump of most admirable construction, we proceed to consider what are the powers which the arteries possess, and what share they take in the general movement of the circulating fluid. Of the three coats which anatomists have concurred in assigning to these organs, the external or cellular, and the internal or serous, may be left out of the question, as contributing merely to increase the strength of the vessels, and afford a smooth well-lubricated surface, over which the fluid may glide; it is with the middle that we are now principally concerned. This consists of pale straw-coloured, highly elastic, but brittle fibres; coiled obliquely round the circumference of the vessels, but not forming complete circles. Much, and, as it appears to me, most useless discussion, has been engaged in, regarding the identity of these fibres with muscular tissue; both those who wish to prove that they possess the vital power of contractility, and their opponents, most unnecessarily and incorrectly assuming that this identity is essential, while they have overlooked the fact that there are many organs whose contractility is undoubted, and in which, nevertheless, muscular fibres have not been shown to exist. The truth is, that as far as chemical and mechanical qualities are concerned, and in anatomical structure also, they differ much from ordinary muscular fibre, and it is therefore highly unphilosophical to call them by that name. But this does not affect the point at issue; the question, viz. that of their contractile power, must be determined by an appeal to experiment; and in this respect, though the evidence is somewhat conflicting, sufficient data are not wanting to form a definite conclusion. The experiments of Verschuir, Thomson, Wilson Philip, Hastings, Kaltenbrunner, Wedemeyer, and many others, showing that the application to the arterial coats of, not only various chemical agents, which are liable to a certain degree of fallacy, but also of mechanical and galvanic stimuli, is followed by distinct and visible diminution of calibre in the vessel, are sufficient to shew that such a power does really exist, and are not to be set aside by the negative results obtained by Nysten, Bichat, and Müller; because, in all physiological investigations, a *positive* is of far more importance than a *negative* observation; and because, as Dr. Thomson has well remarked, there are many causes of failure in experiments of this nature which are too frequently overlooked, more especially the great length of time which often intervenes between the application of the stimulus and the production of the desired effect. Of the correctness of many of the experiments above alluded to, I am convinced from personal observation, and I therefore assume that the arterial trunks, as well as their capillary terminations, are endowed with a true contractile property, in addition to that of elasticity. We have now, then, to inquire what effect this may have on the circulation?

It will be evident to any one who carefully reflects upon the subject, that the general contraction of a vessel throughout the whole of its extent, will,

* Vide Cyclop. of Anat. and Phys., "Circulation," by Dr. A. T. Thomson.

† Edin. Med. and Surg. Journal, xxii. 276, note.

‡ Edin. Med. and Surg. Journ. xxi. 270.

§ Loc. cit.

instead of accelerating, diminish the velocity of the fluid passing through it, by increasing the obstacles which the heart has to overcome. Alternate contractions and relaxations, or a kind of vermicular motion, would be necessary, in order to render any effectual assistance, and such a movement has never been satisfactorily established. For although Dr. M. Hall imagined that he had discovered a pulsating artery in some cold-blooded animals, it has been shown by Müller*, that in this he committed an error—the vessel described by him belonging to the lymphatic, not the sanguiferous system; and the arguments drawn from comparative anatomy, as, for instance, the pulsation of the dorsal vessel in insects, are totally inapplicable; for in these animals, there being no heart, the vessels are in its place endowed with those powers which it alone exercises in more perfect beings.

What effect, then, does this contractile property, for which we have been contending, produce? It would appear that, in the normal state of the system, it acts principally by accommodating the vessels to their contents, and by gradually converting the intermittent motion which is communicated to the blood at each successive impulse of the heart, into a uniform and equable flow. The truth of this opinion is illustrated by the different manner in which blood flows from an artery of considerable size, and one of very small calibre; and by the well-ascertained fact, that in cases of great general relaxation and weakness, the blood in the capillaries, as viewed in the transparent parts of animals, may be seen to move with an intermittent velocity; and that in some more extreme instances the same pulsatory movements are even continued into the veins. But it is also extremely probable that, at certain times, and under peculiar circumstances, one part of the system may contract, while another is dilated; and that many irregular distributions of blood, many cases of local congestion, perhaps also some of inflammation, may be in a great measure, if not entirely, produced in this way. I am well aware that there are difficulties in this view of the subject which are not easily overcome; that the altered products of inflamed parts can scarcely be explained by simple stagnation of the blood, and am willing, in the absence of a better explanation, to admit that there may be an alteration in the attraction or reciprocal influence, whatever that may be, exerted between the containing solids and their fluid contents. But this does not affect the nature of my argument. I merely contend for the possibility, nay, the probability, that these irregular contractions of limited portions of the vascular system may be taken as, in some measure, explanatory of what are commonly called local determinations, or congestions of blood.

If this view be correct, if the arteries and their capillary terminations exert only a modifying influence on the circulation, and in no way assist in propelling the vital fluid, it is evident either the heart must be considered the sole agent, or the cause of movement must be looked for in the blood itself; for the veins, excepting in the immediate neighbourhood of the heart, are totally devoid of any property beyond simple elasticity. With regard to the spontaneous movement of the blood, the subject is far too extensive and complicated to be noticed here. I may, perhaps, at some future time, devote some space to its consideration; but I may now state, that as far as our present knowledge extends, it appears to me far more consonant with reason and observation, to believe the heart the chief, if not the sole mover. This opinion is grounded upon the results of numerous experiments, but more especially that most conclusive one of Magendie's, who found, that when all circulation through the limb of an animal was confined to the principal artery and vein, pressure applied to the former immediately arrested the flow of blood through the latter: this experiment may at any time be verified, during an ordinary venesection): and the no less satisfactory observations of Hales*, and Voiseuille†, the former of whom found, by introducing tubes into the larger veins of the horse, that the *vis a tergo* was sufficient to raise the blood above the level of the heart; while the latter, by employing an apparatus of peculiar construction, showed that there is little or no difference in the force of the blood in arteries of very different size; and

* Loc. cit. p. 203.

* Statical Essays.

† Magendie; Journ. de Physiologie.

proved also, that this pressure is continued into the great venous trunks.

Having thus completed our brief, and of necessity imperfect sketch, of the agents concerned in effecting the circulation, we have now to consider how far, and in what cases, the term "action of the heart and arteries" may with propriety be employed. If the views we have advocated be correct, it will be at once evident that in the great majority of instances in which this term has been used, the heart alone was concerned in producing the phenomena. For although, as we have seen, the arteries may have considerable influence in regulating the distribution of blood to various parts, the effects of this action can only be determined by a reference to the *local* changes produced, and are not appreciable by an examination into the state of the *general circulation*, of which alone the pulse is an index. It is quite possible, however, that the hard and incompressible pulse, which so frequently accompanies inflammatory affections, may be caused not only by increased action of the heart, but also by an augmented degree of that vital contractility which we have shewn the arterial coats to possess, and therefore the term may be applicable here, though in a very different sense from that in which it is usually employed. Great softness of the pulse may in like manner be the effect of a diminution of this power, and an indication perhaps of the general relaxation of the system, but irregularity of its movements can depend upon the heart alone, and the term, therefore, as applied here, is not only useless, but absolutely incorrect.

Seeing, then, that such is the case; that the instances in which this expression is strictly proper are so few, and moreover so unimportant, that it can but rarely, if ever, assist in our diagnosis, or point out the means of cure; it would be well if it were altogether abolished from medical writings, not only as being unnecessary, but positively injurious. In a science so complicated as that of medicine, the employment of superabundant terms, even though not in themselves objectionable, is much to be deprecated, as tending to increase, rather than diminish, the unavoidable difficulties with which the student must contend. But where these terms are calculated to originate false ideas, to induce a belief in the existence of powers of which there is no real evidence; when they are brought forward in explanation of the phenomena of disease, and appealed to as regulators of practice, the injury which they are likely to produce is incalculable. For not only do they arrest the advancement of science, by distracting attention from doubtful points, but they tend also to foster that spirit of careless indifference which takes things for granted without ever examining the proofs; and which, in order to escape the labour of patient investigation, will rest contented with any solution, however imperfect, of those difficulties with which our art too commonly abounds.

MEDICAL GAZETTE.

Saturday, June 30, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

MEDICAL ATTENDANCE ON THE POOR.

DR. GOOCH, in those admirable letters on the subject of medical attendance on the country poor, which appeared in our first volume*, observes, that those who live at a distance from towns and cities well know the deplorable medical attendance which the poor receive in sickness. Those, on the contrary, who live in large towns, "are generally near some hospital, infirmary, or dispensary, where they find as good medical attendance for nothing, as those above them in society can procure for money—less ceremoniously administered, it is true, but in all essential respects as good." If we could coincide in the opinion of this benevolent physician, that the assistance tendered at dispensaries was equal, in essential points, to the aid afforded in private practice, and differed from it only by its less ceremonious administration, we should

* MED. GAZ., vol. i., pp. 55 and 151.

think that little more remained to be done for the medical relief of the poor in towns, and that the efforts of the philanthropist to amend the system must henceforth be confined to thinly-peopled districts. Unfortunately, however, the extreme negligence and inefficiency which are associated with the very name of a dispensary, make every one who has been practically acquainted with the working of such an institution, turn with eagerness to any substitute which presents itself.

The cant of the day, among a large class, is, "leave the poor to their own resources." This theory will make many converts. When the lady, in the Idler, came down into the country with the new doctrine that it was little less than criminal to teach poor girls to read and write, she was at first warmly opposed; but her example and arguments daily gained ground; for "few," says Dr. Johnson, "listen without a desire of conviction to those who advise them to spare their money." The first resource to which the lower classes may be left, is that of calling in the practitioner of their own choice, when they happen to fall ill. The poor man, having no ready money, and not much credit, feels a natural reluctance to calling in a doctor whom he has no very speedy prospect of paying; and the first consequence of obeying the Malthusian cry, and leaving the poor to their own resources, is, that illnesses are neglected until, by lapse of time and the continuance of bodily exertion, what was trivial becomes dangerous, and what was dangerous becomes incurable. Supposing the artizan or labourer to be less bashful, and to demand the services of a medical practitioner in the beginning of his illness, with the quiet self-possession of a genuine paying patient, this inconvenience will be got over, but the ultimate disappointment of the practitioner will be the same, or greater; until, indeed, by constant repetition, disappointment itself dies out, and the Spitalfields apothecary learns the difference between the patient who pays only in theory, and the one who does so in practice!

Mr. Holland, whose pamphlet* would have pleased us much better had he not too often fallen into the cold dialect of the starvationists, has quoted a good illustration of this point from our fifteenth volume. A general practitioner, living in a densely populated district, sent out his bills at Christmas, chiefly among the petty tradespeople and artizans, to the amount of 800*l.*; of this he had received, the following Midsummer, 130*l.*, and did not expect to get more than another 100*l.* with all the aid of dunning and the law! If we subtract from the sum received and hoped for, what was paid by his few substantial customers and the petty tradespeople, we should probably find that the working classes paid him something like half-a-crown in the pound, and this with the aid of constant mnemonic calls, and the occasional stimulus of lawyers' letters. Nor is this by any means a solitary instance. The practitioners of one of our north-eastern suburbs are so dissatisfied with their vain attempts to recover their debts, that their discontent extends even to the administration of justice itself—at the Kingsgate-Street Court of Requests, Holborn; and they publicly advertise, that Mr. Dubois, the deputy-commissioner, not being a medical man, is not a fit and proper person to examine medical men's accounts. Now with every disposition to sympathize with our brethren of Hoxton, in their mortification at being unable to recover their

* An Essay on Dispensaries; addressed to such of the subscribers as desire that what they give should produce the greatest possible benefit to those whom they wish to befriend: showing that, at a much smaller cost, a much greater amount of good might be effected, &c. By P. H. Holland, Surgeon, Manchester. 1838.

just debts, we cannot altogether enter into the spirit of their objection to Mr. Dubois. All that can reasonably be required of such a deputy-commissioner is, that he should have a sound knowledge of some parts of the law, and an intimate acquaintance with the ordinary forms of business and the relations of society; but if he ought to be a medical man, in order to weigh apothecary's bills in a just balance, for a similar reason he ought to be a tailor, baker, carpenter, glazier, and so on, *ad infinitum*. The Latins say, *ex quovis ligno non fit Mercurius;* but of what kind of wood must Mr. *du bois* be made, to allow of so many forms being carved upon him? If it be answered that the apothecary's bill is, from its nature, of more difficult comprehension than those of the baker and tailor, and that a professional eye alone can see at a glance what necessities led to the items of which it is made up, we would reply, that the difficulty now experienced by the deputy-commissioner would be felt, under the sway of a Galenical judge, by the defendant and the bystanders; and that if Justice were to put on medical robes before she held the scales, it would be said that they turned with none but apothecaries' weights. The nature of the court will sufficiently account for the difficulties experienced by our brethren of Hoxton, without imputing any especial partiality to the deputy-commissioner. It is well known that in the Courts of Requests the majority of defendants are extremely poor, and, consequently, the judgments of the courts are very lenient. The defendant acknowledges the debt, and is ordered to pay half-a-crown a week till it is discharged; but he pleads that he is *Iro pauperior*, and has not seen half-a-crown these three weeks. The order is then mitigated to eighteen-pence, which he pays for a fortnight, and then changes his lodging. The young and baffled doctor attempts to ascertain his new abode, blames the shameful clemency of the Court of Requests, and determines to petition the legislature; but he grows wiser in time:—

"Lenior et melior fit accedente senectâ;"

he finds that he cannot "make a silk purse out of a sow's ear," nor a profitable patient out of a Spitalfields weaver.

It was, no doubt, partly in order to put an end to these petty legal suits (or rather squabbles), and the painful hunting down of indigent patients, that Dispensaries were first instituted. Their benevolent founders must constantly have seen the poor man exposed to legal proceedings if he did not pay his bill, and reduced to extreme distress if he did, and they endeavoured to relieve him from this urgent dilemma. Mr. Holland says, with admirable *naïveté*, "the best possible proof that patients procure assistance from these institutions who can pay for it, is the fact that before they were established they did so." As if it was a thing unexampled in the history of man, that the amount of a doctor's bill might be squeezed out of an income already insufficient to procure the comforts or even the necessaries of life!

But Dispensaries of the ordinary kind are infested by two kinds of evils. In the first place they are sometimes abused by persons in easy circumstances, who are not ashamed to take advantage of what was destined for the poor alone. That this is an evil we have always been ready to acknowledge, but to what extent it exists is not so easy to ascertain. Mr. Holland, indeed, would make it out to be the rule instead of the exception; but then Mr. Holland, as we have already hinted, in spite of his better feelings, talks very much like a partisan of the new Poor Law. Now one of the favourite maxims of our modern anti-Epicureans is,

that no income can be too scanty to afford the necessaries of life—and leave something to spare; so that any body may live on bread and water-gruel, and pay his doctor out of the surplus. Hence the result of personal inquiry has made Mr. Holland believe that at the Chorlton-upon-Medlock Dispensary "nearly two-fifths of the person supplied there *gratuitously* with medical assistance have larger weekly incomes than those who are considered too well off to be admitted as as *free members* at the Derby Self-supporting Dispensary." Now what are the incomes which make people too rich to be admitted to the Self-supporting Dispensary at Derby? Why, if a man and wife have 14s. per week, they are too opulent to be allowed to belong to this health-insurance society! How a man and wife with 14s. a week can be thought too rich for the Self-supporting Dispensary, and how in the name of common sense it can be supposed that out of their scanty funds they are to pay doctors' bills, surpasses our imagination, or at least would have done so some five years since. At present, we believe the solution of the riddle to be, that they are intended to live on diet resembling the six schemes of fasting, called Dietaries, No. 1, 2, &c., set forth by the dispensers of "the boon to the poor;" and by such contrivances something may possibly be put by from their meagre revenue. That there are places, however, where those who frequent Dispensaries are in independent circumstances, is but too clear. According to Dr. Collins, at Liverpool patients sometimes drive up to the Dispensary in their gigs.

In the second place, the extreme mismanagement of ordinary Dispensaries is with us a stronger, because a more commonly applicable objection, than the one just discussed. The badness of the drugs, the carelessness with which they are dispensed, and the maundering, slovenly manner in which the whole business goes on, are enough to disgust any one who does not take his opinions from reports and speeches, but has been behind the curtain, and seen the actors with their paint off. In our next article, we will discuss the remainder of this subject.

ANIMAL MAGNETISM.

We stated last week that some gentlemen, most of whom are well known as men of science, had formed themselves into a committee to examine the subject of animal magnetism. They began by making some experiments, or rather observations, on O'Key, the chief performer at University College Hospital; and being desirous of guarding against all sources of fallacy, made it a condition that none of those more immediately concerned in the recent exhibitions in Gower Street should be present. On this being made known, Dr. Elliotson prohibited her further attendance, requiring that either Mr. Wood or himself should be present. As this was not agreed to, the investigation has been for the time interrupted.

UNIVERSITY COLLEGE HOSPITAL.

A SPECIMEN OF MESMERISM.

With the mesmeric phenomena of O'Key, exhibited under their most favourable aspect, the readers of this journal are already familiar through the medium of Mr. Mayo's papers; but as we conceive it to be our duty to make them acquainted with all the passing events connected with medicine, we shall subjoin a specimen of a different kind—one which we believe to be perfectly faithful, as we are sure it is unique in the annals of science. It is taken from the Lancet of last week; and if our readers participate in the disgust with which we ourselves perused it, the blame be with

those who have given importance to these displays, and who regard such maniacal and loathsome ravings as fit subjects for exhibition and record. This same Miss O'Key had predicted (Save the mark!) that on a particular day she was to do something very striking; and she kept her word.

"A piece of paper was thrown to her; she tore it into fragments with her hands and mouth. 'O'Key! O'Key!' Dr. Elliotson said. 'Leave me alone, you villain, do,' she exclaimed, darting towards him with the ferocity of a tiger. (Her voice was full, sepulchral, and resonant, having the depth and force of a powerful adult voice.) 'B——t you, get away. Come a'near me, that's all.' The apothecary entered the ward, and said to her, 'O'Key, what's the matter with you?'—'D—— you, get away, do,' she replied, turning, by the paw-like action of her left leg, in the direction of the voice. She continued, in the intervals of darting at those who approached, to tear into the minutest portions the bits of paper that were scattered around, then venting her rage on a piece of yellow linen, which, on being thrown to her, was met with a 'B——t you, what do you mean by that?'—'O'Key, don't tear it, throw it away.'—*O'Key* (with tremendous voice). 'What are you doing with me? B——t you, leave me alone.' Dr. Elliotson tried to 'mesmerise' her. The process had no result.—*Mr. Wood.* 'Where's the negro, O'Key?'—*O'Key.* 'You b——d fool, be off.' Efforts to soothe her, and 'mesmerism' again tried, with a chain of persons attached to the operator, had no effect.

"Mr. Wood proposed to get her out of this state by some proceeding. At twenty-eight minutes past five o'clock, Dr. Elliotson and one of the visitors each seized an arm as she sat on the floor, and Mr. Wood, from behind, placed one of his hands on her forehead, and the other on the back of her head. She did not at all approve of this, but tried to bite the parties, exclaiming, as she failed, 'D——n your b——y eyes, come here.'"

There are several pages in the same strain, but we presume our readers have had enough, and we shall therefore only add, that Miss O'Key went on to say to Dr. Elliotson, "I never saw such a d—d fool in my life;" after which he proceeded to demonstrate to the pupils of University College Hospital the "influence of magnetized water."

ROYAL COLLEGE OF SURGEONS, IN LONDON.

NEW REGULATIONS.

Regulations of the Council respecting the Professional Education of Candidates for the Diploma, after the termination of the Session 1839-1840.

I. Candidates will be required to bring proof—

1. Of being not less than twenty-one years of age.

2. Of having been engaged in the acquirement of professional knowledge for not less than four years, three of which shall have been passed in a recognized school or schools of surgery; three months of vacation being allowed in each year, and in the event of absence or absences from such school or schools during the said term of three years (exclusive of the vacations,) the full term being completed in a subsequent year.

3 Of having studied anatomy and physiology, by attendance on lectures and demonstrations, and by dissections, during two anatomical seasons.*

4. Of having attended at least two courses of lectures on surgery, delivered in two distinct periods or winter seasons of six months, each course to comprise not less than seventy lectures.

5. Of having attended one course of lectures on the practice of physic, and one on chemistry, during six winter months, comprising not less than seventy lectures respectively; one course on materia medica with medical botany during six months, and one on midwifery during six months, each comprising not less than sixty lectures; and, at least, twenty-five lectures on medical jurisprudence.

Certificates of attendance on these lectures during the summer season will be received, provided they are equally divided over the period of four months.

The lectures on medical jurisprudence may be delivered three days in the week.

6. Of having attended, during twenty-one months, the surgical practice of a recognized hospital in London, Dublin, Edinburgh, Glasgow, or Aberdeen; or for twelve months in any one of such hospitals, and twelve months in any recognized provincial hospital.

7. Of having attended the medical prac-

* An anatomical season is understood to extend from October to April inclusive, and to comprise at least 140 lectures on Anatomy and Physiology, occupying not less than one hour each, given in separate days; and at least 100 demonstrations of the like duration, given in a similar manner; exclusive of dissections, of which distinct certificates are required.

tice of an hospital or dispensary during six months.

II. Members and licentiates in surgery, of any legally constituted College of Surgeons in the United Kingdom, and graduates in surgery of any University, requiring residence to obtain degrees, will be admitted for examination on producing their diploma, license, or degree, together with proofs of being twenty-one years of age, and of having been occupied at least four years in the acquirement of professional knowledge.

N.B.—Certificates will not be recognized, in future, from any hospital unless the surgeons thereto be members of one of the legally constituted Colleges of Surgeons in the United Kingdom; nor from any school of medicine or midwifery, unless the respective teachers be members of some legally constituted College of Physicians or Surgeons in the United Kingdom; nor from any school of anatomy or surgery in England, unless the teachers respectively be members of some legally constituted College of Physicians or Surgeons in the United Kingdom, and have undergone a second or special examination on those branches of science, according to the ordinances relating thereto.

Certificates will not be received, in future, on more than one branch of science from one and the same lecturer; but anatomy and physiology, demonstrations and dissections, materia medica and botany, will be respectively considered as one branch of science.

In the certificates of attendance on hospital practice and on lectures, it is requested that the dates of commencement and termination may be inserted in words at length.

Blank forms of the required certificates may be obtained on application to the Secretary, to whom they must be delivered, properly filled up, ten days before the candidate can be admitted to examination; and all such certificates are retained at the College.—By order of the Council,

EDMUND BELFOUR, *Secretary.*

June 25, 1838.

ON FRACTURES OF THE CLAVICLE.

To the Editor of the Medical Gazette.

SIR,

PERCEIVING in the number of your GAZETTE for the 9th of this month some observations upon fractures of the clavicle, by Mr. Moseley, elicited by a letter of mine in a previous number of the same journal, I must again beg your insertion of the following when a convenient opportunity offers. Mr. Moseley has been pleased to designate such cases as the one to which I have alluded of rare occurrence; but as I must confess myself rather sceptical regarding the truth of this assertion, upon which in great measure depends the relative value of the symptom in point, I shall beg leave to state the reasons for my disbelief; and in order to do this more perspicuously, I intend to advert to Mr. M.'s sentiments, and attempt to shew that they are for the present valueless; and being aware that too great liberty is frequently taken with the sense of a writer, by distorting his sentences into various convenient forms subservient to the end of his opponent, I shall attempt to do this in fairness and honesty.

Imprimis, then; Mr. M. adduces a sweeping assertion of Sir C. Bell's, which goes to affirm, that a fracture of the clavicle never occurs without the knowledge of the sufferer, unless indeed he be in a state of inebriation; on account of the severe pain felt in the part, and the loss of motion experienced at the shoulder-joint; if this be true, how simple the diagnosis! What would be the utility of discriminating symptoms? But the worthy baronet seems afterwards to afford a negative proof, invalidating the general application of this aphorism, for he enters into a minute description of all distinguishing marks; and subsequently cautions us that care must be taken not to confound fractures of the clavicle with *dislocations* of the shoulder. But waiving all inferences which might be drawn from this remark, when Mr. M. expresses his opinion of the rarity of this accident, allow me to remind him that he has offered no proof of this from his own practice, which has been perhaps regulated by the writings of those members of our profession, who, from their talents and experience, deserve in a great measure our implicit reliance. I would ask him if he has invariably examined the state of the clavicle, in all injuries of the shoulder supposed to be sprains; either from the patient's opinion of the non-occurrence of fracture, according to Sir C. Bell, or from his inability to raise the hand to the head, according to other authors, all suspicion of fracture being removed; if such be the case, and probability seems to countenance it, surely little or no proof can be found in Mr. M.'s statements.

In conclusion, I am afraid that your correspondent may have mistaken his own meaning, when he wishes to impress upon us the rarity of this accident, and that he

would more properly intend to refer to the infrequency in which the absence of this symptom (inability of raising the hand to the head) has struck the observation of medical men; and which has, I fear, led too often to mistakes, both in diagnosis and cure. But I would, in future, request him to notice this point more minutely, "*experientia docet*," and he may hereafter find its greater frequency, if he does not (wishing to render diagnosis more easy) trust "in toto" to a rule which seems, like many useful customs, persevered in rather for form than application; and thanking him for the courtesy in which he has conveyed his sentiments, I beg leave to remain, Mr. Editor,

Your humble servant,

OBSERVER.

June 11, 1838.

UREA IN DROPSICAL FLUIDS.

R. MARCHAND has detected urea in the fluid contained in the peritoneal cavity in three cases of ascites. In the first he found 0·42, in the second 0·68, in the third 0·50 per cent.; and it appears probable that there was much more, because the quantity of albumen in the same fluid rendered it difficult to prevent the urea being entangled in the coagulated masses produced by the agents used in extracting it. In all the cases very little urine was secreted; and in two of them there was the disease of the kidney described by Dr. Bright. He mentions, also, that two cases are given by Nysten, (*Journal de Chimie Médicale*, 1837) in which he found urea and uric acid, phosphoric acid, and several other constituents of the urine, in the fluid vomited by women labouring under ischuria renalis.—*Müller's Archiv.* 1137, part 4.

These cases, with those of Prevost and Dumas (*Bibliothèque Univers.* xviii. 208), who detected a considerable proportion of urea in the blood of dogs from whom the kidneys had been removed, by experiments which were confirmed by Vauquelin and Segalas, and by Mitscherlich, Tiedemann, and Gmelin (Tiedemann's *Zeitschrift*, vol. i.), sufficiently prove that the excrementitious matter of the urine is not elaborated by the kidney, but merely separated in that organ from the blood, in which it had before existed ready formed.

ANALYSIS OF HUMAN LYMPH.

MARSHAND and Colberg having met with one of those rare cases in which pure human lymph admits of being collected in sufficient quantity for chemical examination, have communicated the results of their analysis in the last number of Müller's Archiv. (No. 2, 1838.) The lymph was collected from a wound on the back of the foot, which obstinately refused to heal, a case exactly similar to that observed by Müller, (Physiologie, vol. 1, p. 244.)

The specific gravity of this lymph was 1·307: after resting a short time in a glass vessel, a web-like fibrinous coagulation formed, which when filtered, washed with ether, and dried, weighed 0·52 per cent. of the whole quantity. The fluid part gave a precipitate with alcohol, and tichloride of mercury, in the form of delicate flocculi. It was strongly alkaline. When dried in a water-bath at 97·5 Centizr. the lymph entirely coagulated from the albumen which it contained; heated to 100°, and kept for some time at that temperature, it formed a firm, powdery, grey mass, amounting to 3·074 per cent. of its weight, which when washed with ether, lost about 1-20th of its weight by the removal of that quantity of fatty matter. When the mass thus freed from fat was treated with boiling water, about one per cent. of albumen and fibrin were left undissolved; and the fluid being evaporated, left one-and-a-half per cent. of saline constituents.

They give, as the general result of their analyses of several portions of lymph, of which they could could collect about a grain and a half in 12 hours—

Water	96·926
Fibrin	0·520
Albumen	0·434
Osmazone (and loss)	0·312
Oily and Crystalline Fat	0·264
Muriate of Soda ——— Potass Alkaline Carbonates ——— Lactates Sulphate of Lime Phosphate of Lime and Iron	1·544
	100·000

MEDICAL SCHOOLS.

FROM catalogues lately issued, we note the number of matriculants in various medical schools, for the present sessions, to be as follows:—380 in the University of Pennsylvania; 227 in the Lexington Transylvania University; 122 in the Cincinnati Medical College; 100 in the Louisville Medical Institute; 82 in the Harvard University; and 80 in the Medical College of Ohio. From a report lately published, this latter institution seems to be in a prosperous condition.—*Philadelphia Medical Examiner.*

MONTHLY LIST

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns, June 19, 1838.)

	Price.			Duty.		Duty Paid.	
						In 1838 to last week	Same time last year.
	£ s. d.		£ s. d.		s. d.		
Aloes, Barbadoes, D.P. c	12 0 0	to	30 0 0	B.P. lb	0 2	52,756	36,246
Hepatic (dry) BD..... c	5 0 0		14 0 0	F. lb	0 8		
Cape, BD. c	1 16 0		2 0 0				
Anise, Oil of, German, D.P.... lb	0 9 6		0 9 6	F. lb	1 4	—	251
E. I. ..... lb	0 7 0		0 7 6	E. I.	1 4	459	650
Asafœtida, B.D. c	2 10 0		5 0 0	c	6 0	35	7
Balsam, Canada, D.P. ..... lb	0 1 3		0 1 4	lb	0 1	3,586	1,186
Copaiba, BD. ..... lb	0 7 6		0 8 0	c	4 0	55	120
Peru, BD. ..... lb	0 4 3		—	lb	1 0	1,468	346
Benzoin (best) BD..... c	25 0 0		50 0 0	c	4 0	61	70
Camphor, unrefined, BD..... c	9 0 0		—	c	1 0	283	281
Cantharides, D.P. ..... lb	0 5 0		—	lb	1 0	8,672	14,913
Carraway, Oil of, D.P. ..... lb	0 9 0		—	lb	4 0	521	807
Cascarilla or Eleutheria Bark, D.P.C.	1 15 0		—	lb	0 1	4,031	2,096
Cassia, Oil of, BD..... lb	0 7 0		0 7 6	lb	1 4	1,898	1,504
Castor Oil, East India, BD ..... lb	0 0 6		0 0 10	c	1 3	2,814	3,254
West I. (bottle) D.P. 1½ lb	0 2 3		—		—		
Castoreum, American ..... lb	1 15 0		—	lb	0 6	460	147
D.P. Hudson's Bay ..... lb	1 0 0		1 4 0				
Russian..... lb	none						
Catechu, BD. Pale c	1 5 0		—	c	1 0	11,583	16,194
Dark	3 0 0		—				
Cinchona Bark, Pale (Crown)..... lb	0 2 0		0 3 6	lb	0 1	60,325	94,933
BD. Red ..... lb	0 2 0		0 4 0				
Yellow ..... lb	0 2 3		—				
Colocynth, Turkey ..... lb	0 2 6		0 4 0	lb	0 2	4,918	5,067
D.P. Mogadore ..... lb	0 3 0		—	lb	0 2	17,389	8,812
Calumba Root, BD. c	0 12 0		1 15 0	lb	0 6	12,802	14,330
Cubebs, BD. c	5 0 0		—	c	4 0	53	47
Gamboge, BD. c	5 0 0		15 0 0	c	4 0	266	346
Gentian, D.P. c	1 4 0		—	c	6 0	13	24
Guaiacum, D.P..... lb	0 1 0		0 1 8				
Gum Arabic, Turkey, fine, D.P... c	8 0 0		9 0 0	c	6 0	2,857	1,626
Do. seconds, D.P. .. c	5 0 0		7 0 0				
Barbary, brown, BD. c	3 0 0		3 3 0				
Do. white, D.P..... c	4 15 0		—				
E. I. fine yellow, BD. c	3 0 0		3 10 0	c	6 0	2,315	1,024
Do. dark brown, B.D. c	1 15 0		2 5 0				
—— Senegal garblings, D.P. c	4 15 0		5 0 0	c	6 0	4,554	268
—— Tragacanth, D.P. c	8 0 0		12 0 0	c	6 0	254	190
Iceland Moss (Lichen), D.P. .... lb	0 0 2½		0 0 3	lb	0 1	5,179	2,546
Ipecacuanha Root, B.D. ..... lb	0 1 9		0 2 0	lb	1 0	9,259	6,010
Jalap, BD. ..... lb	0 2 4		—	lb	0 6	19,627	33,048
Manna, flaky, BD. ..... lb	0 4 0		0 5 6	lb	0 3	4,356	13,495
Sicilian, BD. ..... lb	0 1 7		—				
Musk, China, BD. ..... oz	1 0 0		1 8 0	oz	6 0	929	1069
Myrrh, East India, BD. c	5 0 0		14 0 0	c	6 0	73	77
Turkey, BD..... c	2 0 0		11 10 0				
Nux Vomica, BD..... lb	0 8 0		0 9 0	lb	2 6	556	838
Opium, Turkey, BD. ..... lb	0 14 6		—	lb	1 0	12,522	16,513
Peppermint, Oil of, F. BD..... lb	0 19 0		—	lb	4 0	437	515
Quicksilver, BD. ..... lb	0 3 6		—	lb	0 1	157,412	102,835
Rhubarb, East India, BD..... lb	0 2 6		0 3 3	lb	1 0	15,734	14,181
Dutch, trimmed, D.P. lb	0 3 6		0 4 0	F. lb	1 0	2,997	3,637
Russian, BD. ..... lb	0 8 3		—				
Saffron, French, BD. ..... lb	0 18 0		—	lb	1 0	2,082	2,841
Spanish ..... lb	0 19 0		—				
Sarsaparilla, Honduras, BD..... lb	0 1 0		0 1 9	lb	0 6	53,613	55,898
Lisbon, BD. ..... lb	0 2 0		—				
Scammony, Smyrna, D.P..... lb	—		—	lb	2 6	4,089	4,795
Aleppo ..... lb	0 12 0		0 15 0	E.I. lb	0 6	31,498	48,747
Senna, East India, BD. ..... lb	0 0 3		0 0 4				
Alexandria, D.P..... lb	0 1 9		0 1 10	Other sorts	0 6	34,681	34,805
Smyrna, D.P. ..... lb	0 1 0		0 1 3				
Tripoli, D.P. ..... lb	0 1 0		0 1 3				

‡§‡ BD. In Bond.—c. Cwt.—B. P. British Possessions.—F. Foreign.—D. P. Duty paid.

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

THE following circular has been sent to us for insertion:—

SIR,—We beg leave to inform you that the Sixth Anniversary Meeting of the Provincial Medical and Surgical Association will be held at Bath, on Wednesday, the 18th, and Thursday, the 19th of July next, and it will afford the members of the Association much gratification if you will honour them with your presence on that occasion.

The general meeting of the Association will be held at the Lecture-Room of the Bath Royal Literary and Scientific Institution, on Wednesday evening, July the 18th, and the President (Dr. Barlow) will take the chair at half-past 7 P.M. The Address of the President will then be delivered, the Report of the Council read, and other necessary business transacted.

On Thursday morning, at 9 o'clock, the members of the Association and their friends will breakfast together, at the Pulteney Hotel, Sydney Gardens.

At two o'clock the members and their friends will assemble at the Literary and Scientific Institution, when the Retrospective Address will be read by Dr. Malden, of Worcester, and the general business of the Association transacted.

You are requested, on arriving at Bath, to repair to the Literary Association, and to enter your name and address in Bath, in a book which will be there provided by Mr. Silverthorn, the Librarian of the Institution. Mr. Silverthorn will also give every necessary information respecting the progress of business, so as to obviate all confusion.

On Thursday evening, at five o'clock, the members and their friends will dine together at the Town Hall. Dinner and dessert, exclusive of wine, ten shillings and sixpence. Dinner tickets, with cards specifying the prices of wines, may be had from Mr. Silverthorn, at the Institution.

At nine o'clock in the evening of Thursday, the members and their friends will hold a conversazione, and every arrangement will be made with a view to promote friendly intercourse, and to facilitate the communication between members who arrive from distant parts of the kingdom.

Should you intend to honour the Association with your company at dinner, you are requested to signify your purpose either to Dr. Barlow, or any other member of the Bath Council, on or before Monday, the 16th of July, such notice being absolutely necessary for ensuring adequate preparations being made. We take leave to remind you that those members who have not an opportunity of paying their subscriptions at the Bath meeting, or to a member of the Council resident in their district, may pay the amount through the medium of their own bankers to Messrs. Robarts and Co., London, for Messrs. Berwick and Co., Worcester, on account of the Provincial Medical and Surgical Association.

As the subject of vaccination and small-pox, owing to the recent prevalence of the latter disease in more than an ordinary degree throughout the kingdom, is become more than usually important, the Council would feel much obliged by your sending any information thereon, addressed to either of us at Worcester, on or before the 10th day of July next.

[A series of queries is appended, which we are obliged to omit for want of space.]

We remain, sir, your's very faithfully,

C. HASTINGS,
J. P. SHEPPARD,
Secretaries.

Worcester, June 6, 1838.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *June* 26, 1838.

Abscess	1	Gout	1
Age and Debility	24	Heart, diseased	1
Apoplexy	5	Hooping Cough	4
Asthma	7	Inflammation	8
Cancer	1	Bowels & Stomach	3
Childbirth	4	Brain	4
Consumption	28	Lungs and Pleura	3
Convulsions	11	Insanity	7
Dentition or Teething	5	Jaundice	1
Dropsy	3	Liver, diseased	2
Dropsy in the Brain	5	Miscarriage	1
Epilepsy	1	Paralysis	2
Erysipelas	1	Small-pox	21
Fever	20	Unknown Causes	30
Fever, Scarlet	12		
Fever, Typhus	6	Casualties	3

Decrease of Burials, as compared with the preceding week . . . 196

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ *W. of Greenwich.*

June.	THERMOMETER.	BAROMETER.
Thursday . 21	from 56 to 68	29·50 to 29·62
Friday. . . 22	52 67	29·74 29·87
Saturday . 23	47 71	30 04 30 05
Sunday . . 24	44 77	29·93 29·94
Monday . . 25	53 75	29·99 30 00
Tuesday . 26	55 61	29 94 29·92
Wednesday 27	55 65	29 94 Stat.

Prevailing wind, S.W.

Generally clear, except the 21st, 26th, and following day, when rain fell.

Rain fallen, ·775 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 7, 1838.

THE CROONIAN LECTURES,

For 1838 (*continued*).

By John Clendinning, M.D., F.R.C.P.

EXPERIMENTS AND OBSERVATIONS RELATING TO THE PATHOLOGY AND PATHOLOGICAL RELATIONS OF THE HEART.

When we look at Albertini's work, *De Cordis Palpitationibus*, or Lancisi's work, *De Corde*, the one within the former half of the seventeenth, and the latter within the former half of the eighteenth centuries, and compare their semeiologies or ætiologies with those of any well-informed physician of the present day, we at once perceive that great progress has been made since their times, and that more has been done for cardiac pathology within a few dozen recent years, than had been effected through all previous time. But much remains to be done, and the efforts of very many successive as well as contemporaneous inquirers, working under various circumstances, and with various methods and instruments of investigation, will be required, to raise the science and management of cardiac disease to a degree of perfection equal to that of the least perfect of the physical sciences or mechanical arts.

Disease, as well as health, has many and various attributes and aspects, which are not all to be examined successfully by any one method, or test, or instrument. Of the means of which the pathologist may usefully avail himself, some are strictly anatomical and physiological, but these are not all; the instruments in use in physical inquiries are many of them available in pathological investigations also. Amongst the rest, the balance is one of which I think too little use has been made, although an instrument that might be supposed to be of some utility, and is certainly of comparatively easy application. It is now some time since I first satisfied myself of its utility in observation, and of the extreme difficulty of accurate pathological investigation in numerous cases with the unaided touch and sight, and of the necessity of adding to our other means of examination, in certain organic diseases more especially, the mechanical means just named, for the purpose of correctly estimating the effects of disease upon the viscera, in influencing the mode and degree of their nutrition. The College will judge how far I have reasoned correctly, or have been deceived on the subject. I now proceed to state the results of my observations.

Development, &c. of the diseased heart.—The heart itself, of course, claims the first notice. Since I began to employ the balance, &c. in post-mortem examinations, I have had opportunity of inspecting the remains of from 170 to 180 cardiac subjects, dead of morbus cordis in most instances, of course, but in a considerable number of cases dead of other diseases, of which diseased heart was a cause or else a complication. These are from amongst a much larger number who have come under my notice within a few years, principally in official practice. Of these the majority—the great majority, indeed, but I cannot say the exact proportion—had suffered during life from symptoms by which they could easily be, by any experienced pathologist, recognized as examples of morbus cordis; but several died without its having been known before their deaths that there was any organic affection of the heart involved in their complaints. Some of the latter had been brought in moribund, others had had their cardiac symptoms masked by mania, or by typhus, or by phthisis with delirium, or by spasmodic cholera, or by universal bronchitis with emphysema pulmonum, otherwise vesicular dilatation, with bronchial hypertrophy; and there were also some examples of violent pleuro-pneumonia in advanced stages, by which

morbus cordis was masked, until the cases were hopeless, or even until death. Of these 170 to 180 cases, two-thirds almost exactly were males, and one-third only females. Thirty were cases of well-marked valvular disease, combined with obvious hypertrophy in almost every case; and the remainder, or nearly five-sixths, were cases of simple hypertrophy, without pericarditis or endo-carditis, or unequivocal valvular deficiency or disease.

The diagnostication of the hypertrophy was, as is usual, easy in most cases. The unaided touch or sight was often sufficient to shew that the volume and weight of the organs, and thickness of their walls, were abnormal. Where decided valvular deficiency or disease existed, of course the evidence of morbus cordis was obvious. But in none of the cases could *the degree* of hypertrophy be correctly estimated without measurement, whether linear, or by volume, or by weight. In a very considerable proportion of them, no observer could, without such aid, affirm that the excess of weight or volume was slight, considerable, or extreme, upon any better or less equivocal ground, than his own private conviction; and in several cases in which considerable hypertrophy existed, the heart being found to weigh 11 to 12 oz. or more, in persons of common size, it was at first conceived, *judging by the touch and sight*, that the heart was normal.

One case particularly struck me. It was that of a person whom I had known for some years during life, and who for many years had been short-winded, and what is popularly called asthmatic. This man had had influenza during the last great epidemic, and had not completely recovered, when he over-exerted himself by carrying a weighty parcel, and was immediately attacked with violent dyspnœa, with extreme anxiety, and some pneumonic symptoms, and sank within a week. The heart of this man was pronounced normal by several spectators familiar with pathological anatomy, and appeared to myself much less than from his history I should have expected. When placed in the balance, it was found materially to exceed the limits of health, his size, sex, age, and every other circumstance, duly considered. It weighed 11¼ ounces avoirdupois, and was, of course, in bulk as measured in water, some drachms less.

In nearly all the cases there was more or less hypertrophy, often on both sides, but almost always in the left ventricle at least. The exceptions, amounting to two or three per cent. of the 170-80, were mostly wasted females advanced in life, and dead of valvular disease. The average weight of about eighty male hearts, taken without selection on account of weight, was about 15 ounces avoirdupois, instead of 9 ounces, the normal weight for adults above fifteen years, or an increase of substance of two-fifths, or 40 per cent., and the female average on forty to fifty specimens was about 13 ounces, or 38½ per cent. increase, or nearly the same proportional increase as in the male cases. In no instance of morbus cordis, whether including valvular disease or not, have I observed any diminution of the heart in weight or volume below the normal proportions above stated, not even in such as were wasted by phthisis, antiphlogistic remedies, long suffering, &c.

Complications of morbus cordis.—In almost no instance in which the heart was known, during life, to be diseased, or was the subject of just suspicion, do I find that the heart alone was in a morbid state. On the contrary, complication with diseases of other organs was the rule, to which the exceptions bore a very small proportion indeed, limited nearly to cases of persons cut short suddenly by accidents, and mostly of no advanced ages. The complicating diseases were numerous, principally inflammatory, often acute, more frequently chronic; and these complications bore as large a proportion to the uncomplicated cases in the 140-50 instances of simple hypertrophy, very nearly, as in the thirty cases of decided valvular disease. In each class the complications were of the same nature likewise. No marked difference was observed between them, except that, on the whole, there was, during life, more pectoral distress in the class of cases involving valvular disease, and disease of the orifices,—which latter by the way, when real, and not an accident depending on the state of the contractile energy immediately preceding and attending death, usually, I conceive, implies the former or valvular disease;—Also, that the tendency to diffused dropsy, including all the cavities nearly, as well as the cellular substance of the lungs and lower extremities, and also the pulmonary vesicles, was greater on the whole in the class Valvular Disease. The particulars of the well-marked complications ascertained post-mortem, were as follows:—

Of the cases of simple hypertrophy,
23 had phthisis.
29 — pleuritis.
36 — pneumonia and pleuro-pneumonia.
22 — pericarditis.
6 — mania.
23 — various other diseases; amounting together to 14, viz.—typhus, delirium tremens, influenza, metritis, peri-

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tonitis, cholera, rubeola, morb. cerul., erysipelas, diarrhœa, quotidian, hæmorrhages, aneurism, and mors subit. Besides the above 139, fifteen had apoplexy, viz. nine males and six females, out of a total of 34 cases of apoplexy, or nearly 3-7ths or 42 per cent. of all the cases of apoplexy examined. Of the remaining 4-7ths, six were males, of whose hearts two were not examined, and thirteen were females, of whom four were doubtful for the same reason.

The general total of complications of morbus cordis was then 154, exclusive of bronchitis, and exclusive also of several instances of morb. chron. cerebri — such as comatose, epileptic, and fatuous cases of long standing; also cases of softening (ramollissement) of the brain in advanced years; none of which are included above. Of the whole number of cases, a small fractional part only were free from chronic bronchial disease, in the shape of bronchial injection and hypertrophy, with vesicular dilatation. A majority had likewise œdema pulmonum, and a large proportion had serous effusions, or dropsy. In 5-6ths, to 6-7ths, or between 80 and 90 per cent., there was enlargement of all or several of the viscera; the lungs included, which were generally considerably denser and heavier than normal. In all these respects there was little if any difference between those cases of simple hypertrophy and those other cases in which the valves were implicated.

The following tables give the average weights of the viscera in the classes *Varia* and *Morbus Cordis*, according to age and sex:—

Table of Weight of Viscera according to Age and Sex.

VARIA.

	15 to 30.		30 to 50.		50 to 70.		70 to 100.		No. of Obs.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Lungs	38	33½	50	35	56¾	36	41	38¼	31	34
Liver.....	55	55⅔	49	47	49⅓	40⅔	41⅔	35	55	101
Spleen	5	5	4½	5	4½	4	5	3½	50	93
Stomach ..	5	4⅔	5	4½	4⅓	4⅙	4	4⅓	34	51
Kidneys ..	8¼	9½	9	8⅔	8¼	7½	8	6⅓	55	98
Pancreas ..	3	2⅓	2½	2⅓	2½	2⅙	1⅔	2⅕	51	84
Encephalon	49·33	42·17	43·5	42·5	45·8	42·75	47	39·9		

Table of Viscera according to Age and Sex.

MORBUS CORDIS.

	MALES.				FEMALES.			
	15 to 30.	30 to 50.	50 to 70.	70 to 100.	15 to 30.	30 to 50.	50 to 70.	70 to 100.
Encephalon	50·75	49·66	47·1	41·5	..	44·5	45·8	40·5
Cerebellum	4·5	5·5	5·12	4·7	..	4·75	5·25	4·25
Lungs	47·5	59·	56·5	59·	33·	32·	41·	35·
Liver	66·5	63·25	51·5	48·5	59·	54·	50·25	40·5
Spleen....	7·	8·25	5·17	4·33	7·5	5·75	5 5	4·
Stomach ..	5·25	5·5	5·5	5·5	5·5	5·	4·75	5·33
Kidneys ..	11·25	11·25	9·5	8·33	10·5	10·	8·5	8·
Pancreas ..	3·	3·2	3·	3·11	2·5	3·25	2 25	2 38

From these tables, which are founded on totals which are in most cases considerable, and include several scores of separate observations, it would appear that in morbus cordis there is an increase in the volume and weight of every single viscus, as compared with the standard of the class *Varia*, (with the exception of the male cerebellum,) on the whole of life, and in each subdivision of the 4 in each table— viz. from 15 to 100, and from 15 to 30, 30 to 50, 50 to 70, and 70 to 100. The average, in fact, in nearly every observation is considerably above the standard in the case of

every viscus. The encephalon, if we take the first column, or the interval, 15 to 30, gives for the male 50¾, or 3lbs. 2¾ oz. avoirdupois, instead of 49½, or 3lbs. 1⅓ oz.; being an excess over the normal dimensions of nearly 1-33d part. The lungs, in like manner, are found to exceed by about 1-5th; the liver by about 1-11th; the spleen by 2-7ths; the stomach by 1-20th; the kidneys by about 1-4th. Then, in the second column, for the interval 30 to 50, the encephalon exceeds the normal standard by more than 1 8th; the lungs by nearly 1 5th; the liver by more than 1-5th; the spleen by 1-half; the stomach by more than 1-20th; the kidneys by more than 1-5th; and the pancreas by more than 1-6th. In the third column the encephalon is less than in the second, as there it was less than in the first; but it is still 1-35th in excess, at least; while the lungs are about the same weight: but the liver is larger by more than 1-5th; the stomach by 1 5th; the kidneys by 1-9th; the pancreas by 1-6th; and so on of the rest. And in every column, in like manner, the female portion of the table gives the same or a similar excess of the viscera above the normal standard; whilst, on both sides of the table, and in the case of every organ, the life averages for morbus cordis exceed those for other diseases not complicated with disease of the heart. The averages for the two classes for life are as follows:—

Encephalon.

	Males.	Females.
Varia	45·33	42·62
Morbus cordis	47·25	43 60

Male Varia.	Lungs	46·5	Male Morbus Cordis. 55·5
	Liver	49·	57·
	Spleen	4·75	6·12
	Stomach	4·5	5·40
	Kidneys	8·33	10·
	Pancreas	2·25	3·

Female Varia.	Lungs	35·5	Female Morb. Cordis. 36·
	Liver	44·5	50·75
	Spleen	4·33	5·5
	Stomach	4·40	5·12
	Kidneys	8·	9·25
	Pancreas	2·25	2·5

Since, then, the general averages, and not those only, but also the sub-averages or quotients of the different ages of each sex respectively, differ so decidedly, it seems difficult to avoid the conclusion, that in morbus cordis there is usually a considerable increase of volume and weight in the viscera of all the cavities: and indeed, to judge by my own opportunities, I should say that there cannot be a doubt that the enlargement of several of the abdominal viscera remarked by several authors, and usually referred to congestion of blood in the capillaries, owing to reflux, real or imaginary, from the heart, is a substantive, and not an apparent hypertrophy only. After first observing the frequency of such visceral enlargements, I constantly incised every viscus before weighing it, until I satisfied myself that the conjectural explanation founded on supposed reflux and stasis of the blood, would not meet the facts, and that, in addition to occasional or even frequent and considerable congestion, there must be excess of deposition of assimilated fluids, otherwise excessive nutrition. But latterly the encephalon, heart, lungs, and stomach, have been the only organs uniformly incised, and saving the heart and stomach, and occasionally the lungs also, I have not observed the weight of any viscus to be materially altered by incision, which, however, they could not fail to be, if any considerable portion of their volume or weight depended on congested fluids.

Additional Considerations.—Now as the question, as to whether morbus cordis is ordinarily attended by general visceral enlargement or not, and is itself directly or indirectly either the cause or the effect of such enlargements and hypertrophies or not, is a question of some importance in several respects, it seems fitting that I should here state any additional facts or considerations that may be at my disposal, in confirmation of the view I take of the subject.

To begin with the encephalon. One fact is this, that of 58 of the 520—30 cases, in which the encephalon weighed 50 oz. and upwards, and of which 53 were males, and 5 females, 33 were known cases of morbus cordis, or more than 61 per cent.; and of six or eight, it is uncertain whether the heart was hypertrophous or not. This coincidence of excessive volume in the brain and heart in so large a proportion of cases, is not easily reconciled with any other supposition than that which I have adopted, viz. that the encephalic augmentation is an effect in some way of the cardiac disease. This conclusion derives partial support from Dr. Sims' paper. Of his cases, 37 were of 50 oz. and upwards of encephalic weight, and of those most had died either of morbus cordis, or dropsy, or apoplexy, or phthisis, or pneumonia, and were, some certainly, others probably, complicated with more or less hypertrophy, more especially of the left ventricle.

Another fact is the connexion long since remarked by Albertini, I think, and above exemplified, (p. 595, c. 1), between apoplexy and morbus cordis, and which has been exemplified also, it is said, in the persons of several eminent medical professors: of Malpighi, and, if my memory rightly serves me, of Lancisi, of Corvisart, of Cabanis, of Ramazzini, of Parry, and others; and of many lay persons of consideration; one of our British Kings of the Hanoverian line being amongst the number. Sudden arrest of action in an organ naturally of the most delicate susceptibility, when in a plethoric condition, is no more singular than sudden increase of activity. These extreme fluctuations are common in all functions depending essentially on vital properties and dynamic conditions; and seated in organs over stimulated and over nourished. Sudden arrest of the action of the hypertrophous heart is one of the ordinary forms of death in morbus cordis. Syncope, another form of suspended action, or at least diminished action, is another frequent incident in the course of cardiac disease. Assuming, therefore, the hypertrophy of the brain in those individuals, I can easily understand the occurrence of apoplexy. If cerebral pressure be the true cause of apoplexy, or even supposing it the commonest exciting cause only, which appears to me the more reasonable supposition of the two, it is easily understood that a moderate transitory congestion, or sudden afflux of the blood, may, in a too voluminous encephalon, excite an apoplectic seizure. It is likewise very obvious that, to persons advanced in life, as were Malpighi, George the Second, &c., in whom induration and fragility may have commenced in the cerebral arteries, a very moderate accumulation of blood may be attended by cerebral hæmorrhage.

Observations of authors.—Then, with regard to the other viscera, it is well known that numerous pathological writers have recorded instances of enlargement of induration, and other morbid changes of several of the viscera, in connexion with morbus cordis. With respect to the lungs, for example, Lancisi states, that in his experience aneurism of the aorta (which I find invariably accompanied by hypertrophy of the heart) had often caused suffocation, asthma, hydrothorax, and other pectoral symptoms, commonly referred to other viscera.—(1740, p. 284.) Lancisi also attributes aneurism of the heart to asthma and chronic catarrh, shewing thus that he was aware of a connexion between pulmonic diseases and morbus cordis. Senac adds, phthisis and pneumonia, (L. 6., C. 8., S. 4.) Corvisart speaks of induration and engorgement of the lungs, attending morbus cordis as an effect of it. (By Hebb., p. 388.) Dr. Bree quotes from Morgagni, and other authors, several cases of asthmatics, whose lungs were found charged with fluids, and which were probably cases of chronic catarrh, with morbus cordis. In several, he mentions the lungs were firm and heavy. In all, the symptoms were those of the bronchitis of advanced years, which, as Andral has well remarked, usually depends on, or is connected with, enlargement of the heart. He remarks, as it were in confirmation of that supposition, that, in his opinion, and those of numerous authors, the spasmodic asthma is more common in males than in females, which is just what must happen if asthma be always dependent on, or connected with, disease of the heart; as, along with Kreyssig and several excellent French pathologists, I believe it to be. Lieutaud gives, in his great collection, numbers of cases of heart disease, certain or probable, in which the lungs were also diseased—viz. emphysematous, œdematous, inflamed, &c., especially in his second book, sections 2, 5, 6, 7, 9, 12, 14, 15, 16, in each of which cases are given in which the heart appears to have been diseased in connexion with grave pulmonary disorder, as adhesions of the lungs, sect. 5—emphysema, sect 6—infarction, sect. 7—inflammation, sect. 9—pulmones purulenti, sect. 12—ulcus pulmonum, sect. 14—pulmones præduri et schirrosi, sect. 15—pulmones tuberculosi, sect. 16—and in the two following, several cases are given in which cardiac symptoms with pulmonic were mingled during life; and in several of which, though disease of heart is omitted, because probably overlooked, enlargements, indurations, &c. of lungs, liver, spleen, &c. are mentioned. In his second volume, from observations 407 to 441, numerous cases in point are cited. Observations 613 to 652 have most of these pectoral symptoms, cardiac and pulmonic, and many morbid appearances in each organ. The observations 701, 705, 847, 861, present similar combinations of pectoral symptoms and appearances. In like manner, in his chapter "De Colluvie Serosâ," numerous observations illustrate the pathological connexion between the two great organs of the organic life; and though the heart is not mentioned in several, yet morbus cordis appears to have existed, and produced visceral indurations, congestion, and enlargements. Dr. Hastings has a chapter, in his History of Bronchitis, "On dropsy dependent on bronchitis;" and in his description of the disease, he enumerates "constriction across the chest, as if by a ligature; violet colour of the lips; lividity of the face; anxiety and uneasiness referred to the heart; palpitations; extension of the

heart's action beyond normal limits; pulsation in epigastrio; an undulating motion of the heart during the action of the ventricle; irregularity of the pulse; dulness on percussion on the left thorax;" amongst the symptoms. He adds that it is common in old people who have had chronic cough for years, and much expectoration; common also in spirit-drinkers in the decline of life; the heart in such cases being generally enlarged, and sometimes adherent. He attributes the enlargement of the heart to extension of disease from the bronchia. Notwithstanding all this equivocal ætiology, there is abundant evidence of a knowledge of pulmonic disease in connexion with diseased heart. In the third volume of the Clinique Médicale, we are informed that when chronic bronchitis is accompanied with much oppression, it is in that case complicated with morbus cordis; the disease of the heart being often prior in date, but sometimes being posterior to the bronchitis, and an effect of it and the dyspnœa which it occasions.

Such are some of the principal particulars that I meet with in authors confirmatory of the connexion I have represented as nearly constant between pulmonic and chronic cardiac disease. Had the practice of slitting open the bronchia in *post-mortem* examinations, with a view to inspect the inner surface of the bronchial ramifications, and to ascertain the condition of the parietes of the air-tubules and air-vesicles, been more common, I cannot doubt that I should readily find much more precise information on the subject of the connexion between the lungs and heart in disease.

But the evidences respecting the connexion of abdominal visceral enlargements with morbus cordis found scattered over authors, are much more numerous and precise than those that relate to pulmonic complications of morbus cordis.

Lieutaud has been already cited as noticing the existence of abdominal visceral enlargements in connexion with diseases of the heart: that he does in numberless places, many of which have been already referred to. Senac states, that what he calls "engorgement," by which I understand apparent enlargement and real hypertrophy, is a frequent, though (then) little known consequence of dilatation of the heart. Corvisart points out "engorgement of the liver with tenderness" as an occasional source of error in diagnosis, (an observation I have had myself frequent opportunities of verifying); but the large liver is common in one degree or another in all heart cases, owing, he thinks, to regurgitation. Dr. Bree mentions enlargement of the liver as common in connexion with enlarged heart, and as a cause of asthma. Dr. Hastings remarks the connexion between the bronchitis of the drunkard and enlargement of the liver, and ascribes the thoracic disorder to the extension of the hepatic irritation to the lungs. But Portal gives the most information respecting the morbific powers of the liver, of such authors as I have had opportunity of consulting. He ascribes the symptoms of steno-cardia to disease of the liver, including the stricture of the chest, and spasms of the the diaphragm, also the numbness of the arms, risus sardonicus, palpitations, syncopes, and every other symptom of angina pectoris. He states that numerous observers have found hepatic enlargement to attend steno-cardia, and says that pains truly hepatic are often referred to the heart, and various other organs, which he enumerates. He adds, however, that obstructed circulation through the heart is a frequent cause of hepatic disease, and that in such cases the heart is usually found enormously enlarged. But whatever may be thought of Portal's opinions, his facts are valuable. In the second part of his work on the Liver, he gives, in the 11th article, eleven cases in which the liver was found enlarged, dyspnœa, &c. having existed during life, and in two or three of which traces of pulmonic and cardiac disease are noted. In his 12th article, he gives eight cases of palpitations and syncope from steno-cardia, depending, he conceived, on hepatic disease. In all the eight, the liver was found enlarged after death; and nearly all were observed to have enlargement of the heart, and other pectoral disease. Many recent writers in Britain and France have noticed the connexion of abdominal visceral enlargement, more particularly hepatic, with cardiac disease. The writings of Doctors Bouillaud, Lobstein, Abercrombie, Hope, Latham, Bright, Andral, Copland, and others, contain distinct statements to that effect, and many examples. The work of Professor Bouilland, "Du Cœur," contains more examples of abdominal visceral enlargement than any recent writer that I know of. For examples, vide Cases 21, 23, and 24. Observations 43, 51, 53, 57, 60, 61, 62, 63, 68, 69, 72, 119, 121, 125, 129, 130, and 132: in all of which the heart was of course diseased, and the liver is recorded as enlarged, and in several instances other abdominal viscera also. In Lobstein's Pathology, in the article "on the Heart," there are three cases at least in which the coincidence between cardiac and hepatic enlargement is noted. Dr. Abercrombie has mention of the fact just referred to in three of his cases in his paper in Edin. Med. Chir. Trans.; viz. Cases vii. x. and xi. Dr. Hope records the pathological connexion under consideration, in several of his cases, as in C. iii.

ix. xv. xxii. in his work on the Heart. Dr. Bright mentions in his valuable Medical Reports, that in several cases the coincidence between pectoral and abdominal hypertrophous disease, so far at least as the liver and heart are concerned, was observed by him. For examples, see his 7th, 22d, and 24th cases. Dr. Latham informs us in his instructive lectures delivered in this place, and published in the MEDICAL GAZETTE, vol. iii. that he is familiar with the occurrence of enlargement of the abdominal viscera as a complication or effect of morbus cordis; and he specifies the liver, kidneys, and spleen, as the organs most liable to hypertrophy under those circumstances. Dr. Copland mentions the enlargement of the abdominal glands, as according to his observation a common complication of heart disease, in his articles, "Asthma," "Disease of the Heart," &c., in his well-known and admirable digest of professional opinion and experience. Vide p. 94.

The opinions of the authors just quoted are by no means in harmony on every point, and vary especially, I think, on the subject of ætiology. But to the fact of the occasional connexion so often named, they clearly all bear witness, more or less decidedly.

The acquaintance of German pathologists with abdominal and pulmonic complications of morbus cordis, is evinced by Kreyssig, who speaks at length about it in his 6th chapter; while Testa, speaking we may suppose for Italy, assures us in his eighth chapter, that in most bodies of cardiacs he has found the liver enlarged and elevated above its normal situation, so as sometimes even to surpass the third rib.

In fact, there is no organ that I have examined, respecting which I have not met several notices in preceding accounts, excepting the stomach, the encephalon, and the cerebellum, which have not been, so far as I know, examined with views similar to mine; and the pancreas, of which, as enlarged in morbus cordis however, I have seen some notice, I think, in *post-mortem* reports, to which I have mislaid my references.

Summary conclusion.—It may then, I think, fairly be assumed that visceral hypertrophy and enlargements are usual attendants of morbus cordis in every cavity, and that if there are exceptional cases (and how should there be a rule in physiology, morbid or healthy, without exceptions?), such cases constitute but a small fraction of the sum total of cases of diseased heart, and confirm that rule of the truth of which they are part of the evidence.

Deductions from foregoing observations.—Having thus given a summary view of some of the principal results of more than five hundred post-mortem examinations of persons above puberty, and without selection of cases, I proceed shortly to state such observations as may be suggested by the facts related, as throwing light upon the diseases of the heart: and before entering on any particulars of this second part of my inquiry, I would make one or two general explanatory remarks, in order to avoid the necessity of interjecting any thing personal to myself into the body of the argument.

Preliminary observations.—Practical medicine is to be cultivated, not by one method only, but by several. The principal are *the natural history method*, and the *numerical method*; which latter might also be called the empirical or statistical method. The former necessarily precedes the latter. To examine, classify, and describe objects—diseases or morbid changes, for example—is the first step in all scientific medicine; and beyond this first step little progress has as yet been made in the science. But when the enumeration and description of the genera, species, and varieties of the causes, signs, effects, and remedies of diseases are completed, practical medicine is only begun; for a second and more difficult step remains to be accomplished, which is, for all social and technical purposes, as necessary as the first, and has for its end the fixing with precision the relative practical value and importance of the objects examined and classified, by ascertaining for each respectively the order and frequency of its occurrence, and the extent of its distribution, and the limits of its power and duration; and thus accurately defining the preponderant tendencies of each, in its relations to life and health. This step is made principally by the aid of numbers, without some aid from which, direct or indirect, it cannot well be done at all. A small or little varied experience may, perhaps, be correctly estimated by the rude informal arithmetic of recollection or memory. But in dealing with facts, numerous and complicated, memory is unworthy of confidence. From neglect of this in great part, no doubt, it is that the writings of many practical authors, especially the older authors, are so deficient in materials of rigorous proof or disproof; and might be justly entitled "Popular Essays," or "Magisterial Discourses on Medical Subjects," rather than contributions to practical science. Now, however, there is happily a better spirit abroad, and facts are more skilfully observed and more accurately recorded. But something still remains to be done, and more especially in the way of improvement of the means

of observation; and the use of instruments of various kinds has as yet, I apprehend, been too much neglected. Partly owing to imperfections from this cause, in various works I have consulted, and partly owing to some peculiarity in the means of examination I have employed, I have been obliged to limit myself too exclusively to my own papers, for precise facts upon which to reason; and I think it advisable to mention this before-hand, lest, from not referring so much as I should like, or might be expected to do, to authors, I should be suspected of an indolence that had found reading laborious, or of a vanity capable of fancying it to be unnecessary.

THE MORBID ANATOMY OF PHTHISIS.

By F. W. Grant Calder,
Assistant-Surgeon, 2d Regiment of Life Guards.

[Concluded from p. 574.]

The bronchial glands were frequently enough found slightly enlarged, but rarely to such a degree as to draw particular attention; and in only two cases out of the seventy can I say that they presented well-marked scrofulous disease.

In only one case were the cervical glands enlarged and in the stage of tuberculous conversion; and in this case some of the deeper seated glands around the œsophagus were likewise diseased. There is a preparation in the museum from this patient, exhibiting the mucous surface of the œsophagus for a space equal in size to the nail of the little finger ulcerated. The muscular coat is exposed and likewise ulcerated, and there is a small pouch connected with this in the cellular tissue behind the ulcerated base. The cervical glands were much affected with tuberculous disease, and for months before his death had degenerated, and were discharging their contents by ulcerated openings in the integuments of the neck. There are likewise two œsophageal glands, enlarged and connected to the back part of the preparation, laid open, and exhibiting complete tubercular transformation. I am therefore inclined to suppose that this ulcer of the œsophagus was owing to a diseased gland having connected itself by the adhesive process to the walls of the tube, and subsequently becoming soft, the disorganized matter had given rise to the ulcerated opening in question, and had thus become discharged. It gave no symptoms during life, and was merely detected in the usual course of examination after death.

In no case was there any tubercular disease connected with the heart, and in only one case was the lining membrane of the ascending aorta found somewhat thickened, and this was in a man aged 48. Indeed, the arterial system appears to be remarkably exempt from disease in phthisical subjects.

In only one case was tuberculous disease of the stomach met with. The subject was G. Bell, 60th Rifles, who died of empyema of left side of thorax. Both lungs were studded with tubercles, and there were a few small cavities in the upper lobe of the left one. The mesenteric glands were enlarged and tuberculous, and the lacteals contained a whitish-looking substance, which might or might not be considered scrofulous. The mucous membrane of the lower portion of the ileum and commencement of the cæcum was ulcerated, but without any appearance of tuberculous disease. There was a *single* tubercle, of the size of a pea, ripe, and possessing the usual cheesy characteristics, situated beneath the mucous coat of the stomach, about two inches from the pylorus*.

In only one case were tubercles met

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* In connexion with this subject I may give the following curious case of ulceration of this organ occurring in a subject who died of phthisis, and before whose death not the slightest suspicion existed of any such affection. He was attentively watched during his latter illness by a gentleman of superior medical attainments—I mean Dr. Hume, Assistant-Surgeon to the Forces; and he was regularly visited by that accomplished surgeon and naturalist, Staff-Surgeon Burton, who superintended the division. Both of these gentlemen remarked to me afterwards, that it was often a matter of some surprise to them during his life, that even when he appeared sinking his appetite yet continued so good, that when to all appearance he had not many hours to live, he used earnestly to implore that he might have a beef-steak allowed him; and this he invariably enjoyed up to the very day of his death. The stomach of this man, Josiah Davis, 23d, or Royal Welch Fusileers, is one of the finest specimens of disease in the collection. The mucous membrane of the whole of the lesser curvature, and close to the very pylorus, is one sheet of irregular ulceration. The ulcerated surface has a worm-eaten appearance, with the mucous membrane in some places apparently healthy, and thus forming small isolated points in the midst of the ulcerated base. It is entirely unconnected with any tuberculous matter, and gave no symptoms during life save a little singultus which he was now and then troubled with during his latter days.

with in the duodenum, and the following are its particulars:—Thomas M'Donald, aged 24, 79th Highlanders. Both lungs very extensively diseased. A number of small ripe tubercles, complicated with ulceration of the mucous membrane, were found situated beneath the outer coat, and occupying its primary division only. The ileum and cæcum were similarly affected, while the jejunum was free.

In only three cases out of the whole number was the jejunum found affected with tubercular disease. In one of these cases ripe tubercles, rather larger than millet seeds, were found pretty generally scattered throughout this portion of the gut, and situated beneath its serous coat. Small ulcerations with well-defined borders were at the same time found to exist in considerable numbers in the mucous coat; these ulcers had apparently no connexion with the tubercles, being clean and healthy-looking, and entirely free of any tubercular matter, a circumstance, I believe, which is seldom met with when the reverse is the case, there always existing some in that stage of development which tend to explain the origin of those which might appear otherwise more doubtful. In the remaining two cases the tubercles were clearly situated beneath the mucous membrane, which at the same time in several places presented ulcerations connected with them, and by which they were being discharged into the alimentary canal.

In nine other cases the mucous membrane of this portion of the gut was found pretty extensively occupied with small deep ulcers, having well-defined borders, but without any trace of scrofulous matter in them, the gut being indeed perfectly free from this morbid product. One of the cases included in this number was rather curious; it was that of Henry Rushforth, 22d Regiment, the mucous membrane of whose jejunum and cæcum was very extensively ulcerated, while the ileum throughout its whole extent was perfectly healthy.

In two cases the mucous membrane of the duodenum generally was found studded with small ulcers, but without any appearance of tubercular disease.

In three cases ulcerations of the mucous coat of the ileum existed, complicated with tubercles situated beneath its serous coat, but without apparently being connected with or dependent upon them, after the same manner in which I have described a like state to have existed in the jejunum.

In nine cases ulcerations of the mucous coat were met with, and complicated with tubercles on its cellular surface; and in one case tubercles were found in the latter situation, while the mucous membrane still remained sound: they had most likely not advanced sufficiently far to prove obnoxious to the surrounding tissues.

Again, in sixteen cases the mucous membrane chiefly of its lower third was found in various states of ulceration, and without the semblance of tubercular disease in any part of it; so that we may say that in twenty-eight cases out of the whole number we had ulcerations of this portion of gut.

In eight cases tubercles were found pretty extensively scattered beneath the mucous membrane of the cæcum, and in seven of these cases this state was complicated with ulceration of this latter membrane; in the other case the tubercles were present, but without being attended with ulceration. Five times this same state existed in the ascending colon, *i. e.* ulceration of the mucous coat, with tubercles on its cellular surface; twice the transverse portion was similarly affected, and only once were the descending and sigmoid divisions so diseased.

In twenty-six other cases the mucous membrane of the cæcum was found extensively ulcerated, but unconnected with any tubercular disease; fifteen times this prevailed in the ascending portions four times in the transverse portion, and twice in that of the descending; so that in thirty-three cases we had ulceration of the cæcum and colon; the frequency of ulceration in this portion of the gut exceeding that of the small intestine by five *. To give any particular description of these ulcers would be impossible, they presented such variety of character both in size and appearance; and there is the less

* I have unfortunately lost that part of my notes where I had it marked down as to how the ileum was found ulcerated, independent of a like state being present in the cæcum, and *vice versâ*; and again, as to how often both these portions of the intestinal canal were so diseased at the same time. The result of these observations, therefore, proves only that the cæcum was five times oftener affected than the ileum, but without reference as to how often both these were so situated in the same subject.

to be regretted on this account, seeing that such a description could lead to no better mode of treatment; their very existence being at best but equivocal, until it is too late for any treatment to be of avail. Thus, I have known a man in the last stage of phthisis for weeks previous to his death affected with severe diarrhœa, tormina, and frequent pains in the abdomen, and whose intestinal canal from the beginning to the end exhibited no vestige of disease after death; and again, I have known another similarly situated as regarded diseased lungs, and who never during his whole illness made any complaint of his bowels, or who was ever troubled with diarrhœa, and yet after whose death the greater part both of the ileum and cæcum was found extensively ulcerated. I only, indeed, met with these extreme cases once, yet nothing is more common than to meet with others where during life the symptoms would indicate much more extensive disease than after all we really do find.

The rectum was not always examined, but in those cases where the colon was found extensively ulcerated, it was likewise found in a similar state.

In four cases the omentum majus was found studded with tubercles, and in one of the cases the mesentery was likewise so.

In four cases beneath the peritoneum generally (both visceral and parietal) large numbers of tubercles were seen, and in two of these cases well-marked symptoms of peritonitis, which had existed before death, were further verified afterwards.

In nine cases the mesenteric glands were enlarged, of a fleshy consistence, and in that state which I have already described as the primary stage of the *tubercular transformation*. In thirteen cases these glands were not only so enlarged, but were likewise much affected with tubercular disease.

In two cases a large number of lacteal vessels, many of which could be traced from the intestine and the root of the mesentery, were found tortuous, much dilated, and crammed with soft tuberculous matter.

In no case were tubercles met with in the substance of the liver, but in two cases scrofulous matter was found effused in small patches under its peritoneal lining, both in its convex and concave surfaces. Dr. Baillie remarked the rarity of scrofulous tubercles in the liver, and on this had observed that in the only instance which he had seen of the disease, the tubercles were dispersed through the substance of the organ at pretty regular distances, but did not render its surface irregular, as in the common sort of tubercle; and he further observes, that they were a little brown in their colour. Now this is an appearance which I am not aware that tuberculous matter ever assumes; yet it is possible that in this case it might have been accidental, depending on an admixture of bile; but as his extensive experience only afforded one doubtful case, the rarity at least of tubercles in this organ is certain*. I remember the following case, which will attest the great care that is necessary in not too readily deciding from mere appearances. A young soldier died of phthisis, and at the *post-mortem* examination, in addition to the various changes which extensive tuberculous disease had wrought in the lungs, the viscera of the abdomen were generally affected. The spleen presented an extensive distribution of miliary tubercles throughout its substance and beneath its peritoneal investment: on making a section of the liver, the same appearances were exhibited, and it was concluded that they were likewise tubercles; but the ready talent and ingenuity of my friend Mr. Hibbert, of the 2nd or Queen's Royal Regiment, suggested to him the propriety of a more strict scrutiny, and, on further inquiry, it was found that each of these small greyish-looking bodies presented a central nucleus of green. The organ was now thoroughly examined; its parenchyma generally was healthy; it was throughout closely studded with small white-coloured bodies, a little larger than the head of a pin, firm to the touch, and closely adherent to the healthy structure of the organ. The centre of each, as has been mentioned, contained a trace of greenish matter. Yet this circumstance might still have rendered their real nature somewhat obscure, had we not detected some large ones, equal in size to a garden pea, and which, when cut into, quite resembled small

* My own opinion is, that miliary tubercles are very rarely, if ever, met with in this organ; hence, for an explanation of the origin of these tuberculous deposits which are occasionally seen, the reader is referred to what has been said under the head of tubercular masses and tubercular conversion.

cysts, from their possessing thin parietes, and being filled with a semi-concrete substance of a greenish colour and bilious appearance. The gall-bladder was much distended with bile, and a chain of glands which had completely undergone the tuberculous transformation, and which were very much enlarged, was found occupying the lesser omentum in such a manner as to press upon the ductus communis choledocus, and thus obstruct the flow of bile through it into the intestine. It is presumed that these small white bodies dispersed throughout the liver, and which at first sight so much resembled tubercles similar to those met with in the other parts of the body, were the radieles of the biliary ducts enlarged and distended with their contents, in consequence of the mechanical pressure offered to its passage through the common duct. A portion of this liver was made into a preparation, and my intelligent friend, Staff-Surgeon Nicholson, who superintended the *post-mortem*, expressed himself much pleased with the result of the examination*.

In one case the substance of the spleen was found thickly studded with small sized tubercles; some likewise existed beneath its serous investment.

In only two cases were the kidneys affected, and both times it was the right one. Once a few small circumscribed deposits of scrofulous matter were found in its substance; and in the other cases a portion equal in diameter to a shilling, and a quarter of an inch in depth, exhibited evidences of having passed into the second stage of *tubercular transformation*, from its possessing the cheesy look and other characteristics which I have already described as indicative of that state.

In one case both renal capsules were found with tubercles in their interior. Not unfrequently, however, these parts present an appearance very much resembling this state, and this, too, in subjects who die of other diseases, and who present no vestige of scrofulous tubercles in any other part of their bodies. The following appearances were observed in both renal capsules of a man named Develin, aged 36, who died of dysentery, and in whose body no tubercles existed. They were both somewhat enlarged, and contained no cavity. A section displayed two distinct structures, the external being of a yellow tuberculous appearance, and the internal having an ash-colour, with a central furrow running in a longitudinal direction from above downwards. The external one, in addition to the yellow cheese-like colour it possessed, had likewise a granulated surface, which gave it more the appearance of being tuberculated; and, indeed, some of these granules seem merely connected to the surface of the capsules by a loose filamentous tissue. On minute examination, however, and by making a neat section of these granules, a central ash-coloured portion occupied the interior of each, with the same kind of furrow as was seen in the interior of both capsules. In fact, they appeared as so many additional capsules; and had it not been for the structure displayed by their interior, on a clean section being made, doubts might very reasonably have been entertained as to their real nature; and it is more than probable that a less careful scrutiny might have set them down as scrofulous tubercles.

In three cases the testicles were affected, viz. the right twice, and the left once. I have said that the testicle is subject to undergo the tuberculous conversion; and this is the only organ, that I am aware of, as being, likewise, the seat of miliary tubercles; for in corroboration of this remark there is a beautiful preparation in the collection at Chatham, exhibiting a portion of the body of the testicle so converted, while the remaining part is occupied with miliary tubercles.

Sir Benjamin Brodie, in his lectures on the Diseases of the Testicle, and which are to be found in the 13th vol. of the MEDICAL GAZETTE, has described at page 220, under the head of Chronic Tubercular Inflammation of the Testicle, a disease which I had occasion to see twice. The cases were those of two soldiers, who arrived from India after a long voyage, ill of scurvy and dysentery. They very soon died after

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* Louis found in two cases of phthisis cysts of a similar nature in the liver.

It is a curious thing that we tried, in almost every case of phthisis, to see whether the liver was fatty or not, and much in the same way, too, that Louis did, viz. by putting a thin slice on a bit of paper, and then either exposing it to the flame of a candle or the heat of the fire, and yet not a single piece of fatty liver did we find. Dr. Davy, who I believe is second to none in investigations of this nature, superintended these examinations.

their arrival in this country; and on a post-mortem being made, both the testicles of each were found with tubercles in them, of a yellow appearance, and answering, in a general manner, to the description given by Sir Benjamin, yet possessing such strong analogies to the scrofulous matter which is found in the same situation when the patients have died of phthisis, that I cannot help, with all due deference to such an authority, looking upon them as cases where scrofulous diseases had been lighted up locally, and independent of the disposition which gave rise to this, extending towards the other parts of the body. I am, moreover, strengthened in this opinion, in consequence of a few miliary bodies exactly similar to those met with in the lungs of phthisical subjects being present in one of the testicles, along with a well-marked example of this yellow tubercle, and which Sir Benjamin has thought fit to consider of another nature. I may further observe, that as the patients never complained of their testicles, it is pretty clear that the action which attended this production, if inflammation, could not have been accompanied by the usual symptoms of such a state; and, indeed, from having compared very attentively these specimens with others, about the nature of which there could be no mistake, the resemblance was so striking, that I cannot help, in the present state of our knowledge, from marking them down as two additional exceptions to Louis' general rule, viz. that tubercles in the lungs take precedence of a like occurrence in other parts of the body.

In the following case, which is extracted from the catalogue of the museum, two well-marked scrofulous deposits were found in the liver, while the other viscera of the body were perfectly free from any such affection. The diseased portion of liver was made into a preparation, and may be seen by all and sundry, whose object may be the investigation of science, or pursuit of their profession; for through the liberality of Sir James M'Grigor the museum is open to the public.

Serjeant Michael M'Cadden, aged 29, 97th regiment, for some years previous to his death had been subject to repeated attacks of cholic and diarrhœa, and had been latterly in hospital for general anasarca, chronic catarrh, and well-marked diseases of the liver. He was ultimately admitted into the General Hospital, at Fort Pitt, complaining of cough, pain at the chest, and mucous expectoration, with inability to lie on the right side. Afterwards, œdema of both the upper and lower extremities took place. He gradually grew worse and died, after having been for a long time an invalid.

Post-mortem examination. — Brain healthy. Each pleura contained about six ounces of serous fluid. The mucous membrane of the bronchi generally in both lungs was much thickened, and of a red colour; and in the upper lobes of both lungs they were much enlarged. Moreover, the upper half of the right lung was hepatized, and both were very œdematous. The liver was, upon the whole, small; and in the lower and posterior part of the left lobe were two depositions of concrete inorganized scrofulous matter, of the size of hazel-nuts, firm to the touch, and of the common cheesy characteristics. Otherwise the organ was healthy, as were the other viscera of the abdomen.

In the following interesting case the abdominal cavity generally was much affected with tuberculous disease; and not only was the pleuritic surface of the diaphragm on each side much studded with tubercles, but the very adhesions which connected the visceral and parietal layers were likewise so, while both lungs were perfectly healthy.

James Green, aged 27, 28th regiment, for three years previous to his death was affected with chronic buboes in both groins, which frequently suppurated, but which never shewed any disposition to heal. During the whole of this time he was in and out of hospital; he, on the whole, however, kept gradually emaciating, and for some months before his death was continually confined to his bed. Seven weeks before he died, his abdomen began to swell, and he frequently complained of darting pains in it; his urine became high coloured and scanty; his lower extremities became œdematous; he was attacked with frequent vomiting; his abdomen went on enlarging, and hectic fever and death quickly ensued. On examination after death, eight pints of a yellowish-coloured fluid were found in the cavity of the peritoneum, and the serous coat of the abdominal viscera generally was covered with shreds of

recently effused lymph. In the subserous tissue both of the parietal and visceral layers of the peritoneum, were numbers of small scrofulous tubercles, and along with these, and occupying the same situations, were numerous red-coloured spots, not unlike petechiæ. Both lungs adhered generally to the walls of the thorax by recent adhesions, and in these adventitious bands were numerous small tubercles. Beneath the pleura covering the convex surface of the diaphragm on each side, many tubercles were likewise deposited. The lungs contained none, and were in structure healthy throughout.

It is interesting to remark the great similarity which exists between this case and that of Adam, of the 60th Rifles, already detailed, on account of the universally cretaceous nature of the tubercles: in both, the abdomen was the chief seat of the disease; in both, tubercles existed beneath the pleura covering the convex surfaces of the diaphragm, and in the adhesions connecting its visceral and parietal layers; while in one the lungs were perfectly healthy, and in the other only a few existed in each superior lobe. And when we take into account the case of M'Cadden, just detailed, and add to these the two cases where the testicles were alone affected, the exceptions to M. Louis' general rule would appear not to be so few on this side of the channel.

In drawing up this paper it has been with me a matter of much regret that my time would not permit me to avail myself of the labours of Sir James Clark, and a number of other valuable English authors, "good and true," who have written on this head; and I have likewise to regret much the circumstance of my duty having pressed so hard upon me as to prevent me from benefitting by the extensive and very valuable collection of reports and returns which now exist at the Army Medical Board in St. James's place, and which have been compiled and arranged under the talented instructions of my excellent friend, Deputy Inspector-General of Hospitals, Dr. Gordon. I believe, however, that through the kindness of the Director-General, these records are open to the profession, and the circumstance is the less, therefore, to be regretted.

ON INFLAMMATION,

AND THE MOTOR POWERS WHICH CAUSE AND REGULATE THE CIRCULATION.

By Professor Graves.

[Continued from page 562.]

I have already spoken of the dilatation of the arteries and veins of inflamed parts, as being produced by something very different from mere distension; and that it is not of a passive but an active nature. That the larger vessels actively dilate can scarcely be doubted by any one who has observed the state of the temporal arteries in phrenitis, or apoplexy; that the veins have a similar power, may be observed on plunging the hands or feet into a hot medium, whether moist or dry. Blisters applied to the skin produce for the time increased size of the cutaneous veins; and sores on the leg may, when considerable and long continued, give rise to a varicose state of the veins. When a grain of sand falls into the eye, how sudden is the redness—how numerous the vessels which now appear gorged with blood! This change takes place in a few seconds, and, in my opinion, can be much more satisfactorily accounted for by supposing that the capillaries and smaller vessels enjoy a wide range of size, if I may use the expression, and are capable of enlarging or diminishing their calibre, according to the exigencies of the case and the state of the circulation. That the large arteries and veins do so, is acknowledged by all, and is proved by arterial trunks contracting on their contents so as to maintain their proper tension, no matter how much blood is drawn from an animal. The larger veins are capable of a like contraction and expansion: can similar properties be denied to the smaller arteries, possessing, as they do, an elastic coat proportionally thicker? The vascular phenomena attending a blush ought to have taught physiologists how rapidly, how instantaneously, blood may be drawn to a particular part, and may again desert it; and that, under circumstances where the *vis a tergo* could not determine a flow of blood to the part in question, more than to any other in the body. Do we need microscopic examinations of the capillaries of recently killed animals, to instruct us, when such

phenomena offer themselves, as it were, for the very purpose of illustration? When the child breathes for the first time, the air admitted into the lungs gives new energy to their capillaries, and at once the great current of blood flows through the pulmonary arteries, deserting the ductus arteriosus. In a seven months' child the latter passage is still very large; and yet, when the child breathes, its being open effects very little, if any thing, towards diminishing the flow of blood into the pulmonary arteries.

Here, again, we observe how arteries grow independently of mere pressure from within; for the pulmonary arteries and pulmonary veins are enlarging themselves long, long before they are called on to be channels for a quantity of blood at all proportioned to their calibres. John Hunter observed the enlargement of the arteries of an inflamed part, and his observations, and those of others, have brought to light a periodical and remarkable increase in the size of the vessels destined to promote the growth of the stag's horns. Are we, in this case, to explain that enlargement by obstruction, or by the *vis a tergo?* It is impossible to do so; and we must, then, look to the vessels of the part itself for a solution of the question. In such instances, as in the case of the pregnant uterus, these vessels are endowed with this power of growth and enlargement, quite independently of the general vascular system, or the action of its centre—the heart.

I am the more anxious to impress on you this view of the subject, as the hypothesis of obstruction has been adopted by many late writers, as explanatory of the local changes of circulation attending inflammation. Thus Dr. Williams, in his admirable lectures, published in the Medical Gazette (No. 528), says, "We cannot, in the present state of pathological knowledge, doubt that the circulation through the inflamed vessels is to a certain degree obstructed; whilst, either as a consequence of this, or from some co-operating influence, the vessels leading to the part become dilated, and being thus more open than others to the pulse-wave from the heart, they become the seat of that throbbing hard pulse that has been mistaken for increased action of the vessels themselves."

Now, gentlemen, you observe here, that Dr. Williams expresses himself doubtfully about the dilatation of the vessels being caused by obstruction, and he even speaks of some co-operating influence. We shall therefore content ourselves with having recited his opinion on this point. I must observe, however, that the dilatation of the vessels, *however caused,* can, on no principle, account for their becoming the seat of throbbing, and a hard pulse: their being more open than others to the pulse-wave from the heart, could at the utmost only place them in the situation of other arteries naturally of the size they have now attained to; but we do not find that such arteries throb, or have a hard pulse. Arteries do not throb, or become the seat of a hard pulse, in proportion to their size; that is not the fact, and consequently Dr. Williams's explanation cannot be admitted.

Dr. Weatherhead, who has arrived at very nearly the same view of the subject with myself, says, "the first effect of an excitant or irritant, applied to any part of the body, is to attract the blood to the seat of irritation, and to quicken its current in the capillaries." So far we perfectly agree. Here Dr. Weatherhead estimates the vital energy of the vessels of the part at its true value, and does not call in the aid of an increased *vis a tergo* to account for an augmented determination of blood to any particular locality; but to what follows I cannot concede. "If these effects be kept up beyond a certain period, or carried beyond a certain degree, the excitation continues to attract as much blood as before, while the power of the capillaries to forward it diminishes, by the exhaustion ensuing from their prolonged over action." There seems but a weak analogy in support of the assertion that increased vascular action must necessarily produce vascular exhaustion.

It may be objected to my view, that dilatation of an active nature cannot be conceded to the capillaries, whose coats are quite thin and membranous; but when the objects are so minute, it is quite impossible to determine the physical or vital powers of tissues; and we should recollect that what is deficient in degree, may, in the case of the capillaries, be made up by their number, which is immense in every part of the body. Still, so far as our observations do go, they seem to esta-

blish the property in question. Müller, whose opinion upon all physiological questions is of the greatest weight, has adopted on this subject an hypothesis which appears to me to be quite untenable. It is observable that the first of the following paragraphs, which I quote from his work, proves that, when writing it, he felt conscious that the remarkable phenomena of *vital turgescence* are totally irreconcileable with the theory, which denies any permanent circulating power but that of the heart, and which asserts that "the motion of the blood in the capillaries is wholly dependent on the heart's action!" Let us hear what he says concerning *vital turgescence of the blood-vessels*, (p. 224):—"Although it be denied that the circulation is in any way aided by an attraction between the blood and the capillaries, the existence of such an attraction or affinity may, nevertheless, be admitted in the instance of the 'turgescence, turgor vitalis, or orgasm,' which is observed to take place in certain parts of the body, independent of the action of the heart. This condition of turgescence in animals is analogous to phenomena which are so evident in plants, such as the afflux of sap to the fruit-bud, which contains the impregnated ovum.

"*The mutual vital action, or affinity between the blood and the tissues of the body*, which is an essential part of the process of nutrition, is, under many circumstances, greatly increased; and an accumulation of blood in the dilated vessels of the organ is the result. It is seen, for example, in the genitals, during the state of sexual desire, in the uterus during pregnancy, in the stomach during digestion, and in the processes of the cranial bones on which the stag's antlers afterwards rest, during the reproduction of these parts. The local accumulation of blood, with the dilatation of old, and the formation of new vessels, is, however, seen most frequently in the embryo, in which new organs are developed in succession by a process of this kind; while, on the other hand, other organs, such as the branchiæ of the salamander and frog, and the tail of the latter animal, become atrophied, and perish, as soon as the vital affinity which existed between the blood and their tissues ceases to be exerted.

"The phenomena of turgescence have been supposed to be dependent on an increased action or contraction in the arteries. But arteries present no periodic contractions of muscular nature; and a persistent contraction of the arteries, unless it were progressive, (vermicular, as it were, or aided by valves arranged in a determinate direction,) would be quite inadequate to produce a state of turgescence of any part.

"To explain the state of orgasm of the uterus during pregnancy, and of the bony processes which bear the antlers of the stag, we must presuppose the existence of an increased affinity between the blood and the tissue of the organ. This condition may be excited very suddenly, as is seen in the instantaneous injection of the cheeks with blood, in the act of blushing, and of the whole head, under the influence of violent passions; in both of which instances the local phenomena are evidently induced by nervous influence. The active congestion of certain organs —of the brain, for example—while they are in a state of excitement, is a similar phenomenon.

"If the organ which is susceptible of the increased affinity between the blood and the tissue is, at the same time, capable of considerable distension, tumefaction and *erection* take place."

It will, I believe, be readily acknowledged that Müller's explanation is, after all, a mere hypothesis. Is this affinity between the blood and the tissues of the body, chemical; or is it a mutual vital action? If the latter, then the vessels, *they being the only tissues in contact with the blood*, are active, contrary to his previous hypothesis. As to the chemical explanation of a *blush*, it surely does not merit examination.

The facts referred to by Müller, in the above passage, all tend to corroborate the view I have adopted, and shew that local changes of nutrition, vascularity, and circulation, may be quite independent of the heart's action.

We must next turn our attention to the increase in size of some of the larger arteries.

"Apply a ligature," says Dr. Hall, "to the principal artery of a limb, the circulation is then carried on by the collateral branches, which become enlarged for this very purpose, and in consequence of the obstruction." Now let us study the phenomena a

little more accurately, and we shall soon see how erroneous is this explanation.

In the first place, what are the physical results produced by tying one of the large arteries of a limb? The *vis a tergo*, or propelling power of the heart, continues just as before; the quantity of fluid or blood within the whole system of arterial tubes is unchanged, while the forces to be overcome by the circulating power remain also the same. In fact, all the general physical conditions are unaltered after the ligature has been applied, except that a portion of the blood can no longer enter the tied artery. Let us now investigate what effects this non-entrance of a certain portion of the blood into its accustomed channel is likely to produce on the rest of the arterial system. When the principal artery of a limb is tied, the blood circulating in the remaining arteries of the body, and the other arteries of that limb, is pressed more strongly against the arterial parietes. But as the distending force resulting from this increased pressure is not confined to any particular artery of the body, but affects all, more or less, it is obvious that a power so extensively distributed and subdivided can exert but little distending influence on any individual artery, or, in other words, can tend but little to dilate any of the arterial tubes. Now it is obvious, from the laws of hydrostatics, that this increased pressure will be more exerted *in proportion* on the main collateral arteries of the limb, than on the smaller; it will, in truth, be scarcely sensible in the latter, and yet these are the very arteries which enlarge first after the operation for aneurism. The increase in the size of the arteries, commences, not where it ought to commence, if it depended merely on dilatation from increased pressure, viz. in the larger arteries, and in the collateral branches close to the ligature, but it commences in the smaller and more distant arterial ramifications. In addition to the fact that a proportionally less pressure is thrown on the smaller arteries, we shall recollect that the latter have parietes much thicker in proportion to their calibre, than the larger branches. This is another material objection to Dr. Hall's explanation of their increase in size. What are the phenomena observed after applying a ligature to an artery of large size, where a sufficient collateral circulation may be applied? First, the sudden diminution of circulation in the parts below the ligature gives rise to coldness and paleness of the limb; but in a few hours the circulation gradually returns, the thermometrical temperature of the limb rises, and the activity of the capillary system is greater than in the natural condition of the limb. This excitement continues for some time, and then diminishes to the ordinary standard of health. In eight, twelve, or twenty-four hours after the application of a ligature to the main artery of a limb, we find the skin of the parts below the ligature pale and cool, but in a few hours afterwards its temperature rises, and it exhibits an evidently increased arterial action. Now it is difficult to conceive that the main collateral branches have been dilated in so short a space of time. The mode in which the phenomena witnessed in this instance are best explained, seems to me to be the following. When a large portion of the blood destined for the supply of a limb is cut off, all the tissues of a part so deprived receive a shock: the muscles, nerves, capillary vessels—in fact, the vital functions of the whole—are more or less affected. After some time, however, the vital depression is followed by reaction, and this commences in the smaller arteries and capillary system, its commencement being marked by uneasy sensations, increase of temperature, and arterial throbbing. The initiative of the restoration of the circulation belongs to the extreme vessels, which take on an increased action, and this is gradually extended to larger arteries. These gradually augment in power, become enlarged and distended, and at length the circulation of the affected limb is restored to a state of efficacy, equal, if not identical, with its pristine condition. Now, you are told that the increased activity of the capillary vessels, in this instance, is referrible to the *vis a tergo* operating through the anastomosing branches. This is a false assumption. In the first place, the influence of the heart's action, when the blood passes through small anastomosing branches, and by circuitous courses, must be less powerful than before the operation, when the main channels remained pervious.

The *vis a tergo* is therefore lessened,

and yet the capillary distension is greater than before the operation, or in the sound limb. In the next place, this argument is of more force, when it is considered that the enlargement commences in the smaller, and gradually extends to the larger vessels; and also, that in some cases the branches from the main artery through which the blood must pass, do not become perceptibly enlarged. Thus Mr. Hodgson, in the work on diseases of arteries, says—"The dilatation takes place principally in minute ramifications. The trunks, and the mouths of the vessels going off above the place of obstruction in the main artery, in several preparations, did not appear larger than in their natural state, and in a few instances only a slight dilatation was perceptible." From these facts it is obvious that the vessels least under the influence of the heart are the most dilated. But the most decisive proof is the return of the capillaries and minute arteries to their ordinary size, at the time, when, the larger branches being dilated, the *vis a tergo* should be more operative. Hence it would appear that the power of distension resides in the arteries themselves, the irritation commencing in the capillaries, and being sympathetically propagated to the larger vessels. This is further confirmed by the fact, that, if an inflamed part, or a vascular tumor be supplied by several arteries, and one of them be divided, the others will enlarge.

[To be continued.]

NEW SLING IN INJURIES ABOUT THE SHOULDER.

To the Editor of the Medical Gazette.

Sir,

Having recently suffered from an injury which is not very common, and having adopted a mode of treatment in some respects varying from that usually resorted to, I have taken the liberty of transmitting the particulars of the plan for your perusal; and should you consider the case worthy of insertion in your valuable periodical, it is at your service.—I remain, sir,

Your obedient servant,

W. J. Keal.

Oakham, June 5th, 1838.

On the 30th day of December, 1837, my horse, under the impression of fear, bolted off the road, and passed rapidly by a tree, with which the right side of my body came violently in contact. The injuries received were—1st, dislocation of the scapular end of the clavicle, the tree striking the bone in front an inch from its external end. 2nd, fracture of three ribs, viz., two true, in the convex part near their attachment to the vertebræ, and one false. 3rd, a deep contused wound of the skin and soft parts of the crista of the ilium.

The surgical friends who were called in to render me their assistance, applied bandages and rollers to the body and arm, according to Desault's plan; intending, on their next visit, to resort to the late Mr. Earle's apparatus. Before, however, I saw them for the second time, I was continually putting my arm and shoulder into various positions, to learn in which of them the end of the bone kept *in sitû* with the greatest facility; and I soon completely discovered, that when the elbow was drawn backwards and pressed to the body, in such a position that the forearm formed a right angle with the trunk, with the additional aid of a soft pad in the axilla, the clavicle was only thus preserved in its natural situation. My object was now to secure the arm in this position as perfectly as I could, with as little bodily inconvenience as possible. In the first instance I adopted the ordinary body belt, with the addition of two buckles on the right side, placed so as to receive two straps attached to a circular tick-bandage round the upper arm, padded on the under side so as to fill the axilla (according to Boyer's plan), in order to fix it securely to the side of the body. Being inconvenienced by the partial pressure of the buckles, as well as by the benumbing effects of the arm-straps, I had recourse to an unbleached calico body-roller, ten inches broad. By the means of this, the undue expansion of the chest was counteracted, for as yet I suffered more pain from the fractured ribs than from the dislocated clavicle, and was unable to lie in bed during the first thirteen nights. In addition to the body-roller, in order to fix the arm to the side and to retain the clavicle in its right position, I employed a second roller of the same material, about six inches broad,

which was first passed twice round the body, and then being continued from the left side across the back was passed under the right arm, brought round the fore part of the same, and returned across the back, encircling the body and upper arm four or five times, thereby binding the latter to the trunk, at the same time that the fold over the arm prevented its being moved forwards. As the fore-arm was now at a right angle with the body, I became fully persuaded that the ordinary sling was inapplicable, the effect of it being to pull the shoulder forward, and the elbow from the side; I therefore contrived a sling upon a new principle, of which I now attempt to give you a description. The sling itself in which the fore-arm rested was made of strong leather, encircled by two straps with buckles, to which were attached by two other straps a piece of webbing, which passed over the right clavicle across the shoulders, under the left arm, and the strap at the end fastened to the first buckle of the sling. My surgical friends so fully approved of the plan as at once to relinquish their former intention of using Mr. Earle's apparatus. At the end of two months I put on my coat, though I believe that I was premature in so doing; I consider that it would have been more prudent to have waited another month. I am convinced that the apparatus which was recommended by by the late Mr. Earle is by no means suitable in this injury, as the least elevation of the humerus raises the end of the clavicle from its situation; likewise the position of the hand and arm, when confined in the sleeve across the body as he directs, dislocates the clavicle by pulling the acromion process under the end of it. Yet in fracture of the cervix scapulæ it is especially applicable. From the experience which I have acquired under my late injury, I am persuaded that a similar six-inch-wide bandage with the like sling, differently worn, are the most effectual for keeping the ends of the bone in apposition, when the clavicle is broken. The sling in this case is to be worn over the uninjured shoulder, passing across the back, under the arm of the injured clavicle. I am likewise thoroughly confident that, in every case where a sling is required, the one now recommended is infinitely preferable to any other. At the present moment I find that my shoulder is much more comfortable in this sling. In the ordinary one the deltoid, biceps, and pectoral muscles, are under a painful restraint. I am more sensible of this, perhaps, in consequence of the severe contusion that was inflicted upon these muscles at the time of the injury; yet it indicated that the muscles were not in a free and passive state, which I conceive to be a great desideratum.

Puncture of the Sac in Ganglion of the Wrist.

Being consulted a short time ago in a case of ganglion on the wrist, which had been variously treated by others with leeches, blisters, &c. without relief, I first attempted to burst the sac by three or four blows with a book, but did not succeed. In three or four days, when the effects of the blows had subsided, I had recourse to puncturing the sac; and for this purpose used Mr. Davis's puncturing needle; and so freely did the viscid fluid escape through the groove, that I am satisfied of its superiority over the cataract needle, as recommended by Mr. Key (Med. Gaz., vol. xix. page 380.)

EFFECTS OF SWALLOWING NEARLY AN OUNCE OF CALOMEL.

To the Editor of the Medical Gazette.

Sir,

The following case, which has just occurred to me, is perhaps deserving of a lasting record, as an interesting experiment of the effects of a common remedy on the human subject; being, as far as I am acquainted, the only one of the kind that has ever been published:—

Miss H——, a healthy girl, 19 years of age, feeling languid and rather feverish, took, at 11 o'clock on Monday morning, two tea-spoonsful of what she supposed to be magnesia, but what was afterwards found to be calomel *, in half a tea-cupful of milk; taking care to rinse out the cup, so as to swallow within a few grains of the whole. The mistake was not discovered till 1 o'clock, when I was called in to see her; of course she was in a state of considerable agitation. After soothing her by assuring her she was likely to do well, and

* It had been purchased at Apothecaries' Hall.

giving her half a drachm of pulv. ipecac., I began to inquire more particularly into her sensations: she observed, that on swallowing the calomel she had remarked something peculiar in the taste of it; no symptom had, however, appeared, except slight nausea and faintness, which had not prevented her from partaking of a luncheon of bread and salad, with a little ale. Ten minutes having elapsed in these inquiries, during which she had drunk a tumbler of warm water, and vomiting not having ensued, I repeated the pulv. ipecac., which was scarcely swallowed before copious vomiting followed. On examining the egesta, by letting it stand quiet to settle, and then pouring off the upper part, and testing the sediment with lime water, I could detect no traces of the calomel. I now gave her half a tumbler of lime-water, mixed with an equal quantity of warm water, every three or four minutes, and she vomited seven times in the course of the next half hour: every thing she had eaten since taking the calomel came off her stomach; still, however, there were only slight evidences of the calomel—a few blackish specks here and there: she now complained of a weight and pain at the epigastrium, and in the right side; a few minutes afterwards she brought off her stomach a thick ropy stuff, consisting almost entirely of undecomposed calomel, entangled in a quantity of mucus. I should suppose, on a rough calculation, that there must have been at least an ounce of calomel. The patient expressed herself relieved from the pain and weight; and, as I believed, the most formidable part of enemy was expelled, I considered it the mildest course to be contented with neutralizing the remainder, and purging it from the bowels. My patient was undressed and put to bed, the vomiting did not recur, and (2 o'clock) the following mixture was prescribed:—

℞ Magnesiæ Calcin., ʒiij.; Magnes. Sulph. ʒiss.; Sp. Ammon. Arom. 3ij.; Liq. Calcis. ʒxiv. M. Cap. coch. iij. amp. quâque semihorâ.

At 4 o'clock she was complaining of slight headach; her pulse was full, hard, and about 90. She had not vomited since I last saw her, and had been lying quiet; half the mixture was taken, but the bowels had not been opened. She was directed to continue the mixture, and to take frequent draughts of milk and water to allay thirst, which was urgent.

I saw her again at 8 o'clock; the whole of the mixture was consumed, and her bowels had been opened four times between 5 and 6 o'clock; the stools had unluckily been thrown away, but I was informed that when the chamber utensil was emptied, a quantity of black particles, similar to those which the attendants had observed precipitated from the vomited matters when I poured lime-water over them, were seen adhering to the bottom of it. She was now suffering from severe griping pains, which had come on within the last half hour, and there was some tenderness of the abdomen; the pulse was 100, small and hard; the tongue had a narrow streak of fur down the centre. She had vomited once at 5 o'clock, simply the milk and water she had been drinking, without any appearance of calomel. The following mixture was prescribed:—

℞ Misturæ Cetacei, ʒxij.; Vini Ipecac. 3iss.; Tinct. Hyosciam. 3iij. M. Cap. coch. ij. amp. omni horâ.

10½ o'clock.—Has vomited twice after each dose of the mixture; is relieved from the griping, but has yet some tenderness of the abdomen.

To omit the medicine for a little while, and then resume it.

Tuesday morning.—The mixture has not made her sick since the last report; has very little abdominal tenderness. The bowels have been opened once; the stool was watery, green, and small, without any admixture of the black matter noticed yesterday evening; has some headach; tongue thinly coated; pulse 90, small and hard; has no appetite whatever; is teased with a frequent tenesmus, and complains of soreness round the fundament. Slept during the night from three till eight, and is disposed to sleep at the present time.

To have a little arrow-root or mutton broth, according to inclination.

Rep. Mistura 2da quâque horâ.

In the evening she was sitting up, free from all complaint, except a little pain or heaviness across the forehead, languor, some slight tenderness of the abdomen on firm pressure, and an uneasy sensation on gnashing the teeth. Has had two stools of a dark green colour, and accompanied with some griping pain.

Wednesday. — Appears quite well; says she had an unpleasant taste in the mouth on first awaking in the morning, but the gums are not in the slightest degree affected; has a good appetite.

My patient called on me this morning (Friday), and I cannot remark any effects from the calomel; she says her health is as good as it was previously.

The above case calls for no comment; but if you think it worthy of publication in your journal as something out of the common routine, it is at your disposal.—I remain, sir,

Your obedient servant,

H. P. Robarts.

10, Everett Street, Russell Square,
June 29, 1838.

NEW MODE OF EXHIBITING COPAIVA.

To the Editor of the Medical Gazette.

Sir,

I have just seen the number of your journal for June 16, in which is an article headed, "New Method of preparing Copaiva for Administration."

New it may be to the profession generally, and probably is so altogether as regards the exact mode there recommended. But there are others as well as myself who have exhibited copaiva for some years past in a form very similar, quite as efficacious, and attended with much less trouble.

The following is the method I have generally adopted, first premising that the article used must be *good* copaiva, *not* such an article as many druggists supply under that name:—Put any weight of copaiva into a vessel which will admit of its being conveniently stirred, and gradually add to it, constantly stirring until they are thoroughly incorporated, one-sixteenth of its weight of pure calcined magnesia; set it by for a few days, and it will then be found of sufficient consistence to admit of its being formed into pills or boluses of any shape. When about to use it, wet the hands, spatula, and slab, with water, which will prevent the mass adhering to either: form half-drachm oval boluses; then cut a slip of wafer-paper, such as confectioners use for baking biscuits on; moisten it with water, and roll the bolus in it.

Before taking one, place it in water for a minute or so, for the purpose of softening the envelope, and it will then slip down the œsophagus much easier than a small pill, and leave no trace behind it in the shape of a disagreeable taste: in fact the dose is perfectly insipid, and in this form it seems to be more grateful to the stomach, for I never have observed that loathing of the medicine and food which so frequently occurs during the ordinary exhibition of this medicine.

The gratification which many of your readers must derive from any method enabling them to exhibit this valuable medicine to their patients, free from its disagreeable flavour, is the only apology I can offer for troubling you with this communication.—I am, sir,

Your obedient servant,

Charles Evans.

9, Upper East Smithfield,
June 27, 1838.

TREATMENT OF VARICOSE VEINS.

Portsmouth, 3d July, 1838.

My dear Sir,

As you have taken some interest in my attempts to radically cure varicose veins, I beg to send you the accompanying case, which has proved very successful, and I think the plan of using the common surgical needle, and turning its edge up to the vein, very effectual, as complete ulceration through the vein soon takes place, thereby most effectually obliterating it. The subject of this case seems to feel very grateful for what has been done, and says he has not felt so comfortable for many a year, and being constantly in hospital before with indolent ulcers he was an inefficient soldier.

I shall have much pleasure in communicating to you the result of any other subject I may meet with.

I beg to remain

Yours very sincerely,

A. Melvin.

Dr. Gordon,
Deputy Inspector General, &c. &c.

The following case of varix, which has been treated here, is very satisfactory indeed in its result; I therefore forward it to you:—

Private Jas. Gleeson, aged 30 years, 84th Regiment, a very delicate looking

soldier, was admitted into the hospital of his regiment 19th May, 1838, for indolent ulcer of the right leg. Has been subject to ulceration of lower extremities for ten years; legs covered with dark-coloured cicatrices.

Great enlargement of venous branches of both lower limbs, much increased on assuming the erect posture. Saphena trunks as large as his thumb, and somewhat tortuous; in right leg there are two venous sinuses nearly as large as walnuts. In consequence of these affections this man was brought forward to be discharged the service, at the last half-yearly inspection, as an inefficient soldier, and the ulcers shewing no disposition to heal.

On the 26th May I inserted four common curved surgical needles under the enlarged veins of the right leg; a ligature was then passed firmly over the needles in the form of the twisted suture, to compress the veins. The needles were then turned down, so as to bring the sharp edge in contact with the vein, a bit of cork fixed on the points of the needles, and the whole retained by adhesive plaster.

3rd June.—Ulcers granulating rapidly. Two of the needles have ulcerated through, and the ligatures on the remaining two have become so loose as to be easily removed. Fresh ligatures were applied.

4th.—The operation was this day performed on two veins of the left leg, precisely in the same manner as formerly, and a large needle was passed under a venous sinus on the right leg, which had not been obliterated by the former operation.

7th.—Some puriform discharge from around the needles.

12th.—Ligatures on all the needles tightened. The patient expresses himself grateful for the relief he has obtained, and says the pain was nothing.

By the 21st the whole of the needles had ulcerated through; the ulcerated parts were dressed a few times with a poultice, there being some inflammation. On the 27th the man was discharged to his duty, cured.

A. Melvin,
Surgeon to the Forces.

Portsmouth, 3rd July, 1838.

OXALATES AS REDUCING OR DEOXIDIZING AGENTS.

To the Editor of the Medical Gazette.

Sir,

Formate of soda has been recommended by Goebel as a powerfully reducing substance. Formic acid consists of two atoms of carbonic oxide and one of water:—

$$\dot{C}\,2\,\dot{H}$$

By the application of heat to an earthy formate, carbonic oxide and water are driven off, and the metal or its oxide is left; whereas, in the case of an alkaline formate the carbonate remains. Goebel recommends formate of soda in preference to charcoal or black flux for reducing arsenious acid, sulphuret of arsenic*, &c.

From analogy in constitution, it occurred to me that an oxalate might be substituted for a formate, and upon trial this was found to be the case. On account of the generally supposed difficulty of preparing the formate of soda, and its consequent scarcity, an oxalate recommends itself more especially. Dobereiner's process for artificial formic acid† is simple and easily conducted, by the distillation of one part of tartaric acid, one and a half of peroxide of manganese, and one and a half of sulphuric acid, diluted with about two and a half parts of water, in a common flask furnished with a tight-fitting cork and conducting tube. A common phial, containing a little water or carbonate of soda, may be used as a condenser. The odour of formic acid, which is strongly smelt during distillation, is peculiar, resembling that of rotten apples or turnips, and would lead us to suspect its formation in those substances by natural decomposition. May not formic acid be obtained for manufactures from apples or turnips?

Oxalic acid is composed of one atom of carbonic oxide, one atom of carbonic acid, and three atoms of water‡:—

$$\dot{H}\,\dot{C}\,\ddot{C}\,\dot{H}2$$

Oxalate of soda consists of an atom

* Griffin's Chem. Recreat. 8th edit. p. 140.
† Ann. of Phil. new series, iv. 311,
‡ Phil. Trans. Part I. 1837.

of anhydrous oxalic acid, and an atom of soda, and may be prepared by saturating carbonate of soda with oxalic acid. By the application of heat this salt is converted into carbonate of soda and carbonic oxide; every 100 grains of anhydrous oxalate of soda yielding 70·09 grains of carbonate of soda, and 20·91 grains of carbonic oxide; while 100 grains of formate of soda should yield 40·57 grains of carbonic oxide, 13·18 grains of water, and 46·3 grains of soda. The residuum of ignited formate of soda is carbonate, and not caustic soda, as we might *à priori* expect. Arsenious acid, or sulphuret of arsenic, in order to be reduced, may be mixed with three parts of oxalate of soda, and introduced into a tube which has been previously heated, in order to expel moisture. The tube must be held about two inches above the top of a spirit-lamp flame half an inch long for a minute, and then gradually depressed until the sealed end of the tube be immersed in the flame. By this gradual application of heat, the decomposing point of the oxalate is attained before the escape of any oxide or sulphuret of arsenic—a circumstance which frequently happens when the heat is applied suddenly. It is particularly desirable to have such a reducing agent as an oxalate or formate of soda for a class of students, whose first attempt at reduction frequently fails. The soiling of the tube, which often occurs with the tyro in the case of charcoal or black flux, never takes place in that of the oxalate or formate. The form of tube which I use is that recommended by Professor Clark, of Aberdeen; it may be from an eighth to a tenth part of an inch wide, two inches long, and having the sealed end blown into a bulb. By the above procedure sulphuret of antimony, sulphuret of cadmium, nitrate of silver, perchloride and protochloride of mercury, acetate of lead, sulphate of copper, &c. were instantly reduced. Fixed substances, heated to redness in a glass tube by a spirit-lamp flame, may be deoxidized by passing a stream of the carbonic oxide over them.

From many comparative experiments I prefer the oxalate of soda to the formate, and for three reasons:—

1st, The oxalate is more easily procured—a consideration which will weigh materially in its favour, in the event of its introduction into manufactures.

2dly, It is anhydrous, whereas the formate gives out a considerable quantity of moisture.

And 3dly, It does not swell or froth so much by fusion as the formate does. In a small bulb the rapid application of heat causes the formate to froth much, and obscure the metallic crust.

Of many oxalates experimented upon, that of soda is to be preferred.

Your obedient servant,

R. M'Gregor,

Member of the Faculty of Physicians and Surgeons; and Lecturer on Chemistry in Portland St. School of Medicine, Glasgow.

June 25, 1838.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'Alembert.

A Practical Compendium of the Materia Medica, with numerous Formulæ, adapted for the Treatment of the Diseases of Infancy and Childhood. By Alexander Ure, M.D., Member of the Royal College of Surgeons, London. London, 1838. 12mo. pp. 222 and lxx.

This useful Compendium is founded upon Fränkel's *Practische Heilmittellehre für die Krankheiten des kindlichen Alters*, lately published at Berlin; and throughout the book Dr. Ure chiefly, though by no means exclusively, gives the practice of the physicians of Germany in the diseases of children. Hence this Compendium is an excellent supplement to our ordinary dispensatories and compilations on the Materia Medica, the authors of which seem in general to have drawn but little from the vast stores of German literature. There is a preliminary dissertation sixty-six pages in length, consisting of very sensible essays on blood-letting, blistering, emetics, enemata, bathing, and cold. We then come to the Compendium itself, the following extracts from which may serve to shew the nature and style of the work:—

"Cupri Sulphas is endowed with properties similar to the above, in small doses; and in large doses is emetic. It exercises a tonic and astringent power on the nerves of the intestines, in

ulceration of, and morbidly increased secretion from, their mucous membrane. It also checks excessive mucous secretion from the mucous lining of the bronchial tubes and cells. In these cases it ought to be combined with opium. It has recently been much extolled in Germany as a remedy in croup. Hoffmann was the first who prescribed it in that complaint instead of calomel. During the prevalence of epidemic croup he gave it in cases of bronchitis and tracheitis, in the dose of $\frac{1}{4}$ to $\frac{1}{2}$ gr., according to the age of the child, at intervals of two hours. Were laryngitis present, he ordered the sulphate of copper, in the dose of 3 or 4 grains, after depletion, in order to excite vomiting, and followed it up with the above small doses every quarter or half hour. After him, several other medical practitioners have successfully pursued the same line of treatment. Dr. Serlo (Hufeland's Journal, Bd. 78) cured several cases of croup by its means. To an eighteen months' child he gave in the first instance, to provoke emesis, 3 gr. of sulphate of copper, with 6 gr. of sugar; and then every two hours, $\frac{1}{4}$ gr. with 5 gr. of sugar. To a child aged two and a half years, he gave in like manner, first of all, 3 gr., and then $\frac{1}{3}$ grain every two hours: from 25 to 30 grains were generally consumed in the course of cure. Hufeland strongly recommended the use of copper, in conjunction with leeches and enemata, in the same ailment. In commenting upon some observations on the efficacy of sulphate of copper, in croup, by Dr. Droste (*Heidelberger Klinische Annalen*, Bd. 10, 1834), Harless observes, that without calling in question the utility of the remedy in the instances cited, calomel, in virtue of its power of resolving plastic formation, ought ever to take the first place, in the marked catarrho-inflammatory forms of croup, or where the effused lymph is firm and membraneous."

After detailing the diseases in which the aqueous solution of chlorine gas is used, he says—

"*Dose and form of exhibition.*—Internally, Pfeufer gave children, from three to six years old. ℥ss.—℥j. in the twenty-four hours. Kopp gives ʒij.; but in urgent cases, as violent angina, incipient stupor, he prescribes from ℥ss. —ʒvj. in the same space of time. Braun advises it to be given in the quantity of a tea-spoonful every second or third hour to children three or four years old. The most suitable vehicle for it, is distilled water sweetened with syrup. Wittke (*Medizinisches Conversationsblatt*, 1831) gives, to infants of six months old, ʒj. daily in divided doses, to those of a twelvemonth ʒiss., and to those under two years ʒij. The solution of chlorine has been applied externally in the form of linctus (with equal parts of honey) in stomatitis, asthenic sore throat, and aphtha. It has also been used as a liniment in *tinea capitis;* and as a gargle in the proportion of ℥ss. to ℥vj. of pure water."

We will now mention very briefly a few other points worthy of notice, some of which will be interesting to our readers for their deviation from ordinary English practice, and others where some mistake has probably crept in. We do not, however, pretend to distinguish too accurately to which of these classes each point may belong; for who shall venture to settle the bounds between eccentricity and error?

The first prescription at page 1 is rather a poser. We are directed to take two drachms of wormwood, and half an ounce of valerian, and after macerating them in a q.s. of boiling water, we are to add an ounce of syrup of oranges to two drachms of the strained infusion. Perhaps for ʒij. we ought to read ℥ij.; but even then the formula is defective in omitting the quantity of the boiling water. The quantity of valerian is the same as the London College order to be infused in twenty ounces of water; add four more to allow for the wormwood, and we shall then have twenty-four ounces of infusion, all to be rejected except two drachms or two ounces, whichever may be the true reading.

Muriatic acid is given by Dr. Thiel as a specific in hooping-cough, beginning with ʒij.—ʒiij. in the day, and increasing the quantity to ℥ss. or ʒvj. Dr. Ure's doses, in the table at the end of the work, are more reasonable; they are from three to ten drops for a child from one to three years old, and from five to ten drops for a child from four to seven years old. Here, however, as throughout the table, it is not stated how often the medicine is to be taken.

Under the head of *Antimonii potassio-tartras* (p. 19), we are directed to give a tea-spoonful of an antimonial mixture every quarter of an hour until vomiting occurs. This method is disapproved of by Dr. Merriman, who says in his edition of Underwood, "'Let it be given every ten or fifteen minutes till it produces vomiting,' is a very common but very injudicious prescription. No emetic ought to be persevered in if the child, after two or three suitable or usual doses, does not cast its stomach, unless under the especial direction of some skilful practitioner."—P. xix.

The late Dr. John Clarke used to mention a case in his lectures, where two grains of tartar emetic "were dissolved in an ounce of water, of which half a tea-spoonful was given every quarter of an hour till it produced vomiting, which, when it took place, never ceased till the child died*." Dr. Clarke, however, does not disapprove of ipecacuanha administered in this manner.

The next preparation mentioned, is the *Pulvis Jacobi verus*, which is always called, in this work, *vera*—a most unusual gender for *pulvis*. Propertius, indeed, has *horrida pulvis;* but we rather suspect that the Italian *polvere*, or French *poudre*, was running in our author's head, and confusing his classical recollections.

The *Argenti nitras* has been applied, it seems, by some French practitioners, to the pustules in small-pox, in order to stop their development. Experience, however, has shown the practice to be highly dangerous. The author prescribes this remedy in the dose of one-eighth of a grain three times a day; this would now be generally reckoned rather a large dose for a child; yet Underwood says, "Dr. John Wilson, of Spalding, has lately strongly recommended the internal use of the argentum nitratum, in doses of gr. iiss. three times a day." (Merriman's Edit., p. 270.)

Saffron, we see, has not lost the confidence of our German brethren; according to one author it may be given to infants in the dose of 1-8th, 1-6th, to half a grain; but in the prescription by Fränkel, which immediately follows, a grain is ordered every hour for an infant, together with half a grain of musk and three grains of chalk; and six such doses are to be made up. (P. 41.)

Cautious as we would always be in the administration of narcotics to children—impressed as we are with the conviction that *saltem non nocere* is the first principle in medicine—we almost doubt whether Dr. Ure is not too cautious in his exhibition of the *Extract of Belladonna*, to children under four years of age. His doses are—to infants under two years old, one-ninetieth of a grain; to children from two to four years, one-forty-eighth of a grain three or four times daily; to those more advanced, one-tenth, one-eighth, one-fourth of a grain several times a day. In the table at the end of the work he is more liberal, allowing 1-20th to 1-8th of a grain for a child from one to three years of age, and 1-8th to 1-half of a grain for a child from four to seven years of age. This last dose of half a grain is too large for a child; or, at any rate, too large to be repeated the same day, if the drug is of first-rate quality*.

Dr. Jackson recommends this medicine in hooping-cough in quantities which would certainly be dangerous if the extract were well prepared; such as 1-6th of a grain to a child three months old, every three hours, from "sunrise" to bedtime; and a grain to a child two years old. His paper affords an instance of the diffieulty of fixing doses, arising from the badness of drugs, for when he first tried the

* The London Practice of Midwifery, 5th edit. page 307.

* Dr. Young's rule is, that "for children under 12 years old, the doses of most medicines must be diminished in the proportion of the age to the age increased by 12"—(Med. Liter. 2nd edit. p. 435); so that a child one year old will take one-thirteenth part of the dose of an adult. Hence, if the dose of extract of belladonna is half a grain for an adult, three times a day, it will be one-twenty-sixth of a grain for a child a year old. But on account of the great sensibility of young children to narcotics, let us suppose the doses to be further diminished by one-half, and then those of extract of belladonna may be stated as follows:—

One year	1-52d of a grain	3 times a day.
Eighteen months .	1-36th	.. ..
Two years	1-28th	.. ..
Two yrs. & a half	1-23d	.. ..
Three years	1-20th	.. ..
Three yrs. & a half	1-18th	.. ..
Four years	1-16th	.. ..
Six years.....	1-12th	.. ..

Whenever it can be avoided, however, it would be better to abstain from giving this medicine to children too young to complain of the symptoms of an over-dose.

extract, it was adulterated to the utmost; that is, it was entirely spurious, and had no belladonna in it.—*American Journal of Med. Sciences*, No. 28.

There is a good deal of evidence in favour of belladonna in minute doses, as a prophylactic against scarlet-fever. —(*Ure*, p. 48.)

The following prescription is curious both for matter and manner:—

"℞. Extr. Cinch. ʒiij. Camphoræ, Cantharidis, aa, ℈j. Tere simul ut fiat pulvis.

Dose:—8 *grains every third or fourth hour in hooping cough.—Burton.*" p. 50.

A family of but moderate resources would be rather astounded at the entrance of a package or bale, containing no less than one hundred and eighty-five doses of physic for their suffering infant. If instead of ʒiij. we read 3iij. we get out of this difficulty, but only to fall into a worse one; for each dose would then contain 8-11ths of a grain of cantharides, which could not possibly be given to a child every third or fourth hour.

From p. 67 to 83, we find a pretty full account of mercury and its preparations, such as hydrargyrum cum cretâ, calomel, corrosive sublimate, &c. The dose of the last is stated to be from 1-30th to 1-16th of a grain, twice, thrice, or even oftener in the day; and in the table we are directed to give from 1-10th to 1-12th of a grain, to a child of from four to seven years old. These doses seem large; yet in hydrocephalus, Dr. Merriman has sometimes given from a thirtieth to a sixteenth of a grain every four or six hours.—*Notes on Underwood*, p. 538.

There is a good account of iodine, pp. 86-92; but we much doubt if it can ever be advisable to carry the dose so high as a grain for a child from four to seven years old.—*Table*, p. 216.

Our author states that Baudelocque administered a solution containing 1-8th of a grain of iodine, and 1-4th of a grain of the hydriodate of potash, in an ounce of water; and then adds, "He gradually increased the dose of this solution, from 1 ounce to 12 ounces in the day—that is, to 6 grains of the hydriodate of potash and 3 grains of the iodine, daily." Not so; the largest dose will make only 3 grains of the hydriodate, and gr. iss. of iodine daily. This solution was, of course, always diluted before it was administered.

We do not often hear of our medical brethren in Albania, so that any thing which proceeds from them has the value which arises from scarcity.

"Mastic-water (μαστιχονερου) is recommended by the physicians of Albania, as a remedy in infantile diarrhœa. It is simply water which has been boiled along with mastic. *Medicin. Almanach für* 1838, p. 155."—(*Ure*, p. 117.)

The *Bulletin Thérapeutique* has supplied our author with a method of improving senna tea: an infusion is to be made with cold water, with which coffee is afterwards to be prepared; by which a purgative potion is produced, without any nauseous taste.

Under the head of tinctures, our author says, "of the officinal tinctures (excepting those of cantharides and opium), the quantity given to infants during the first year or two, is from five to ten drops, two or three times a-day; for children more advanced, about double these portions."—(Pages 200-1.)

Many others must be excepted, in our opinion; for example, the Tr. Digitalis, Tr. Ammoniæ C., and the Tr. Iodinii C.

The following sentence is very quaint:—

"Some German practitioners, supposing the ordinary mercurial salve, prepared with hog's-lard, not bland enough for the tender skin of new-born children, have proposed in its stead one composed of oil of cocoa. (See Formula, No. 418.) Oil of cocoa is much more irritating than fresh hog's-lard."

There is one error which occurs rather often in the prescriptions; *colatur*, is printed for the abbreviated word *colatur*. The latter stands for *colaturæ;* but the former, if taken for the verb, will perplex some beginners exceedingly.

What is the meaning of *Macidis*, of which we are directed to take a drachm in prescription 99, p. 38?

The long notice we have written of this Compendium, will shew that we think it a laudable compilation; and when the author shall have added more of the results of English experience to his foreign investigations, his work will be justly numbered among the most valuable contributions to the literature of the Materia Medica.

GERMAN WORKS.

Dr. Lucas Joh. Boër's Leben and Wirken, von R. F. Hussian. 4to. pp. 51. Wien, 1838. Schloss, London.

This is a biographical sketch of the distinguished accoucheur of Vienna, who died upon the 19th of January, 1835, at the advanced age of 84 years. He was eminent as a teacher, and as the author of several works on obstretrics, of which the "Naturalis Medicinæ Obstetriciæ, libri septem," first published in 1812, is the principal. It is a remarkable fact, that, during the years 1787 and 1788, Boër studied midwifery in London; and attributed much of his practical knowledge to the instructions of Smellie and Dr. Hunter.

Ausfürliche Encyklopädie der gesammten Staatsarzneikunde, von Dr. G. F. Most. 2 vols. 8vo. Leipzig, 1838. Schloss, London.

In a former number of the Medical Gazette we noticed Dr. Most's elaborate Dictionary of Practical Medicine; we now take occasion to recommend to our readers a work executed with equal ability—his Cyclopædia of Medical Jurisprudence. We have looked over with great satisfaction the articles, Age, Arsenic, Atmosphere, Blood, Fœtus.

Die Medicinal, Verfassung Preussens, wie sie war und wie sie ist, von Dr. J. N. Rust. 8vo. pp. 199. Schloss, London.

This work is an exposition of the past and present medicinal constitution of Prussia, from the pen of President Rust. The following is the classification according to the Order in Council of the 28th June, 1825:—

I. 1. Doctors of Medicine and Surgery (*Chirurgo-Medici.*)

2. Doctors of Medicine (*Medici*).

II. Surgeons of the first class.

III. Surgeons of the second class.

Subordinate to these last, are:—

1. Man-Midwives.
2. Oculists.
3. Dentists.

In another category are placed, veterinary-surgeons, midwives, and apothecaries. The work is interspersed with many curious critical remarks on the state of medicine.

MEDICAL GAZETTE.

Saturday, July 7, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

MEDICAL ATTENDANCE ON THE POOR.

We observed in our last article that the present management of Dispensaries labours under two faults. The first, which is a comparatively trivial and pardonable one, is that persons in good circumstances occasionally reap the advantage of gratuitous advice and medicine, originally destined for the indigent alone; the second defect, by far the most weighty and grievous of the two, is, that the really poor (who, after all, form the vast bulk of the patients) are not properly attended to at these institutions.

If it should be found that under a well-devised system of Self-supporting Dispensaries, the purely eleemosynary ones may be allowed to disappear, it will of course be unnecessary to investigate under what conditions the reform of the present institutions might be effected; but if it should unfortunately turn out, that in large towns there are numerous classes who cannot be made to subscribe to the Self-supporting Dispensaries, it will be necessary to probe the subject to the bottom, and to devise the appropriate remedies, which are less difficult to find than many would imagine, judging from the universal neglect of them.

It would probably be found, that so large a proportion of the more diligent and prosperous workmen of London would become members of well-managed Self-supporting Dispensaries, that not more than eight or ten of the ordinary institutions would be required for the whole of London, by which we mean

the space comprehended within a radius of about three miles from the General Post-Office. This might be conveniently divided into the districts of Marylebone, Bloomsbury, Spitalfields, Whitechapel, Westminster, Lambeth, Southwark, and Rotherhithe. Three or four more, indeed, would be required to take in the outskirts, such as Kensington, and Camden Town; but if we confine ourselves to the limits for which we have supposed eight to be sufficient, it will be clear that these eight would supply the place of twenty or thirty now existing within the same boundaries. The saving of funds accruing from giving up so many separate establishments, might be applied to carrying on the surviving ones with more vigour. The necessity, as well as expediency, of paying the medical officers, is so obvious, that it is unnecessary to dwell upon it; but we would merely suggest that the salaries should not be beggarly allowances of 50*l.* or 60*l.* a year, but such as would form a tolerably fair remuneration for the time devoted by men of talent to the purposes of the institution. With such men, inspired with the zeal that rewarded talents bestow, and the train of well-educated pupils whom they would infallibly attract, the visiting of the patients who lived even at the very extremity of the district would be comparatively easy. If, however, it should be thought that the districts we have imagined are too large, they might be increased to ten or twelve, and a great portion of the advantages from the abolition of the other dispensaries would still remain. From the less exorbitant number of these establishments, it would be easier to procure that attendance of gentlemen at the committees and quarterly meetings, without which they cannot flourish. Once let a knot of neighbouring tradesmen — peradventure some clever overseer and his grog-mates—get the upper hand at these assemblies, and then adieu to all decency and common sense. The midnight hours are consumed in splenetic debates, silly resolutions, and motions to rescind the same; and the unhappy doctors pay their unwilling visits the next day, with their minds exasperated by the last night's squabbles, and a draught of a letter of resignation in their pockets — by no means an anodyne draught.

It would be advantageous in many or most cases, that the present buildings occupied as dispensaries should be enlarged and improved, or better ones built in their stead. They are generally too small, or otherwise inconvenient, and the room used as the *shop* is commonly a mere closet, quite inadequate to the object for which it is destined. The attendance, also, of the medical officers is for too short a time in the day; and as they sometimes come punctually to their hour on one day after having been unpunctual for twenty, it will then naturally happen that the majority of the patients are not yet arrived, so that the medical officer, after prescribing for the half-dozen whom he finds, may whisk off, blaming the absent for their lawless unpunctuality, instead of himself for his unseasonable exactness. But enough of this painful subject. Let us turn to a better era, which we hope is approaching—the era of *well-managed* Self-supporting Dispensaries. We say *well-managed* with emphasis, for if they are left to the exclusive direction of the Malthusians and starvationists—if a man and his wife with 14 shillings a week are thought too rich to belong to them—the humane, seeing how cruel it would be to squeeze the amount of doctors' bills out of persons so poor, will necessarily cling to the old eleemosynary

Dispensaries, with all their imperfections. At the Derby Self-supporting Dispensary, where, as Mr. Holland informs us *, a man and wife are excluded, if convicted of the receipt of this princely income, a single woman is thought over-opulent if she has 9s. a week, and is therefore left to employ such physicians, surgeons, and apothecaries, as she may prefer, paying them from the handsome overflow of her fortune in her strong box, or perhaps at her banker's. How this may be at Derby we know not, but it happens that in London this precise sum of 9s. a week is the dole of a class of women whose condition is synonymous with half starvation—we mean the assistants at dress-makers'. This, and tea at the houses of their employers, is all they have to subsist on; and how they can honestly manage to keep body and soul together, maintaining a decent appearance at the same time, is the wonder of the philanthropist. A man must have been steeled with a long course of Poor-law Reports and essays on the merits of water-gruel, before he could venture to assert that a half-fed girl like this was too rich to be allowed to subscribe to a Self-supporting Dispensary; indeed we think that the contemplation of so striking an instance of the defects in our social economy, might make a man of humanity pause before he consented to the utter abolition of eleemosynary dispensaries, or made up his mind to demand even the smallest coin of the realm from one whose long and unwholesome toil was so miserably remunerated.

Some of the necessaries of life are, no doubt, cheaper at Derby than in London, but the difference is far from being so great as to affect the argument.

A difficulty that has more than once struck us, is, how is the income of a working man to be accurately ascertained? In addition to which, we may ask, does income mean average income, or the accidental wages of the week preceding the one in which he becomes a subscriber? Now, the wages of the majority of journeymen in London are fluctuating. A tailor earns five shillings a week in September, twelve or fifteen in January, and from thirty to five-and-forty in May. Which of these rates is his income? And if he enters in September, when, weasel-like, he is able to pass through the narrow aperture, is he to be turned out again when his May wages would, according to our rigorous economists, enable him to keep some eminent practitioner in pay?

The ascertaining the income is entrusted to the secretary and a sub-committee; but in a great town we fear it would not be very difficult to deceive their vigilance, and the constant variation of wages would make it difficult even for an honest man to answer their questions; as we need not inform our readers that it is not the custom among the working classes to keep ledgers or cash-books.

The objection, therefore, to common dispensaries, which seems to weigh so heavily on Mr. Holland's mind, namely, that improper patients get admitted, would be valid here also, if a money test were adopted.

Hence a coarser but more applicable criterion will, perhaps, work better in practice; and it will be found more expedient to admit subscribers to these Self-supporting Dispensaries if they belong to a certain class in society, than in accordance with their supposed income. All labourers, artizans, and journeymen (excluding foremen), should be admitted to the benefits of these institutions; and the subscription might be one, two, or three pence per week, at different dispensaries, to suit the purses of the contributors. Those who gave the larger sums would manage

* An Essay on Dispensaries, &c. Manchester, 1838.

their own institutions without the aid of honorary subscribers, and this, as well as other subsidiary advantages, would induce the superior class of workmen to prefer the higher rate of payment.

As soon as these arrangements are made, medical practice will accommodate itself to the then existing state of things; law will not so frequently be seen as the assistant of physic, nor will the Court of Requests and a pint bottle of black draught be so nearly linked together, by the natural concatenation of ideas, in the mind of a Bethnal-Green practitioner. The Bill which has just passed the House of Lords for abolishing imprisonment for debt on mesne process, will tend to further this salutary change; for there is no doubt that the facility with which the body of the debtor was seized under the old law, gave a preternatural stimulus to credit. Imprisonment for debt, after judgment, will still be allowed; but this tardy and expensive remedy will seldom be resorted to; and the practitioner, whose patients are among the classes who live from hand to mouth, will soon discover that it is better to accept pence from the many than to go to law for shillings from the few.

COLLEGE OF SURGEONS.

To the Editor of the Medical Gazette.

Sir,

Will you, with your accustomed liberality, oblige me by inserting the accompanying communication in an early number of your journal.—I remain, sir,

Your very obedient servant,

John Wiblin, M.R.C.S.L.

July 5, 1838.

On Tuesday, the 3d inst., having occasion to visit the Library of the Royal College of Surgeons, in Lincoln's-Inn Fields, I was very much surprised on my arrival there to witness the display of illiberal feeling evinced towards five foreign surgeons, of no ordinary eminence in their profession. These gentlemen (French, and all members of the "Légion d'Honneur,") were endeavouring to obtain admission to inspect the museum of the College; and after soliciting in the most humble manner, they were simply told by the beadle, "that they could not visit the museum, as it was not the usual day." These gentlemen expressed to me their great astonishment, and left the College of Surgeons exceedingly disappointed. Previous to this, I had sent two Professors, from one of the first schools in Italy, and they were refused admittance; these gentlemen left town the same day, and consequently, as they expressed themselves, have been deprived of seeing "*tout ce qu'il y avait de plus magnifique.*" Before I left the College I was fortunate enough to meet with Sir Anthony Carlisle, to whom I mentioned the above circumstance; this gentleman immediately expressed his extreme regret at such an occurrence, and immediately told me, that whoever had prevented the gentleman in question from visiting the Anatomical Museum, had by far exceeded his duty, and taking from his poeket a printed paper, he there pointed out to me, as one of the rules of the College, the following item: "That the museum shall be opened on Mondays, Wednesdays, and Fridays; and to scientific foreigners, *every day.*" What can be more plain or just? Yet when I saw Mr. Belfour, which I was requested to do by Sir A. Carlisle, I was only informed, "that certain days were allowed for the inspection of the Museum, and what could he do?"

Perhaps, Mr. Editor, you will be enabled, in your ensuing journal, to name with whom the evil rests; for certain, the sooner such a disgraceful act of illiberality is put an end to, the more creditable will it be to those who can so easily do away with the grievance complained of.

ON THE STRUCTURE OF THE RETINA.

Abridged from Müller's Jahresbericht. 1837.

Langenbeck distinguishes in the retina, 1st, an external granular or cortical layer; 2d, Ehrenberg's nervous fibrous layer; and 3d, a vascular layer formed of blood-vessels, connected by cellular tissue into a delicate membrane. He also, as Volkmann did, found the fibres in the retina, like those of the optic nerve, varicose; but the latter found that this appearance was not constant, and that the swellings were some-

times altogether absent, differences which depend in part on the period and mode of making the examination. In the situation of the macula lutea of the retina, Langenbeck saw that the granular layer alone ceased with sharply defined edges; at the part where the retina is limited at the zonula, the granular layer ceases, while the fibrous layer is continued very thinly forwards, forming what the author calls the ciliary part of the retina, which he says lies on the zonula, covers the posterior surface of the ciliary processes, turns round their edge to their anterior surface, and terminates at the boundary between the corpus ciliare and the uvea. This prolongation contains also delicate varicose tubules. He saw the vessels of the retina, in the fœtus of the pig, communicating with those of the zonule. Treviranus does not acknowledge a ciliary portion of the retina. At some distance from its edge he could not discover either the vascular layer or the membrana Jacobi on its medullary substance, but in the neighbourhood of this edge its two surfaces were covered by a delicate membrane, an external one homogeneous, and an internal vascular. Both of these extended beyond the edge of the medullary layer, then being firmly attached to each other, were longitudinally folded, and continued to the zonula, without any appearance of a continuation of the medullary substance between them being discoverable.

Gottsche describes four layers in the retina. The optic nerve, he says, separates into fasciculi, which lie on a tough membrane, and are surrounded by vessels. The fasciculi divide into smaller bundles, and these again into fibres; but if the vascular network of the retina be regarded as a tunica vasculosa, and the pulpy little scales on the outer side of the retina be taken as a membrane, then we have four layers in it. Most internally, and next to the hyaloid membrane, Gottsche finds, 1st, the vascular expansion; 2d, then follows the division of the optic nerve into fibres; 3d, these lie under the firm substratum which was formerly named the retina of Gottsche; while, 4th, most externally lies the pulpy layer of scale-like granules. For the mode in which the author demonstrates these several layers, his Treatise (in Pfaff's Mittheilungen. Jahrg. 2. p. 40) is referred to. Michaelis renders the nervous expansion visible by creosote; Gottsche by a preparation invented by himself. The separated nervous membrane is laid out on a black ground, and allowed to lie some time in water; then the author lets fall upon it a few drops of a solution of corrosive sublimate (one part of sublimate to three parts of sulphuric æther), and brushes it gently with a paint-brush, so as to crack the tough external membrane, and to wash away the little scales. Gottsche has sent us preparations thus made, which are extremely instructive, for shewing the general structure of the retina. The pulpy external lamella is to be distinguished from the membrana Jacobi, lying between the retina and choroid, which is a matter like mucus, and may be drawn out in threads in water. The pulpy lamella consists of roundish molecules, and may be removed in portions of various sizes from the tough subjacent membrane, by water, alcohol, or acids. The granular layer, according to Gottsche, is lost anteriorly earlier than the tough membrane, which is applied immediately on the zonula, and supports the nervous fibrils, being itself not fibrous but uniformly smooth. Anteriorly, it is firmly connected with the zonula, but it is not intimately united to the fibrils of the retina. The nervous filaments could only be traced to the vessel at the edge of the retina. The vascular ramifications of the retina remain in the eyes of man and mammalia adhering to the nervous membrane; but in fish they lie on the vitreous humour, when the rest of the retina is removed. Hence the author considers the existence of the vascular layer as a separate membrane to be not yet made out. Langenbeck observed the same in the fœtus of mammalia, in which the vessels of the retina might easily be taken for those of the vitreous humour.

The more exact relation of the layers to one another had hitherto remained unknown, so that some years ago we had said that the structure of the retina was still a question. An extended writing for the explanation of this subject has now appeared, from the pen of Treviranus, who enriched science with this discovery shortly before his death. The essential part is substantially expressed in his own words. The optic nerve divides into nervous cylinders, which radiate on the outer side of the retina. Each individual cylinder, or each bundle consisting of several cylinders, at a certain part of its course bends from the horizontal direction, and passes towards the opposite internal surface of the retina. Directly after changing its direction, it passes through the openings of a vascular net-work, formed of branches of the central veins of the optic nerve. Before it gets to the internal surface of the retina it passes through a second vascular net-work, which is formed of the ultimate ramifications of the central artery. After its passage through the latter, it is received in a sheath-like process of the vascular layer; and covered by this, it terminates behind the vitreous humour, in the form of a

papilla. The diameter of the cylinders was in the hedge-hog ·001 millim.; of the papillæ in rabbits, ·0033; in birds, ·002—·004: in the frog, the diameter of the cylinders was ·0044; and of the papillæ, ·0066.

Gottsche's observations contain also many details, which are interesting to Treviranus' discovery. The so-named granular layer of the earlier writers, of which so many different views had been taken, is not found in the fresh eye. If a perfectly fresh retina be examined, says Gottsche, an appearance something like a straw-thatch is seen, with minute nervous cylinders projecting out from it; if it be allowed to lie for some time on the glass, and be now and then moistened, granules become evident, as may be seen in a fish's eye in from three to four hours after its death. In this remark we completely agree. We have seen on the inner surface of the nervous membrane of a mammal, frog, or fish, recently dead, the cylinders projecting free and clear, and the comparison with the straw-thatch is very good. Sometimes the cylinders are also visible from the outside, as Gottsche also thinks, but Michaelis believes that they are seen through the membrane; and so, indeed, it has seemed to us. These little cylinders easily break off, and then swim separately in the fluid: when detached, they are much longer than broad; and in the frog we have often seen them of unequal lengths, and some of them somewhat curved.

Gottsche distinguishes the cylindrical or staff-shaped bodies from the papillæ. According to Treviranus, the papillæ are the extremities of the nervous cylinders; but according to Gottsche's investigations, the staff-shaped bodies pass through the papillæ. He distinguishes the following varieties: — 1st. The nervous cylinder merely acquires a sheath, just as the hair has a sheath, which is not consistent with the idea of a papilla. This form occurs in the toad, frog, and among fish in the sturgeon. 2d. The nervous cylinder passes through a conical papilla, the diameter of whose base is three times as great as that of the cylinder. 3d. Two nervous cylinders pass through one obtusely pointed papilla, which, when pressed, divides into two papillæ; or the nervous cylinders lie in pairs, each having a bulb-like papilla at the extremity attached to the vascular net-work. Papillæ also occur without the staff-headed bodies, and there is a spot on the retina of fish and frogs, in which the papillæ present bluntly rounded tops.

Volkmann and E. H. Weber have confirmed the views of Treviranus in mammalia: they saw the fibres constantly bending inwards, but no papillæ, which are indeed always absent in that class, in which nothing but the cylinders are found. I extract the following from a manuscript treatise on the Structure of the Retina, communicated to me by Dr. Michaelis, and which will appear in the "Nova Acta Naturæ Curiosorum." The retina only goes to the zonule, for there the granular layer and the expansion of the optic nerve cease. He describes four layers in the retina—an external serous layer, a granular layer, a nervous and vascular layer, and lastly, an internal serous layer. The external layer is the membrana Jacobi. The second, or granular layer, is the thickest of the retina, and presents both on its external and internal surface a globular structure: it is clear and transparent, but becomes turbid after death, and by the action of chemical means, while the nervous layer remains transparent in spirit and in water. When cut through, the divided surface presents upright cylinders, which stand closely pressed together, each bearing a globule at the extremity, turned towards the nervous layer. The nervous bundles of the nervous layer are most clearly displayed after being treated with creosote; their diameter the author states to be 1-2,500th part of a line. The vessels lie on the interior of the nervous layer. The serous or innermost layer is seen after treating the retina in water with nitric acid. On the surface turned towards the retina, there hang here and there, after the removal of the nervous matter, a number of minute globules, of 1-1,500th of a line in diameter, at tolerably regular distances, of from 1-100th to 1-50th of a line; most of them have a fine filament of uncertain lengths, of the size of the primitive fibres. Michaelis regards them as the extremities of the nerves. The serous layer remains after the separation of the vitreous humour, still adhering to the retina.

Michaelis has made numerous observations on the macula lutea. At this situation the granular layer is very thin, but at 1-10th of a line distant from the centre of the yellow spot it becomes suddenly thicker, so that at half a line distance it attains its greatest thickness. This prominent ring or swelling round the single layer of globules, forming the foramen centrale, is the macula lutea. The fibres of the retina have a peculiar arrangement round it; for while in other parts the nerves radiate straight from the optic nerve, they go towards and around the macula lutea in an arched form; a part of the arches on each side meeting in the so-named foramen centrale, while the next outwards converge in a regular series from both sides, towards a line which passed from the macula lutea. Between it and

the optic nerve the nervous filaments are few, and more or less straight. A union of fibres is found nowhere else. The thin transparent spot called the foramen centrale is not quite round, but appears when magnified to be star-shaped, and in young subjects is elongated.

[It may be considered as proved by these investigations, that the retina, which has very generally been regarded as merely a single expansion, or melting out of the optic nerve, is formed of several distinct layers. That immediately subjacent to the membrane Jacobi, there is a pulpy soft matter, which hardens in alcohol, &c., and may then be removed in small scales; that next internally to this there is a dense firm membrane, which has a granular aspect on both surfaces, and supports the nervous fasciculi and filaments of the divided optic nerve, which lie beneath it, radiating from the nerve as a centre, and covering the whole vitreous humour as if by an equable expansion; and that immediately beneath the radiating filaments of the nerve, the vessels are distributed in a venous and an arterial network, forming a single vascular layer. Through the meshes of these networks, each ultimate fibril, having turned inwards, passes, receiving from them a simple sheath, or having a more or less perfect papilla formed at its base. Thus curved, they project towards the anterior part of the eye, being arranged so densely, that when viewed in front they present a perfectly smooth and even surface, of the same kind as, but much more fine and delicate than, that of velvet. Thus the retina reeeives impressions as other nerves are supposed to do, on the extremities of ultimate nervous fibrils, and presents that most admirable opportunity of observing their mode of termination, which it is probable that future investigations will prove to be the same in all parts of the body.—*Translator.*]

FORMATION OF ARTIFICIAL SERUM.

M. Denis is led, by some experiments which he has made lately, to regard the fibrine as identical with albumen, and as held in solution in the blood, during life, by the salts of the serum. He says, if some pure and well-washed fibrine be taken, and macerated for twenty-four or forty-eight hours, in water containing a neutral salt, as nitrate of potash, it dissolves. The new product resembles serum, or white of egg; it precipitates bichloride of mercury; alcohol reduces it into a curdy mass, and at 74° C. it coagulates like pure white of egg; but if the solution be much diluted with water, the fibrine will gradually reappear, with its original properties. If a little caustic soda be added to the saline solution of fibrine, it becomes permanent; and when diluted with a great deal of water, will deposit only a slight cloud. But if the salts employed are not in too great quantity, the alkalized solution will coagulate by heat; will precipitate with alcohol and bi-chloride of mercury, and the new fluid, which may be called *artificial serum*, will act like white of egg, or natural serum. — *Archiv. de Médicine, Fevrier* 1838.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, July 5.

Jacob Ashley, Wick Hebson, Gloucestershire.—Philip Chas. Hayman, Axminster, Devon.—Wm. Rollinson Warwicke, Newark, Notts.—William Forbes Laurie, Reading, Berks.—John Eric Erickson, Brighton.—Harrison Packard, Middleton, Suffolk—Joseph Rickerby Donald, Islington.—John Bourne, Stafford.

WEEKLY ACCOUNT OF BURIALS.

From Bills of Mortality, *July* 3, 1838.

Abscess	5	Heart, diseased	2
Age and Debility	26	Hooping Cough	4
Apoplexy	4	Inflammation	12
Asthma	6	Bowels & Stomach	2
Cancer	2	Brain	3
Childbirth	2	Lungs and Pleura	3
Consumption	52	Influenza	1
Convulsions	18	Insanity	1
Dentition or Teething	8	Liver, diseased	1
Dropsy	7	Measles	4
Dropsy in the Brain	7	Mortification	3
Dropsy in the Chest	1	Paralysis	1
Erysipelas	1	Small-pox	34
Fever	20	Thrush	2
Fever, Scarlet	5	Tumor	2
Fever, Typhus	6	Unknown Causes	34
Gout	1		
Hæmorrhage	1	Casualties	11

Increase of Burials, as compared with the preceding week . . . 67

METEOROLOGICAL JOURNAL.

June.	Thermometer.		Barometer.	
Thursday . 28	from 54	to 66	29·94	to 29·93
Friday. . . 29	51	64	29·92	29·91
Saturday . 30	46	65	29 90	29·83
July.				
Sunday . . 1	52	67	29·83	29·88
Monday . . 2	57	72	29·94	29·96
Tuesday . 3	56	65	29 98	30 00
Wednesday 4	53	68	30 04	30.05

Prevailing wind, S.W.

Except the 29th and 30th ult., and afternoon of the 4th inst., generally cloudy, with frequent and heavy showers of rain; distant thunder on the afternoon of the 29th.

Rain fallen, 1 inch and ·0125 of an inch.

Charles Henry Adams.

Wilson & Son, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 14, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton-Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXIII.—*(Concluded)*

Encephaloid Disease in the Lung.—Scirrhus? in the Lung.—Melanosis in the Lung.—Spurious Melanosis.—Diseases of the Bronchial Glands.

I have occupied so much time with the subject of phthisis (without, however, having half exhausted it), that we have scarcely any left for that of encephaloid, scirrhous, melanose, and similar productions, affecting the lungs. Their occurrence is too rare to be of much practical importance; and they are not known to be in any degree influenced by medicine. They may occur in a circumscribed form, or occupying a considerable extent of the texture; and they would then produce physical signs like those of consolidation from hepatization or tuberculation of similar extent, and could be distinguished from these only by the history and general symptoms, and by the absence of the constitutional indications of tubercles. They commonly cause death, either by their encroachment on the function of the lungs, or from being simultaneously deposited in other organs, such as the mesentery, the liver, the ovaries, &c. But when they occupy the lung chiefly, both encephaloid and melanose deposits tend in time to soften and form ulcerous cavities as in the case of tubercle. I have seen such cavities more than once in both these forms of disease.

I have not met with a sufficient number of cases of these morbid deposits to enable me to give you their anatomical history, as I have that of tubercle. The general appearance of *encephaloid disease*, or *medullary sarcoma*, is that of a brain-white solid, of varying consistence, with a pinker hue than that of tubercle, occurring either in separate tumors, which are sometimes encysted, or infiltrated through the tissues of the lung, and modified by their colours. When occurring in separate tumors it is sometimes soft and cellular; in other cases tougher, and more like the pancreas; in others again, as in this specimen, it becomes of fibro-cartilaginous hardness. A predominance of a loose cellular and vascular structure in it, with patches of extravasated blood, give it occasionally the appearance that has obtained for it the name of fungus hæmatodes. We may conjecture that the albuminous matrix of these products is deposited in an organizable form, and vascular ramifications are certainly formed through it; but it is deficient in the cohesion and contractile tendency of ordinary false membranes; it does not restrain the further effusion from the vessels, whence the tendency to growth in these productions. When encephaloid matter occurs in an infiltrated form in the tissue of the lung, it sometimes presents an appearance intermediate between that of tuberculous and that of hepatized consolidations; and unless there be portions of the diseased production occurring separately, it might be taken for one or other of these lesions.

The only form of disease which I have seen affecting the lung, which approaches in any degree to *scirrhus*, is that which I have already described as a result of a chronic pleuro-pneumonia; there being in these cases firm adhesions to the pleura, a shrunk state of the lung, and dilatation

of the bronchial tubes. The induration and glistening texture which the lung so changed sometimes exhibits, especially around the larger air-tubes, might lead one to suspect it to be of a scirrhous nature; but I have seen neither the tendency to cancerous ulceration, nor the simultaneous occurrence of scirrhus in other parts, which might be expected if this induration of the lung were really of a malignant kind.

I have met with several cases of *melanosis*, or *black tubercle*, affecting the lung, both exclusively, and with the same production in other parts of the system. In this drawing you see it combined with encephaloid disease. The black matter may occur infiltrated in a natural structure, or in distinct tumors or deposits of an irregular cellular organization. I am much inclined to adopt the opinion of Andral, that the black matter is nothing but a modification of the colouring matter of the blood, in which carbon is in excess, or even in a free state. In the lung from which this drawing was taken, the deposits exhibited in different parts various shades of colour, from the dark cruor red of hæmorrhagic engorgement to the deep jet black of perfect melanosis. The intermediate colours were of a bistre or sepia brown. The organized texture of melanose tubercles and tumors presents considerable variety, sometimes approaching to the most perfect products of acute inflammation, being soft and cellular or membranous; and sometimes having almost the totally unorganized structure of scrofulous tubercle. Probably it is only this latter form that undergoes the changes of softening and ulceration ascribed to melanosis by Laennec; and under these circumstances, such changes are to be referred to the same causes as those which operate in the kindred changes of tubercle. The presence and modification of the colouring matter of the blood seems, therefore, to be the essential pathological condition of this disease, as an altered or deficient vitality of the fibrinous matter is of tuberculous affections.

It is necessary to guard you against confounding with melanosis the accumulations of the black pulmonary matter, which take place to a great extent in the lungs of old people, especially among the inhabitants of large towns. These are probably, as Dr. Pearson supposed, derived from the soot inhaled with the air; which, I presume, finds access to the texture of the lungs chiefly through abrasions, softening, or other such lesions of the bronchial membrane, which, in a slight degree, often result from a common cold or cough. Whether from this source, or, as others have supposed, from an altered state of the colouring matter of the blood itself, I think it is plain enough, that when once deposited in any corners out of the immediate sweep of the circulation, such as in the angles of lobules, near old lesions, around large vessels, and in the bronchial glands, there it must lie, accumulating until death, or until it is carried off by the destruction of the tissue by some pulmonary disease. For it consists entirely of carbon; and this being totally insoluble in any animal fluid, is insusceptible of absorption, which cannot act on insoluble solid matter. For the same reason the carbonaceous matter of tattooed skins, and the insoluble oxide or chloride of silver in persons coloured blue from the too long internal use of nitrate of silver, are permanent, and can only be removed with the skin itself. It does not appear that this carbonaceous deposit in general interferes materially with the function of the lungs; but there are some curious cases on record, in which it has taken place so rapidly and extensively as to cause chronic inflammation and consolidation of a perfectly black colour, which tends to ulceration and the formation of cavities, as in other cases of chronic consolidations. Such cases are described by Drs. Gregory, W. Thomson, and others, as occurring particularly in coal-mines, and in persons labouring under bronchial disease whilst continually employed by the light of smoky lamps.

The general symptoms of encephaloid or melanotic consolidation of the lungs, are those of obstructed breathing or circulation, dyspnœa, lividity, and dropsy; more commonly than those of consumption and emaciation, which belong rather to tuberculous disease. This is explained by their more rapid development, and their not so readily leading to softening and ulcerative destruction of the organ. When this process does occur, the expectorated matter may afford means of distinction. I have seen, in the case of encephaloid disease, streaky red and white purilaginous liquid sputa, and, in melanosis, a considerable quantity of black matter, mixed with a muco-purulent compound. But such cases are not common, and the expectoration is more usually that of the bronchitis or pneumonia that may accompany the disease.

The *bronchial glands* are not unfrequently found after death in a diseased state, even when no symptoms referrible to them had been manifested during life. I do not mean the deposit of black matter like that of the lungs, for that is so constant that it can scarcely be considered to be morbid. But they are sometimes found swollen and red, or containing caseous matter, or

osseous concretions. In children they are occasionally so enlarged by the deposition of tuberculous matter, as to press on the air and blood-vessels, and, according to Dr. Carswell, to produce dyspnœa and symptoms of obstructed circulation. They sometimes soften, and become evacuated by ulceration into the bronchi. Encephaloid disease, as well as extensive enlargement, not of a malignant character, may also affect these glands. I suspect that encephaloid disease of the lungs generally originates in this way, and spreads afterwards along the vessels into the pulmonary tissues. I have also met with cases of dulness on percussion on the top of the sternum, with signs of obstructed circulation and respiration, with simultaneous enlargement of the axillary and cervical glands, betokening a glandular tumor about the root of the lungs; and all these symptoms gradually subsided under the use of iodine and alkalies; so I conclude that the tumor was simple glandular enlargement. Considerable tumors of the bronchial glands might perhaps sometimes be discovered by dulness on percussion on the upper portion of the space between the clavicles and on the spinous processes of the upper dorsal vertebræ. The tumors, generally, however, grow forwards, and I have seen them pushing out the sternum or the ribs on one side, and causing dulness at those parts, and symptoms of displacement of the lung further down. They also may produce signs by their pressure on the great vessels, arterial and venous, as you will understand when we treat of the organs of the circulation—a subject which I propose to begin in the next lecture.

[The above was intended to form the conclusion of the preceding lecture, but was not received in time to occupy its proper place. In his next, Dr. Williams proceeds to the Heart; which, being an entirely new subject, we have thought it better not to enter upon it in the present number.—Ed. Gaz.]

LECTURES ON BLOOD-LETTING,

Delivered from time to time,
At the General Dispensary, Aldersgate Street,

By Henry Clutterbuck, M.D.
Formerly one of the Physicians to that Institution.

Lecture IX.

On the Use of Blood-letting in Diseases of the Organs of Respiration.

There is no class of diseases in which blood-letting is so frequently called for, or in which, speaking generally, it is so unequivocally useful, as in those of the respiratory organs. These diseases, with very few exceptions, either consist in inflammation, or are consequences of it, as you will soon see. The term *spasm* has indeed been applied to some of them, but in a way the most vague and unsatisfactory. Thus you frequently hear of spasmodic asthma and spasmodic croup—terms by which little more seems intended than that these are states opposed to inflammation; and so far from requiring the use of blood-letting, as to call for remedies of a totally different nature, such as stimulants and narcotics, under the name of anti-spasmodics. The only idea that can be properly attached to the term spasm is that of a preternatural contraction of muscular fibres; but nothing of this kind will be found in regard to many of the affections here alluded to. To what extent, and in what cases, the term spasm is really applicable, will appear as we proceed.

The organs of respiration altogether may be arranged under three heads:—1st, the air-tube (trachea), with its ramifications (the bronchia); 2d, the lungs themselves, with their vessels, nerves, and membranous covering (pleura pulmonalis), together with the cavity in which they are placed (the thorax or chest, as we familiarly call it), and which is lined by an extension of the same membrane that covers the lungs themselves—the pleura; and 3dly, the muscles of respiration, by the action or contraction of which the cavity of the chest is enlarged, and a vacuum formed, which the pressure of the external atmosphere immediately supplies. This constitutes the act of inspiration; while the admitted air, or rather a portion of it, is again expelled, principally by the elasticity of the ribs, aided occasionally, however, by the forcible contraction of the abdominal muscles. The morbid affections of these different parts give rise to a variety of symptoms each peculiar to itself, though subject to be variously combined.

Dyspnœa, or difficulty of breathing, is the principal character of these affections, and that which leads to danger. Pain and coughing also frequently attend them, but are subordinate to the former in point of importance, upon the ground that disordered function is always of greater moment than disordered sensation. Thus, with regard to the brain, vertigo or giddiness is a much more alarming symptom than mere headache, however severe.

The first division of the subject, as stated above, includes the air-tube and its ramifications, terminating in what are called the air-cells of the lungs. These passages are lined throughout by a conti-

nuous membrane (mucous membrane), a part which is peculiarly liable to attacks of inflammation, partly from its direct exposure to atmospheric changes, and partly from the casual inhalation of noxious or irritating matters. This inflammation is known by the generic name of catarrh, which applies to it in all its varieties, though, according as it is situated in different parts of the tube, it takes specific denominations: as that of coryza, when seated in the nostrils; croup, in the larynx; tracheitis or bronchitis, in the great barrel of the trachea and its ramifications; and asthma or dyspnœa, when confined to the minuter branches of the tube and the air-cells of the lungs. The affection altogether, however, is the same in nature, and marked by the same primary or essential symptoms, and which are few and simple. The membrane first swells, and becomes hot and dry, more or less so according to the degree of the inflammation present. This is soon followed by a return of the secretion, which is at first thin, transparent, and acrid; then, as the inflammation proceeds and begins to decline, the mucus becomes glutinous or viscid; and, after a longer time, opaque and friable, when the heat and swelling abate, and the membrane returns gradually to its natural state.

The tendency of catarrhal inflammation, in all its varieties, to follow this course, and to terminate spontaneously in health after the lapse of a few days, is commonly so strong as to call for no artificial aid. If, however, it occurs with unusual severity, an antiphlogistic treatment may become necessary, and especially blood-letting, in circumstances otherwise favourable to its use. When the mucous membrane has suffered repeated attacks of inflammation, the membrane often becomes permanently thickened, and acquires at the same time a habit of secreting mucus largely, and of a vitiated quality, and thus permanent dyspnœa is established.

There are some varieties of catarrhal inflammation which require more particular notice. When, for instance, the inflammation is confined to the nasal passages, it is productive, comparatively speaking, of but little inconvenience; but when the inflammation, as is sometimes the case, extends into the frontal sinuses, it gives rise to pain over the forehead, general headache, and sometimes febrile action. These take place with such severity at times, as to justify blood-letting to a considerable extent. Sometimes the disease spreads into the Eustachian tubes, which become obstructed by the swelling of their lining membrane, and deafness is the consequence. Deafness, from this cause, however, is seldom permanent.

When the inflammation affects the lining membrane of the larynx (croup,) this being a very sensible and irritable part, it gives rise to much coughing, while the swelling of the membrane at this narrow part of the air-tube, impedes the respiration often in a dangerous degree, especially in very young children, in whom the passage is naturally narrow. The muscles attached to this part of the tube, are apt to be irritated from time to time, into spasmodic contraction, which, while it lasts, is productive of great aggravation of symptoms. In this sense only is croup to be considered a spasmodic disease. The inflammation here is often of so active a character, as to throw out coagulable lymph; forming, as it were, a new membrane on the inflamed surface, and which is not detached, perhaps, for several days; during which, the life of the patient is in great danger. There is another source of danger in infants, namely, the tendency of the brain to become disordered; and thus fatal convulsions are apt to ensue.

The danger of croup altogether, is such as to call for active treatment, especially blood-letting, to the utmost extent, that the age and other circumstances of the patient warrant. General bleeding, either from the arm, or (which is still better perhaps) from the jugular vein, where practicable, is doubtless preferable to local. The earlier, too, this is done, the better; indeed its utility appears to turn chiefly upon this point; for after the disease has subsisted for a day or two, blood-letting is likely to be injurious rather than beneficial.

After blood-letting has been employed to the required extent, it will be proper to have recourse to mercury, giving from one to two grains of calomel *(hydrargyri chloridum)* every three or four hours; continuing this, if necessary, for eight-and-forty hours, or thereabouts; by which time the disease will generally either have begun to decline, or have made such progress as to render a fatal termination almost inevitable. In thus recommending calomel, as an auxiliary to blood-letting in the treatment of croup, I am far from considering it, as some have done, in the light of a specific, but rather as supported by the general opinion in its favour; though this, as you know, does not always rest on indubitable proof. I I have employed calomel on many occasions to the fullest extent recommended; giving, for example, two grains every hour, for six-and-thirty hours, without any benefit, and indeed without sensible effect of any kind. In many instances, the patient has recovered under this treatment. But I could say the same of various other remedies, of very different and even op-

posite natures, that have from time to time been in vogue. And it is no less true that the same varying result has been observed where only the most trifling and inert means have been employed. It is natural, therefore, to feel a little sceptical on the subject. The other parts of the treatment usually resorted to in cases of croup, such as vomiting, purging, and blistering, I need not detain you with. I may remark, however, that where the cough in croup is harassing, and almost incessant, as is sometimes the case, an opiate, though not, strictly speaking, a curative, may be of great advantage in allaying this powerful cause of aggravation.

In persons who have suffered frequently from catarrhal inflammation, especially in advanced life, and when the habit is corpulent, the muscles of the larynx are apt to acquire a morbid degree of irritability, so as to be thrown into a state of spasm, when any acrid matter happens to be applied to the glottis; thus threatening absolute suffocation. The swallowing a little cold water, or indeed any other liquid, will generally suffice to give relief in such cases. I need not add, that this is no case for bleeding.

When the inflammation is confined to that portion of the membrane that lines the great barrel of the trachea, or even its primary divisions, the attendant swelling of the membrane does not materially interfere with respiration; the principal inconvenience then experienced being the cough. This, however, is sometimes so violent, as to disturb the vascular action of the brain; even to the degree, at times, of inducing apoplexy, in habits predisposed to this disease. In such cases blood-letting may be required. But when the disease spreads extensively throughout the smaller ramifications of the bronchial tubes, the accompanying swelling of the lining membrane of the tube comes at length to impede materially the function of respiration. A new train of symptoms then arises, producing not only great distress to the patient, but depriving the system of that influence from the atmosphere that is essential to life. The blood does not undergo in the due degree those changes which respiration is intended to effect. This deficiency manifests itself by a livid hue of the skin, feebleness, and sometimes irregularity of pulse, general torpor of feeling and action throughout the system, and cold extremities. Not unfrequently, also, stupor or delirium occurs, and the tongue is covered with a brown fur. The proper treatment of such symptoms is by no means very clear. Though the mischief originates in inflammation, and that often of an active kind, it is far from certain, that in such a state of the general system, blood-letting is always a proper remedy, or even safe, if carried to any extent. You are to recollect what I before stated, that blood-letting is not a certain or direct remedy for inflammation in any case, but that the propriety of employing it is governed by a variety of circumstances, which I then pointed out. The action of the heart and blood-vessels is so much enfeebled in these cases, that the blood is with difficulty transmitted through the lungs. To draw blood, therefore, for the purpose of checking the inflammation, and thereby reducing the swelling, would rather seem to enhance the danger, by further weakening the heart's action. But, on the other hand, to employ stimulants, for the purpose of increasing the circulation, would be likely to aggravate the existing inflammation. In such cases, we ought to proceed with the greatest caution. If the disease be recent, and the habit tolerably strong, it will generally be right to draw blood; but to a very limited extent, such as three or four ounces. If the effect be good, we are encouraged to a repetition of the evacuation after a few hours. By proceeding in this cautious manner, we give the patient the best chance the circumstances will allow of; without adding to the existing danger. In these cases, there is always a propensity to do too much—as by stimulating, blistering, mercurializing, &c., which can hardly, by possibility, be of any material service, though they are very capable of doing harm. The best hope of escape for the patient is, that, under the depressed state of the circulation, the local inflammation may spontaneously subside, and the air-tube become again pervious. The inhalation of the vapour of vinegar is a remedy from which some good might reasonably be expected in such cases, as tending to promote secretion; and I think experience sanctions its employment.

Inflammation of the pleura, or investing membrane of the lungs and chest, is the most painful, though the least dangerous, form of pulmonic disease, as it does not necessarily interfere much with the respiratory function. It is one also that admits, in most cases, of speedy and decided relief from blood-letting, provided this be employed in an early stage of the disease, and to the requisite extent—a point to be determined by the strength of the individual, and the other circumstances before explained.

When the substance of the lungs is involved in the disease, the case becomes more important, as impeding the breathing, more or less, according to the extent of the inflammation. The severity and danger are judged of by the degree in

which respiration suffers, rather than by the pain, which, indeed, is often very trifling. The prompt application of the chief remedy, blood-letting—the only one upon which much reliance can be placed—is here of the greatest moment; and when so applied, and to the proper extent, it rarely fails to be successful. The same observations in regard to other, and auxiliary means, apply here as in the case of catarrh.

Very little attention is due to the tribe of *expectorants*, as they are called, that are in such general use on these occasions. If they have the effect of withdrawing your attention from the principal remedy, they become, indeed, worse than useless.

The organs of respiration are very frequently the seat of chronic inflammation, which in many instances is the result of neglect of the more acute form of the disease. These chronic inflammations differ from the acute, in little more than in degree; and they require, for the most part, a treatment similar in principle; that is, antiphlogistic; modified, of course, by the circumstances of the case. These affections vary much with season and weather; and, when severe, rarely fail to be benefited, by a moderate loss of blood; the good effect of which is not limited to the giving present relief; it further tends to obviate and prevent that alteration of structure in the part, which, when confirmed, commonly continues through life, bringing in its train numerous inconveniences and evils of a more serious kind, which you will readily understand.

I would advert here, as particularly connected with our present subject, to that most frequent and most fatal of maladies—pulmonary consumption. The worst forms of this disease commence by slow, and at first scarcely perceptible, inflammation in the chest. The pulmonary substance, like other parts, becomes indurated in texture; not uniformly, but to a limited extent at first, but which spreads as the disease advances. These indurations present a rounded or knotty appearance, and differ considerably in size, as well as in their internal structure and consistence, according to the degree and stage of the inflammation. These indurations are commonly termed tubereles, a term that is sufficiently descriptive both of their figure and general texture. A great deal of fruitless labour has been bestowed in endeavouring to prove the specific nature of these tubercles; and the wildest speculations have been broached on the subject. It is sufficient to mention the notion of their parasitic nature and origin, an opinion that some have contended for of late. I will not waste your time by combating such phantoms, which, indeed, if adopted, lead to nothing useful.

This tubercular state of the lungs is never found without being preceded and accompanied by signs denoting the presence of inflammation; these at first, however, are often so slight as to be easily overlooked. As the disease advances, the signs, both local and general, of inflammation, are too obvious to be mistaken; while all the consequences of inflammation, abscess, ulceration, adhesion, hæmorrhage, &c. gradually follow.

The admission of the connexion and dependence of this state of the lungs upon inflammation, is of great importance with regard to the treatment, more especially with a view to prevention; for although, from the state of the general health in these cases, as well as certain constitutional tendencies, a very active treatment is perhaps never called for, the treatment nevertheless should be conducted in reference to such an origin. Something may require to be done that, upon other views of the disease, would probably be neglected; and some things avoided, that are injurious.

With respect to blood-letting, the principal object of inquiry at present, I have to observe, that by the employment of this remedy when the first signs of inflammation appear, the mischief, in numerous instances, will be arrested in its course, and a fatal malady probably averted. At a later period of the disease, although the chance of recovery is small, still, as far as a cure is attainable, it is more likely to be accomplished by this than by any other means; while it has the further and great advantage of being the best and most effectual palliative of the most distressing symptoms of the disease: it relieves pain; lessens febrile heat, and the sweating consequent upon this; disposes to quiet and refreshing sleep; and restores appetite when lost.

I hardly need repeat, that in the employment of blood-letting for the purpose now stated, care must be taken to apportion the remedy carefully to the existing circumstances of the case. The quantity of blood taken at once should hardly ever exceed six ounces at a time; and, indeed, on many occasions, half this quantity will suffice. This may be repeated, at intervals of a week or two; and if, happily, the stomach be in a condition to take food moderately, and of the kind to which the patient is accustomed, (and, under this restriction, solid animal food is to be preferred,) the plan now indicated may be pursued for an almost indefinite length of time; not only without loss of strength, but with an increase of both flesh and

strength; as I have again and again observed. The digitalis, given in minute doses, (six or eight drops of the tincture,) and combined with some simple bitter, forms an exceedingly useful addition to the plan now mentioned.

The remoter consequences of inflammation in the chest requiring notice here, are adhesion of surfaces and preternatural accumulation of serous fluid in the cavity: and to these may be added, *asthma* strictly so called. Suppuration and ulceration in the lungs are rather to be considered as continuations, or advanced stages of the inflammation that constitutes the disease, than as merely consequences or sequelæ of it. They claim, therefore, no particular notice, except it be to observe, that we have no direct power of relieving them. Certain substances in the form of vapour, and some of the gases, have indeed been inhaled in these cases, so as to come into contact with the ulcerated surfaces; but, however natural it is to expect benefit from such means, the trial of them has generally ended in disappointment. Certain drugs, too, have been vaunted from time to time; for example, the *Uva ursi* — the Iceland moss—and a variety of others, as well as excoriating applications to the skin; in order to prove "consumption curable:" but with the same general result. A few years back, a mercurial course was said by an American physician to cure consumption with almost certainty; but in this country, the disease is almost invariably aggravated by such a course. In short, you will find, I believe, upon trial, that nothing really does good in phthisical cases, but means calculated to arrest the progress of the destructive inflammation that constitutes, as it were, the essence of the disease. Such means, if very early resorted to, have a tolerable chance of succeeding; at an advanced stage of the disease, this result is not to be expected. Happily, however, the same means are the best palliatives; and, when used with proper caution, tend to the prolongation of life, as well as the relief of present suffering.

Adhesion of surfaces, after the inflammation which produced it has subsided, is in general a matter of little moment; though it may tend to constrain somewhat the motions of the lungs, when unusual efforts in respiration happen to be made. But it admits of no aid from art.

Serous accumulation in the chest (badly expressed by the term *effusion*), is a variety of dropsy (hydrothorax) that is, I believe, invariably the result of inflammation; though by no means the only effect, for changes of structure, in different degrees, are always found to accompany the disease. In almost every instance of hydrothorax, likewise, inflammation continues along with the accumulation; as is known by pain in the chest, the small, hard, and accelerated pulse, heat of skin, and, above all, by the coated state of the tongue. In these cases, the removal of the still-existing inflammation is the chief object to be looked to, and this is effected, as far as it is capable of being so, by occasional small bleedings, which, when not prohibited by the general circumstances of the patient, are as safe as they are often useful. These small bleedings should be assisted by the digitalis, which not only does good by its sedative power, but, in many instances, acts also as a diuretic; its operation in this way being often followed by a rapid absorption of the dropsical fluid. Other diuretics, such as nitre, squill, &c. with or without calomel, appear to be sometimes useful.

It will not be altogether foreign to our purpose, to make a few remarks on *asthma*, strictly so called; for the purpose of shewing whether, and in what circumstances, blood-letting is admissible for its relief. In common language, the term asthma is applied to all cases of difficult breathing; but, amongst the faculty, it has been long confined exclusively to cases that recur periodically, or by paroxysms; and the prevailing notion is, that the disease is of a spasmodic nature, and not connected with inflammation; and, of course, not requiring antiphlogistic treatment — blood-letting more especially. Now I shall attempt to shew you that the theory here inculcated, as applicable to the disease generally, is not sufficiently established; nor is the practical inference deduced from it, free from exception.

Difficulty of breathing (*dyspnœa*) may arise from various causes affecting the organs of respiration. The most frequent, perhaps, is a preternatural thickening of the membrane lining the air-tube, the consequence of repeated catarrhal inflammation. When the affection recurs periodically, it has been called nervous or spasmodic asthma, implying, of course, that the immediate cause of the paroxysm is spasmodic action of muscles. But no muscular structure is found in the course of the air-tubes, with the exception of the larynx; and it is quite evident, from the manner of breathing in these cases, that the impediment is not seated in that part of the tube. During the paroxysm of asthma, the air passes into and out of the lungs slowly, and with great labour and effort on the part of the muscles both of inspiration and expiration; exactly in the way we should expect from a narrowed state of these passages. Now there is no necessity for supposing the existence of spasm, in order to explain the difficulty

of breathing in such cases; it is sufficient to shew the way in which the paroxysm may take place, without the aid of such a cause.

In most cases of periodical asthma, the disease may be traced back to catarrhal inflammation, as its source. Repeated attacks of this inflammation induce, at length, a permanent thickening of the membrane affected; and accordingly it is found, I believe with hardly any exceptions, that asthmatic patients breathe with more or less difficulty at all times; and in such a manner as to indicate clearly a narrowing of the canal in the smaller ramifications of the tube. Any additional impediment to breathing, therefore, will cause such an aggravation of symptoms, as to constitute a paroxysm, or asthmatic fit. Thus a fresh attack of inflammation (from taking cold, as we term it,) on the already-diseased and thickened membrane, by producing additional fulness or swelling, renders breathing nearly impossible; leaving no passage scarcely, for the admission of air into the lungs, and requiring the utmost efforts of the muscles of inspiration to expand the chest. This difficulty continues (in other words, the paroxysm lasts) till the swelling of the inflamed membrane subsides; an effect which seldom occurs till a copious secretion of mucus has taken place, when the paroxysm terminates; and this is the case in by far the greater number of instances of the disease.

Other causes of an irritating nature produce a similar effect, by increasing the action of the vessels of the part, and consequent fulness, which, though it may not amount to actual inflammation, has all the effect of this while it lasts. Thus the inhalation of acrid vapours of any kind will bring on a paroxysm of asthma where the predisposition to the disease happens to be unusually strong, though the duration of the paroxysm in such cases is less than where actual inflammation arises; for this has a determined and more protracted course.

Asthma, therefore, in a great proportion of instances, appears to be a catarrhal affection in its origin, while the paroxysms are brought on by temporary causes of vascular excitement, producing fulness and thickening of parts, which continue till the increased vascular action again subsides. The returns of the paroxysms are favoured by predisposition; this predisposition appears to consist in an acquired irritability, the natural and ordinary consequence of previous inflammation. The suddenness of the attack, in many instances, furnishes no valid objection to this theory; for the same thing may be observed in another portion of the membrane, namely, that which lines the nostrils; where, from *taking cold*, or from the application of tobacco, or other irritating causes, these passages become almost instantly obstructed, and are not again pervious till an increased secretion of mucus from the surface reduces the vascular excitement upon which the fulness and consequent obstruction depend; thus affording the strongest analogical proof of the correctness of the theory advanced.

What has been now said applies to by far the greater number of cases of confirmed or periodical asthma, the paroxysms of which are readily explained upon the ground stated, namely, temporary increased arterial action in the bronchial membrane. There is, in fact, no necessity for recurring to spasm in order to account for the phenomena of the disease. It remains to inquire, what is the kind of treatment which such a view of the matter suggests; and more especially whether any, and what advantage is derivable from blood-letting in the different circumstances of the disease. But this must be reserved for our next lecture.

ON INFLAMMATION,

AND THE MOTOR POWERS WHICH CAUSE AND REGULATE THE CIRCULATION.

By Professor Graves.

[Concluded from p. 609.]

It is scarcely necessary for me to direct your attention here to the phenomena which occur in the erectile tissues, as the corpora cavernosa, &c. It cannot surely be maintained, that the sudden increase in the afflux of blood to these parts, is owing to any *vis à tergo*, or momentary augmentation of the propelling power of the heart. No; tissues of this kind enjoy the power of attracting to themselves an increased quantity of blood, in virtue of the vital power resident in them, and not from any peculiar exertion of extraneous forces. In fact, the capillary vessels enjoy the property of actively dilating, and drawing the blood into them, and this appears to be one of the principal causes of the circulation. Of this there is abundant proof. It has been observed in vivisections, that after the heart has ceased to act, the capillary vessels remain distended, and appear to carry on their functions as long as any blood is supplied to them from the arteries. It has been also remarked, that the

larger arterial branches become first empty, then the smaller, and finally the capillaries. Dr. Philip states, that he has observed the circulation of the mesentery to continue for several minutes after the heart had been excised. This is the true explanation of the fact, that the arteries are so frequently found quite empty after death.

One of the strongest proofs we have of the power which the capillaries possess of drawing blood to themselves, is derived from the phenomena observed in vascular tumors. If scratched, or slightly wounded, these tumors frequently bleed to an alarming extent; while the division of the arteries which lead to them, and the removal of the whole mass, is attended with a comparatively small loss of blood. This is further exemplified in the familiar operation of opening the temporal artery. If the artery be only partially divided, and its connexion with the capillaries still to a certain degree maintained, it bleeds copiously: but if it be cut across, and the connexion wholly destroyed, it ceases to bleed altogether. Professor Smith, of Philadelphia*, amputated a leg below the knee, for dry gangrene of the foot and ankle. The great arteries were found wholly altered in their structure, being, as it were, converted into tubes of bone. Although pressure was completely removed from the femoral artery, and no means whatever were used to suppress the hæmorrhage, the quantity of blood lost did not amount to half a table-spoonful. At the same time the action of the heart was vigorous, and the pulse at the wrist of the ordinary strength and fulness. Now in this case some blood must have been passing through the tibial arteries before the operation, for there was some circulation in the leg down as far as the ankle, and the collateral arteries, or anastomosing branches, were not enlarged. If we refer to the phenomena of wounds which engage arteries, we shall find, as I have already stated, when alluding to the operation of opening the temporal artery, that the wounded artery of an injured limb bleeds much more than the same artery of an amputated one. Hence it is that branches which would pour out a large quantity of blood, if merely wounded, sometimes do not require a ligature at all, although their divided orifices open on the surface of a stump.

Another instance in which the attracting power of the capillaries may be seen, is in cases where portions of an amputated finger have again united, of which we have several examples. In this case the fluids effused by the upper cut surface are absorbed and circulated by the vessels of the amputated tip. But one of the most remarkable proofs of the position I have laid down is derived from the circulation of the blood in the placenta. In this instance the impetus which the blood possesses in the umbilical arteries, has been attributed to the *vis à tergo* derived from the heart of the fœtus. But after the detachment of the placenta, after the birth of the fœtus, the pulsation in the cord ceases, first at the placenta, and then at the umbilicus of the infant. After this period a section of the cord is not followed by any thing like the amount of hæmorrhage which might be expected from the division of vessels of such diameter, and in many instances there is no loss of blood whatever. Now why does the flow of blood cease in the umbilical arteries? The *vis à tergo* is as powerful after birth as before, and operates on blood in tubes free from obstruction. It cannot be attributed to cold, for the circulation continues in every part of the infant; nor to exhaustion, for the fœtus loses no blood, and its circulation is now independent of the mother. Neither is it owing to the action of the lungs, which are said to divert the blood from the placenta; for although a greater portion of blood is carried to the lungs after than before birth, yet this would not account for the total cessation of the circulation in vessels so large as the umbilical arteries. The explanation, therefore, commonly given is not capable of being proved. From the facts which I have brought forward, it would appear that the organic vital actions of the placenta depend upon its own life, and that when this body is detached from the uterus, it of course dies, and the functions of its capillary system cease. The suction-power of the small vessels then continues no longer to assist the *vis à tergo* in carrying blood through the umbilical

* This fact is mentioned in a monograph which I received from America many years ago. Unluckily I have mislaid it, and cannot call to mind the author's name. He advocated views similar to those I have here attempted to establish, and to him I am indebted for the argument derived from the placental circulation.

arteries, and the circulation declines, first at the placenta, and finally in the umbilical arteries, at their junction with the abdomen of the fœtus.

Such are the arguments derived from experiments and pathology in favour of the capillaries having a direct influence on the circulation. Those which may be borrowed from comparative anatomy are still stronger. If we look to the vegetable kingdom, we shall find that the force with which the sap, *i. e.* the blood of plants, circulates in their vessels is very great. Hales and Dutrochet have proved this by direct experiment. If a vine be cut down in spring to the distance of three feet from the ground, it throws out sap with such force as to raise twenty-one feet of water. In other experiments this power was found capable of raising thirty-two and a half inches of mercury, or thirty-five feet five inches and three-quarters of water; and thirty-eight inches of mercury, or forty-three feet three inches and one-third of water. To effect this prodigious circulation, as it takes place in plants, the force must be very great, for we know that it is capable of raising from the ground a large quantity of water, combined with nutritious principles, to the top of the loftiest palm or forest tree—in fact, to an altitude of one hundred and fifty feet. Now in what organs does this power reside? There is no central organ in plants, nothing like a heart—nothing like large arterial tubes. How, then, is the ascent of the nutritious fluid accomplished? Let us study the phenomena for a moment, and we shall find sufficient evidence to satisfy ourselves that *the fluid circulating in each part of the tree is brought to it chiefly by the action of the vessels of the part itself.* I do not mean to deny the great power which the spongioles of the roots, acting as capillary systems, exert in *driving* the fluids they absorb through the tubular vessels of trees; this power is no doubt aided by the buds and leaves, whose capillaries, when acted on by a proper temperature, discharge their vital functions with activity, and are capable of drawing the sap to the extremities of the branches. Thus in the case of a vine observed by Richerand, one branch of which had crept into a smith's shop, this branch remained in leaf, or rather threw out fresh leaves in winter, while all the other branches continued quite bare. Again, cut off a branch of a living plant, and place it in water: how actively does it absorb the water, and endeavour to prolong its existence. In winter this attraction of the ultimate ramifications of plants ceases, but returns again with the genial warmth of spring, when the buds begin to expand. Phenomena analogous to these are also observed in many animals. There are numerous tribes of animals possessing an active circulation, which have no heart whatever. Thus the Medusæ and Echinodermata, which must enjoy an active circulation, as is proved by their rapid growth, have no heart. In the Holothurio tubulosa, Cuvier has traced vessels going to the organs of respiration (pulmonary arteries), and vessels coming from the same (pulmonary veins), as also a system of arterial and venous tubes destined to carry on the general circulation, but no heart. There are numberless examples of this arrangement to be found in the animal kingdom. In fact, a great deal of the motion observed in the fluids of the human body is effected by other means besides the heart, and those means are the powers possessed by the capillary vessels and membranous tissues of the body, which, by virtue of an unknown law, aid materially in the circulation.

You perceive, then, gentlemen, that my views are quite opposed to those who assert, that in inflammation the enlargement of the capillaries is passive. Dr. Hastings and Dr. Philip allow that the capillaries dilate during inflammation, but they attribute this defect to debility. This, however, is a mere assumption. The phrases, passive and debilitated, put one in mind of another hackneyed expression founded alike on fallacy, namely, indolent ulcers. Now there is nothing more active than what is termed an indolent ulcer. It manufactures more secretion, uses more blood, and produces more pain than any equivalent portion of the same tissue throughout the body, and yet it is termed indolent! It is so with regard to the capillaries. It is said that in inflammation the capillary vessels are obstructed, and their force weakened. What is the real fact? Take an instance of conjunctivitis. What do you observe here? The affected membrane is swollen, its nervous sensibility exalted, its thermometrical temperature increased, its secretion augmented. Are any of these

symptoms of debility? I think they can hardly be looked upon as such. The increase of pain, heat, and fluid secretion—the augmentation in size—all the phenomena, in fact, are opposed to the theory of debility. There is no passive dilatation or weakness; the capillaries enlarge and dilate from increased, and not from diminished action; red blood finds its way into vessels which before received only white; and unusual secretions occur in the affected parts *The capillaries have the initiative; with them commences the enlargement, which afterwards extends to the smaller arteries, and from these to the larger branches.*

Under ordinary circumstances, the capillary circulation continues some time after the heart has ceased to beat, for the capillaries belong to that class of tissues which possesses an inferior degree of vitality; and it has been shewn by Bichat that such tissues survive those of a higher degree of organization. Hence, the capillaries continue to act for some time after the heart has ceased to beat; and as it is a law that the capillaries of the lungs will not transmit non-arterialized blood, the systemic veins become gradually distended, while the sytemic arteries are emptied, so that, after death, we seldom find any blood in the latter.

A very curious case, recently published by Dr. Houston, supports very strongly the views which I have put forward in this lecture. Dr. Houston had a very remarkable case, in which the circulation had ceased in one of the lower extremities. The foot, and afterwards the leg, were attacked with dry gangrene, of which the patient died. No obstruction was found in the vessels after death, and the ordinary injection passed readily into all the arterial ramifications. The arteries were all pervious, and apparently natural in their texture. Now, if the circulation of the limb had depended on the arteries alone, it would not have ceased so completely. Some time ago I attended a very remarkable case with Mr. Cusack. The subject of this case, a young lady of rather delicate constitution, was attacked at a certain hour every day, in a very singular way. The circulation in one of her legs seemed almost to cease, and the limb became remarkably pale and cold. This state of the limb would last for ten or twelve hours, and then an alteration took place; the leg became hot and painful, and its temperature became so disagreeable to the patient, that she was obliged to keep the leg outside the bed clothes, and have it constantly wetted with cold water and vinegar. During all this time the action of the heart was natural, and the circulation of the rest of the body unaltered. Here we have a certain portion of a limb at one period of the day quite cold and pale, and at another, extremely hot and painful. How can this be said to depend on any *vis à tergo?* The true explanation of the matter is, that it depends on a periodic affection of the nerves, capillaries, and smaller arteries of the part.

Such, gentlemen, are some of the arguments in favour of the supposition that the capillary vessels exercise a remarkable influence over the circulation. There are other proofs which I shall not touch on at present, as the more immediate business of the hospital prevents me from deviating any further from the path of strict clinical investigation. You may ask, perhaps, why I have entered on this subject at all, or why I have dwelt so long on matters which appear to possess only a mere theoretical interest. Because I am persuaded that much error exists with respect to the nature of the forces employed in carrying on the circulation, and because I think it of the most vital importance that you should be in possession of correct principles to guide you in the numerous emergencies attendant on the treatment of disease. The human body in its development from a lower to a higher degree of organization, loses none of its character, it ascends, retaining in its more perfect development all that is possessed in an inferior state. In the first stage of its development it possessed a diffused nervous and vascular system. It then acquired small nervous strings and capillary vessels, and finally larger arteries, larger nerves, nervous centres, and a heart. In the same way its circulation commenced, beginning in the smaller vessels, and extending to the larger, aided by the *vis à tergo*, but independent of it in a remarkable degree. From this view of the subject, it follows that, in many cases of disease, we are to look to the forces which regulate the circulation of the part affected, and not to any *vis à tergo*, or propelling power of the heart. The physician and surgeon

must study the life of each part, in attempting to estimate its morbid conditions. It was a want of proper knowledge on this subject which led to so many errors in practice. Among these I may mention the treatment of Egyptian ophthalmia, in which it was thought necessary to drain the patient of blood, for the purpose of subduing a mere local inflammation. In truth, the treatment of local inflammation, whether affecting external organs, as phlegmon, carbuncle, erysipelas, or internal parts, as pleurisy, peritonitis, &c., can never be properly understood, until the old doctrine, which (by teaching that the *vis à tergo* was every thing in inflammation) led to a too general use of venesection, has been laid aside, and sounder opinions adopted.

CASE OF HIDROTIC FEVER;

WITH REMARKS.

To the Editor of the Medical Gazette.

Sir,

Should you deem the following case of hidrotic fever of sufficient interest for your readers, you will oblige me by inserting it in your well-conducted journal.—I have the honour to be, sir,

Your most obedient servant,

John Brown, C.M.

Her Majesty's Victualling Yard,
Deptford, July 2d, 1838.

Mrs. ——, aged 30, of rather a delicate habit, was, on the 4th of May last, after a very easy labour, delivered of her fourth child. Her bowels had been kept open, for some time before her confinement, by mild aperients; and twenty-four hours after, half an ounce of castor oil was prescribed, which operated kindly. On the third day a slight acceleration of pulse took place, and continued for about twelve hours, during which time her breasts became distended with milk. On the 5th she rose, in order to have her bed made; and on the 8th, as she said she felt very well, she was allowed to dress, and to sit up for an hour or two. On the 13th of the month (the ninth day after delivery) she was attacked with shivering, which continued for a considerable length of time, and was followed by heat of skin and copious sweating. The pulse, after the cessation of the shivering, was soft and bounding; her tongue white. The lochial discharge was abundant. She complained much of headache, especially over her eyes, and said that her bowels were rather confined.

Half an ounce of castor oil was ordered to be taken immediately.

14th May.—Has passed a very restless night, during which she had two attacks of shivering; and she now complains of a sensation of cold in the spine. Her countenance is expressive of deep despondency, and she complains of palpitation of the heart, and of a sense of suffocation about the chest. Pulse 120, still soft and bounding. Skin wet with perspiration; tongue white; thirst considerable; urine pretty copious; no evacuation from the bowels.

℞ Ol. Ricini, ℥j. stat.

Vesperè.—Complains much of headache and thirst. Skin hot; perspiration very profuse; pulse 130; no stool.

℞ Hyd. Chlorid. gr. v.; Jalap. Pulv. gr. xv. Fiat pulv. ex vehiculo aliquo idoneo, stat. sumendus.

15th.—The patient states that she was drenched with perspiration during the whole of the night; that she twice experienced the coldness in the spine, which, after extending over the whole of her body, was each time followed by a great increase in the quantity of the perspiration. Her pulse, at present, is soft and frequent. The perspiration very profuse, and the urine abundant. The secretion of milk continues, and the lochial discharge is pale and pretty copious. The left breast, above the nipple, is hard and painful, and the skin is inflamed. The bowels appear to be obstinately costive; they have not yet been moved by the medicine.

℞ Infus. Sennæ Comp. ℥vj.; Magnes. Sulphat. ℥vj. Fiat mistura, de qua sumantur cochl. iij. mag. tertia quâque horâ, donec alvus commode purgetur.—Let warm applications be put to the left breast.

Vesperè.—The bowels have been very freely opened by the medicine. Stools rather dark coloured and offensive. Pulse 140. Skin wet with perspiration. The patient appears to be much dejected, and states that the sweat, which has a peculiar odour, is very distressing to her.

℞ Acid. Sulphuric. dil. gtt. xx.; Infus. Rosæ Comp. ℥ii. Ft. haust. ter in die sumend.

16th.—Has passed rather a better night. Pulse this morning only 90. Skin very moist, but cooler; headache continues. Complains of a pain in the right hypochondriac region, which, however, is not increased by pressure, and which, she says, she chiefly feels on moving. Bowels open; left mamma very painful.

Continue the draught, and let the warm applications be kept constantly to the breast.

Vesperè.—Had a slight shivering fit about mid-day, which was followed by heat of skin and copious sweating. Pulse, which was only 90 in the morning, is now 135.

Cont. Haust.

17th.—Perspiration still copious; pulse 100; bowels open; belly full and soft, in consequence of the bowels being distended with flatus. Tongue white; urine light coloured and copious, notwithstanding the profuse perspiration. Secretion of milk diminished, but not stopped. Lochial discharge moderate in quantity; less headach; abscess forming in the left breast. The patient called for the acid draught repeatedly during the night, and states that it was not only very grateful to her, but that it perceptibly diminished the perspiration, and afforded her relief.

℞ Quinæ Disulph. gr. xxxvj.; Aq. Rosæ, ʒxij.; Acid. Sulph. dilut. ℥xxxvj. Ft. mist. de qua sumantur cochl. ij. mag. tertia quâque horâ.

Vesperè (11 P. M.)—The pulse during the day has been very variable. In the morning, it was 100; at mid-day, only 90; at six P. M. 120; and now it beats 140 in the minute. Bowels uneasy, from distension by flatus. Much borborygmus.

Contin. haust. et mist.

℞ Tr. Valerian. Ammon.; Tr. Assafœtid. aa. ʒss.; Aq. Menth. Jup. ʒjss. Ft. haust. stat. sumend.

18th.—Great debility, much mental depression. Skin very moist. No pain in the right hypochondrium. Pulse 100, soft and round. Abscess pointing in the left breast. Less headach. Much flatus passed from the bowels during the night.

Cont. mist. et haust. Acid. Sulph. dil.

Vesperè.—The pulse has not been above 100 during the whole of the day, and the patient says that she has perspired less, and that she feels easier. Bowels not so much distended with flatus, although there is still considerable borborygmus.

Rep. haust. Valerian. et Mist. Quinæ.

19th.—Perspiration copious. Stomach very irritable. Frequent vomitings during the night. Abscess in the left breast has burst in three places, and a great quantity of matter has been discharged. No headach this morning, but the patient complains of an unpleasant sound in her head, which she says resembles the noise of steam passing forcibly through the funnel of a steam-vessel. Pulse 95. Tongue white. Less flatus in the bowels.

Continuantur remedia.

Vesperè.—Within the last hour the patient's pulse has risen to 133, and the perspiration which, during the day, was moderate, is now profuse. Bowels have not been opened since yesterday morning.

Ol. Ricini ʒj. stat. Mist. Quinæ et haust. Acid. Sulph. ut antea.

20th.—Exacerbations and remissions have been perceptible since the commencement of the fever, but, during the last two days, the remissions have been longer, and the exacerbations less violent. To-day the skin is not so moist, and the bowels have been freely opened by the castor-oil.

Let the mixture be taken every three hours during the remissions; and 30 drops of the Acid. Sulphuric. dil. four times a day, in half a tumbler of water.

Vesperè.—The patient says she feels so weak that she cannot bear to be moved. Her right breast is now hard, painful, and inflamed; the sound continues in her head, and she is very fretful and desponding.

Continue the Quina and the Acid, and let warm applications be kept constantly to the inflamed breast.

21st.—Has passed rather a better night. Pulse only 85. Right breast painful and throbbing.

Cont. rem.

Vesperè.—No exacerbation has taken place since the morning. Pulse 90. Skin still moist. Tongue pretty clean. No stool to-day.

Pil. Hydr. gr. x. horâ somni. Mist. Quinæ ut antea.

22d.—Has been very restless and uneasy during the whole of the night. The pulse is again 130. The skin is wet with perspiration, and the bowels have been twice opened by the blue pill, which she says always purges her.

Cont. remed.

Vesperè.—The pulse, which was 130 in the morning, is now only 97. Complains much to-night of the sound in her head.

Cont. remed.

23d.—The patient slept a little during the night, and says she now feels somewhat better. Pulse only 90. Skin moist. Urine copious. Tongue a little white.

Cont. remed.

Vesperè.—Nearly as in the morning.

24th. — Still continues somewhat better. Pulse 85. Very little perspiration upon the skin. Tongue clean. Great debility. Noise in the head more distressing. Let the quina be reduced in quantity, and taken in the following form :—

℞ Quinæ Disulph. gr. xxiv.; Aq. Pur. ℥xij.; Acid. Sulphuric. dil. ♏. xxiv.; Syrup. Rosæ, ℥ij. Ft. mist. de qua sumantur cochl. ij. mag. ter in die.

Vesperè.—Nearly as in the morning. No stool to-day.

Pil. Hydrarg. gr. x. h. s.

25th.—Has passed a better night than she expected. Pulse now only 95. Very little moisture upon the skin. Bowels have been twice moved by the blue pill. Matter has formed in the right breast, and the skin, in the under part, near the nipple, is clear and shining.

Continue the medicines, and the warm applications to the breast.

Vesperè.—Complains of great debility, loathes the sight of food, and states that she has taken nothing, since the commencement of her illness, except arrow-root and a little chicken broth. She refuses to take wine, and says it makes her sick.

26th.—Abscess in the right breast has burst close to the nipple, and is discharging freely. In other respects the patient is nearly as she was yesterday morning.

Cont. Mist. Omit. Acid. Sulphuric. dil.

Vesperè.—Had a smart rigor about five o'clock this afternoon, which was followed by heat and perspiration. Her skin is again wet. She is very fretful, and appears altogether to despair.

Cont. remed.

27th.—The patient has passed a pretty good night, and says she has had some refreshing sleep. Her pulse is now only 65. Her skin is cool and dry, and her tongue is pretty clean.

Cont. mist.

Vesperè.—Had another shivering fit about 5 P.M., which, however, was less severe than the last. Skin moist; pulse 98; bowels rather confined.

℞ Ol. Ricini, ℥i. stat.

28th.—Slept soundly from 4 A.M. till 7. No fever this morning. Skin cool and dry; tongue clean; pulse 65, soft and weak. No sound in the head. Complains at present only of weakness.

Cont. Mist. Quinæ.

Vesperè.—Remained free from fever until about half-past four o'clock this afternoon, when another paroxysm, similar to the two last, but less severe than either, took place. Her skin is again wet with perspiration, and she complains of slight headach, and great languor. Says she wishes to be allowed to take the acid again.

Let 30 drops be given three times during the night.

29th.—Has passed a very good night. The fever seems to have assumed an intermittent form, the paroxysms taking place daily, about five o'clock each afternoon.

Let half a grain of the Acetate of Morphine be given about an hour before the expected period of the paroxysm, and continue the Quinæ mixture.

Vesperè.—No paroxysm to-day. Has slept a little, through the influence of the morphine, and says that she is now very drowsy, and inclined to sleep.

No medicine.

30th.—The patient slept soundly during the greater part of the night, and this morning there is a marked change in her appearance. Her countenance, which hitherto has been expressive of deep melancholy, has now a cheerful aspect. She says she feels much better, and that she looks forward with confidence to a favourable termination of her complaint.

Cont. Mist.

Vesperè.—Improving. No paroxysm. Eat an egg and a piece of toast for breakfast, with a relish; and for dinner she took a little calf's-foot jelly, and drank a glass of wine, which she says was exceedingly grateful to her.

Cont. med.

31st, *Vesperè.*—Doing well. Took a cup of tea, an egg, and a piece of toast, again, for breakfast; and, for dinner, she eat a mutton-chop, and drank two glasses of port-wine.

June 2d.—Still improving. No paroxysm since the 28th ult.

7th.—Has continued to improve since last report. Her appetite has returned pretty keenly, and she is allowed as much nourishing food and wine as she feels inclined to take.

15th.—The patient is now quite convalescent. Her appetite is good, and her strength is returning.

Remarks.—Hidrotic fever presents itself to our observation in different forms, some of which are more dangerous than others; but, in all cases, during its progress, there is a great liability on the part of some organ or other to become affected with inflammation. Sometimes the mammæ are attacked; sometimes the uterus; and we frequently find a tenderness about the pelvis, which is generally the precursor of swelled leg. But let the inflammation be seated where it may, we ought, in the treatment, to bear in mind that that inflammation is an *effect* of the fever; and, when it fixes itself in the breast, we ought not to endeavour to subdue it by energetic antiphlogistic means, because, in hydrotic fever, there exists a disposition to metastasis; and if we were, in such cases, to have recourse to leeches and saturnine lotions, we might cause the inflammation to quit the mammæ, which certainly is the safest place it can occupy, and to establish itself in some other more important organ. Instead, therefore, of endeavouring to check mammary inflammation, when it takes place as a consequence of hidrotic fever, we ought, I think, to hasten its termination in abscess, by the application of heat; but, when the uterus or any other important external organ is attacked, antiphlogistic measures ought at once to be adopted; and I may add, that when the peritoneum becomes affected, the fever assumes its worst form, and very often, in spite of the best directed efforts of the healing art, proves fatal.

In hidrotic fever the mind is generally more or less affected. Occasionally, as in the above case, there is great despondency. The patient has a dejected, melancholy look, and often appears to be unnaturally placid. At other times, there is much mental excitement. The woman is very voluble. She speaks sharply. Her actions are quick and peculiar; and Dr. Blundell informs me that puerperal mania is not an unfrequent attendant upon this fever. But those symptoms depend upon a morbid state of the nervous system, and are not essential to the fever which may, and sometimes, though certainly very rarely, does exist, without any of them being very plainly manifested.

According to my observation, the essential or pathognomonic symptoms of hidrotic fever, are shivering, more or less violent, which generally recurs several times, at irregular intervals, followed by heat of skin and very copious distressing perspirations. In most cases, a soft, bounding, and always, with regard to frequency, a very variable pulse. White tongue, and headach, generally over the eyes, during the hot stages.

There are other symptoms which may be called accidental, as they sometimes are met with and sometimes not; but when those just mentioned exhibit themselves within nine or ten days after delivery, with or without local inflammation—with or without any very marked cerebral affection—we may conclude that we have before us a case of hidrotic fever; and I may here remark, that some years ago, when I first began to look upon this malady as a distinct disease, I observed, that, in proportion as the perspirations were profuse, the shiverings were frequent and the fever severe; and it occurred to me, that if the sweating could be diminished, the fever might be mitigated. With this object in view, I had recourse to diluted sulphuric acid, and it was with the concurrence of Dr. Blundell that that remedy was given to the patient whose case I have just narrated, in doses of thirty drops, in the infusion of roses, three a-day; but she took it oftener: indeed she frequently called for it in the night time, as it was not only very grateful, but obviously diminished the sweating, which, when profuse, she said was very distressing to her; and I

think it right to state, that, according to my experience, diluted sulphuric acid is a very valuable remedy in the fever now under consideration.

Hidrotic fever is, in my opinion, a remittent fever; although, in some cases, the remissions and exacerbations may not be very obvious, still, if careful observation be made, they will, I think, generally be found more or less distinctly to exist. In the lingering form of this complaint, the remissions are often very remarkable, but the fever frequently continues for a long time. It seems loath to quit its hold; and in favourable cases, it sometimes assumes an intermittent form before it takes its final leave of the patient. Looking, then, upon hidrotic fever as a remittent, and observing that great debility takes place during its progress, I generally, in cases similar to the above, prescribe the disulphate of quinæ; but it ought to be given in large and frequently repeated doses, or it will produce but little effect. In the above case the patient took, with Dr. Blundell's sanction, for a considerable length of time, not less than twenty-four grains every twenty-four hours, with decided advantage.

I do not know what effect mercury would have in the lingering form of this complaint, as I have never seen a case in which the constitution has been fairly brought under its influence; but I think I have reason to congratulate myself for having placed my confidence in diluted sulphuric acid, the disulphate of quinine, and aperients—a practice which I recommend to my professional brethren, in the hope that they may be induced to follow it, in the particular form of the complaint now under consideration, until further experience shall enable us to discover some better method of treatment.

It is to Dr. Blundell—a man of very acute observation—that we are indebted for much of the knowledge we possess of hidrotic fever. He it was who first delineated this complaint, and the picture which he has presented to our view is a very valuable one, because it has been drawn from nature; and I may observe, that the mere circumstance of having given to hidrotic fever its name, is a point of great importance, because, so long as practitioners knew it not as a distinct disease, they were, when they met with it, necessarily perplexed, and their practice was undetermined; but now that it has been formally introduced to us, it is to be hoped that we shall soon become well acquainted with its nature and causes; and I trust I may take the liberty to say, that it would be much to the advantage of the profession, if men who are extensively engaged in practice would make a point of communicating the results of their experience in the treatment of diseases in general, through the medium of the medical periodicals. It is true they cannot be expected to devote much time to the language, but it matters very little in what garb the information, which might in this way be communicated, is clothed: all that can be wished for are faithful descriptions of important cases, new views of treatment, &c.; and if this course were more frequently followed than I fear it is at present, much useful knowledge, which is confined to individuals, would be diffused, and the good effects of the practice would speedily be felt. It is therefore with the view of adding my mite to the knowledge which we now possess of hidrotic fever that I write this paper for publication; and I beg, in conclusion, to express my sincere thanks to Dr. Blundell for the interest which he took in the above case; indeed, he acted towards me with great urbanity and kindness. He devoted a considerable portion of his time to consultations with me respecting the treatment, and he certainly was instrumental in assisting me to conduct my patient—in whose welfare I had cause to take a very deep interest—safely through a disease which, at one time, I had reason to fear might have proved fatal.

ON THE EXTRACTION OF TEETH.

To the Editor of the Medical Gazette.

Sir,

The object of the present communication is to facilitate what has always been considered a formidable and painful operation, viz. the extraction of teeth. The reduction of this part of the healing art to scientific principles has been long a desideratum.

The instruments which have been from time to time employed in its execution, belong to that class of mechanical powers called levers; and those at present in general use are the forceps,

the key, and the elevator. It is quite certain that no possible improvement in the construction of these will ever deprive the operation of that pain which must always attend the forcible separation of one living part from another; but at the same time, the proper or improper mode of applying the levers is of great importance.

I shall endeavour on the present occasion to point out the mode of action, advantages, and disadvantages, attending their use.

In the hands of a skilful operator, I suppose that no instrument is better adapted to the purposes intended than a well-constructed pair of forceps. After grasping a tooth, they may be considered as a lever of the second kind; viz. that in which the fulcrum is at one end, the power at the other and, the weight between them; for the extremities of the roots will constitute the fulcrum, the attachment of the tooth to the alveolus, &c., may be considered as the weight, and the power will be vested in the handle of the instrument. Since, in all levers of this class, advantage is gained in proportion as the weight is further removed from the power than from the fulcrum, it is evident that the longer the handles of the forceps, the greater will be the force, and the exertion of that strength which is generally considered requisite in the hand and wrist of the operator will thus be rendered unnecessary. When the tooth is grasped by the forceps, they move it from side to side, and therefore become not only a single but a double lever of the order above described. It should be particularly observed that the fulcrum is placed in the tooth itself—viz., at the extremity of the roots, and that there is, in addition to this power of leverage, a perpendicular elevation exerted by the muscles of the operator's arm.

The disadvantages of the forceps are, that they are often difficult of application, that they are seldom properly constructed, and their use is not generally understood.

The key is much more easy of application than the forceps, and is a lever of the first kind—viz, that which has the fulcrum between the weight and the power: (levers of this kind are considered the best; they gain in power, but lose in velocity). The force, indeed, does not act in a continuous line with the weight and fulcrum, but parallel with it, being removed to some distance by a steel arm for the convenience of application; but its principle of operation is precisely the same as it would be if the fulcrum were continuous with the ivory handle, which, in that case, would be the power, the fulcrum being the steel bolster of the instrument, and the tooth the weight to be raised. The claw is simply a mechanical contrivance to lay hold of the tooth; the steel bolster or fulcrum rests upon the gum, and the power is exerted in the ivory handle, for without it the instrument evidently could not be used, the length, size, and form of the steel arm or connexion between the power and fulcrum being of no importance. The essential qualities of the instrument, therefore, rest in the claw and the ivory handle: the latter should be of sufficient size to be grasped by the whole width of the hand. Much depends upon the claw and upon the size of the circle of which it forms a section. The same claw is not adapted to a molar and an incisor tooth; for if the circle be too large, the fulcrum slips down upon the gum, which becomes very much contused in consequence between it and the alveolus, producing a wound often difficult to heal. If the circle be too small, the fulcrum will most probably not rest upon the alveolus at all, and the tooth may be broken.

The necessity of the proper adaptation of the size of the claw to the size of the tooth, is well attested by the common practice of wrapping lint or other substances round the fulcrum: this is, however, any thing but a scientific practice, and is often indiscriminately used both for large and small teeth, without attention to the principle upon which the instrument acts.

On account of the facility of its application, the key has perhaps received more encouragement than any other instrument; but the want of adaptation of the claw is a very serious objection, producing in many cases severe contusions and lacerations of the soft parts, not to mention fractures of large portions of the alveoli, which, if they are removed at the time, deprive the neighbouring teeth of their support, or if they remain loose give rise to all the evils of exfoliation. It should be remembered, also, that the key has a leverage only in one direction, and its fulcrum rests upon the gum, not being the tooth itself, as in the forceps.

There is no occasion to multiply sur-

gical instruments, especially modifications of the key; but its imperfections are constantly complained of; so much so, as in some measure to supersede its use by the adoption of the forceps, though, on account of the difficulties of their application, they will most likely never be used to the same extent, except by professed dentists.

The plan adopted by myself is as follows:—The tooth to be extracted is measured just below the enamel with any proper instrument, as a pair of callipers, and a claw of my own construction is regulated accordingly. It consists of two pieces connected to each other (at a part where an increase in bulk is of no importance) by a screw and a nut: the points of motion should be plated with silver, to prevent their rusting; but this is not necessary if a little care be taken to keep them oiled. The circle of the claw by this means can be contracted and expanded at pleasure, and the distance between it and the fulcrum regulated, so as to keep the latter from slipping down and contusing the soft parts.

The fundamental evil, however, still remains—viz. the pressure of the fulcrum upon the gum: this I shall endeavour to obviate, though I have not submitted my alteration to the test of experience sufficiently to justify me in sending you an account of it at present; but the above I think I can confidently recommend as an improvement.

The elevator is also a lever of the first order, the fulcrum being made according to circumstances—sometimes the operator's hand, the neighbouring teeth, alveoli, &c. It is a good instrument in the hands of a dexterous operator, upon whom its value entirely depends.

I remain, sir,
Your most obedient servant,
Thos. Edwd. Eden, M.R.C.S.

248, Regent Street, July 1838.

experience and ingenuity which suggested the plain and sash bandages, as also the pretty addition such an apparatus would be to the accoucheur's instruments. I must say I cannot see any improvement upon the light and simple skirt (already so well known) used in the Dublin Lying-in Hospital, with which you can always supply yourself at the bed-side of the patient, by causing the nurse to sew two towels together, one horizontally, and the other perpendicularly, which, when properly applied, cannot slip. I am averse to applying any bandage before the patient is placed comfortably in bed, and insist that every accoucheur ought to sit by his patient with the uterus in his hand, for at least an hour after the expulsion of the placenta, as by this means only is he certain what state the uterine tumor is in; and having seen his patient dry in bed, he is to apply a skirt. Now as keeping the woman cool, and interfering as little as possible with the already contracted uterus, should be the object, the means employed to support the abdominal muscles ought be as light and uncomplicated as possible. To make pressure, by a firm unyielding substance over the region of the uterus, permanent by any mechanical contrivance, with the belief of preventing hæmorrhage, appears to me an unjustifiable and hazardous practice; inasmuch as to do so in every case is obviously unnecessary, and must be injurious by its local pressure, more particularly in those cases where there is a hæmorrhagic tendency; for I am of opinion that a practitioner can alone control distension, or promote firm contraction of the uterus by manual pressure and dexterity, which the author of the article alluded to admits has so signal an effect, but so soon tires of.

Your obedient servant,
D. Culham.

Croydon, July 7, 1838.

PRESSURE ON THE UTERUS AFTER DELIVERY.

To the Editor of the Medical Gazette.

Sir,

Having read an article in your journal of the 23d ult. upon the best means of making pressure on the uterus after delivery, I am induced to make the following remarks, acknowledging the

CAPILLARY CIRCULATION OF THE BLOOD.

To the Editor of the Medical Gazette.

Sir,

As the circulation of the blood through the heart and all the several sections of the sanguiferous system has of late years deservedly engaged the particular attention of many physiologists, lec-

turers, and writers, and seems still to continue to be the theme of many a valuable paper, from time to time, in your excellent journal, I beg to forward for insertion in its pages a transcript of some corollaries I deduced from a rigid series of experiments on the membranes of living animals, and which were published, with many correlative proofs and arguments, at pages 62—5, in my "Short Inquiry into the Capillary Circulation of the Blood," &c. in 1825. I am the more induced to beg your republication of them, as the "Short Inquiry" has been some time out of print; and as they may, by your kindness, re-exhibit a record of the observations and deductions of one inquirer, with which those of others, founded on experiment, may be compared, and which the merely theoretical physiologist may perhaps be led to review, along with the previous and later investigations of other inquirers, before he makes up his mind on the great doctrines of the blood and its circulation; and I am, sir,

Your obedient servant,
J. Black, M.D. F.G.S.

Bolton, July 9, 1838.

Corollary 1st. The greater part of the capillary blood-vessels are distributed in a reticular manner, the innumerable meshes of which chiefly affect the quadrangular form; and this tissue is principally composed of veins, and is intersected and supplied by more linear capillary arteries, endowed with apparent muscular contractility.

2nd. The more natural and undisturbed the circulation, and the stronger the animal, the greater is the velocity of the blood, except, sometimes, immediately after the application of a comparatively slight stimulus, which increases the circulation a little.

3rd. The healthy and natural circulation in the capillaries depends directly on a tonic or tensive state of their delicate tubes; and mediately, on some power inherent in the part, a degree of which power seems to reside in the blood equally with the vessels, and its energy depends more on the integrity or vitality of the part or parts of this system (capillary,) than upon any impulse derived from the heart.

4th. The effects of a stimulus are according to the length or intensity of its application; and as regards the vessels to which it is applied, they are proportioned to the vitality of the part, and to pressure of the column of blood in the leading arteries.

5th. A strong stimulus suddenly applied, in a short time produces a contraction of all the fibres of the part, and a diminution of the diameters of whole classes of vessels, but more particularly of the arteries, which is followed at a greater or less distance of time, by a corresponding dilatation; according to the extent of which, the phenomena of inflammation will be more or less observable, and more so if the *vis à tergo* be powerful or stimulated.

6th. A gentle or less stimulus produces an evanescent contraction, followed by relaxation, increased redness, and a slight acceleration of the blood in the leading capillary arteries and veins, owing, it seems, to the increased capacity of the distended reticular vessels, while the former are yet relatively contracted.

7th. The circulation in the capillary system is independent of the control of the heart, except so far as this organ affords a constant pressure and a ready supply of blood, upon which the capillary vessels may act; and it is still more independent of the brain.

8th.—There are three states in which the capillary system may be situated, the more perfect degrees of which are—1st, an *atonic* or *collapsed* condition of the vessels, wherein no circulation takes place, nor can red blood be observed, but the return of both which can be more or less easily accomplished. 2d. The *tonic* or *tensive* state, which is the natural one, and wherein the circulation is brisk, uniform, and capable of being slightly accelerated by heat and moderate stimuli. The 3d is the *turgid*, *distended*, or *congested* state, wherein the blood has a diminished velocity; but, if distension has not reached a certain point, contraction and a quicker circulation can be restored. This last condition is a species of atony opposite to the first: both, however, depending on weakness or exhaustion in the part, but with a different contingency of the *vis à tergo*, or the pressure of blood from the larger arteries.

9th. The *vis à tergo* being a constant force, the velocity of the blood is inversely as the diameters of any set of

vessels, compared with the capacities of those which immediately succeed them: thus congested capillary veins, and a quick circulation in the larger capillary arteries, are quite compatible.

10th. The condition of a part in what is termed inflammation, is essentially seated in the capillary vessels; and primarily and chiefly in those of the veins.

11th. Inflammation and congestion affect a portion of the capillary tissue, more from a local or topical condition than from any action communicated by the vessels leading to the part.

12th. Congested, and even stagnant blood, may be brought again into circulation; while a part that has been inflamed or turgescent for a short time, may be found pale, flaccid, and collapsed, in a dying animal.

And again: at p. 91 — Then it appears that in the healthy and natural integrity of the capillary circulation, the blood is supplied by the propulsive action of the heart, in a constant reservoir of streams, which the capillary vessels feed upon with an independent action; that the blood is transmitted through this system of vessels by a power which is resident, in some degree, in the blood itself, as well as in the vessels; that this power is an inherent constituent of the part, including all its organizations, and may be said to be of the identical nature with the power by which a muscle contracts and the heart performs its constant functions; and that the blood circulates through this system (capillary), in a *uniform and brisk* current, without pulsation or undulation, and that the vessels are then in a *tensive* or *erectile* state, suffering no distension from the blood, nor yet embracing their contents with any physical pressure or constriction.

ANIMAL MAGNETISM.

FALLACY OF CERTAIN EXPERIMENTS AT UNIVERSITY COLLEGE HOSPITAL.

To the Editor of the Medical Gazette.

Sir,

The inclosed communication was forwarded to the Editor of the Lancet, with a request that he would insert it in the forthcoming number. This was done, as it was in his journal that the misstatements which called forth my observations appeared, and I therefore thought (supposing the errors to have been unintentional) that the most proper place for their correction would be in his columns. It appears, however, for reasons which I shall not trouble myself to comment on, but which will perhaps be obvious to most of your readers, that the communication was not such as suited his pages. I have therefore taken the only course which remains open to me, in order to correct the misstatements with which my name has been coupled, and I shall therefore be obliged by your inserting the accompanying paper at your earliest convenience.—I am, sir,

Your obedient servant,

James Blake.

University College,
July 10, 1838.

To the Editor of the Lancet.

Sir,

In the last number of your journal I find my name mentioned as having been present at some magnetic experiments performed at University College Hospital on the 21st June, and coupled with it certain indirect reflections on the manner in which these experiments were conducted.

Now as Dr. Elliotson did me the honour of allowing me in a great measure to conduct these experiments, I might even on these grounds have been justified in making a few remarks on the report in question. I should, however, gladly have allowed this to pass unnoticed, had not the incorrect statements contained in your report been such as are likely to mislead the public in a matter which is now attracting considerable attention, and on which it is of the greatest importance to the medical profession, as a scientific body, that right opinions should be formed. Under these circumstances I feel myself called upon to give a short statement of the facts as they actually occurred, in order that, in one instance at least, the public may have an opportunity of judging for themselves of the reality, or illusive nature of these supposed magnetic phenomena; and also, that my name might not remain connected with statements which are any thing but correct.

Had it not been for this, I should have much preferred being a silent spectator of the extraordinary scene which is now being acted, both on account of the relation in which I stand to those who view mesmerism in a totally different light to myself, and for whose opinions I entertain the greatest respect; and also to my being averse to take any part in a controversy which, in many instances, has not been conducted in a very scientific manner. The remarks I have to offer on the facts I shall bring forward, and on the misstatements contained in your report, have I find extended themselves to a length far beyond that which I had originally intended, but as it is impossible to treat the subject fairly in a shorter compass, I shall be obliged by your allowing me on this occasion to occupy so large a space in your pages, as it is for the purpose of correcting errors, which your widely circulated journal has tended to diffuse.

JAMES BLAKE,
Medical Student.
University College, July 9.

The only experiments to which I shall allude*, are to those performed on the 21st June, in order to observe the effects of water, to which a degree of magnetic power was supposed to have been communicated by immersing the fingers in it for a few seconds. This water was tried in two ways — by placing it within the lips by means of small pieces of stick, which had been dipped in the water, and also by the girl drinking the water from the glass. But before going into an investigation of the laws which were supposed to govern these phenomena, it was necessary to prove that any such phenomena really existed. For this purpose a number of glasses, containing water, were placed on the table; some of these were magnetized, others were not; and sticks which had been dipped in these glasses were applied to the lips. When this was done by Dr. Elliotson the experiments certainly succeeded much more frequently than they failed, the girl being generally sent to sleep when magnetized water was applied. But there was one important source of fallacy in these experiments to be guarded against; and which when taken advantage of by girls whom a long-continued tutoring has brought to seize the slightest hint as to when magnetic phenomena were to be expected of them, is quite sufficient to throw a doubt over the results obtained, supposing even the experiments never to fail.

I allude to the great difficulty which a person who believes in the power of mesmerism has of totally concealing from close observers, like these girls, those occasions on which results are expected to follow the application of so powerful an agent. In the present instance, for example, although the girl did not know which glasses were magnetized, and which were not, yet the manner in which the stick was applied to her lips, was often quite sufficient to give her a hint as to when the magnetic phenomena were expected to take place. I observed, that if the stick had been dipped in unmagnetized water, it was carelessly passed through the lips; but if magnetized water were being used, it was applied in a more careful manner between the lips, and results were evidently looked for, and this without any intention of deceiving on the part of the operator, for none can doubt from the opportunities afforded by Dr. Elliotson for the investigation of the subject, and also from the obliging manner in which he is always ready to receive any suggestions which may be offered on it, that he at least, in pursuing these researches, is actuated by no other motive than an ardent desire for truth, and from the elucidation of which he is not to be turned by any obstacles which may be thrown in his way. That the circumstance above stated is a source of fallacy, is, I think, proved, by my succeeding on many occasions in sending the girl to sleep when using unmagnetized water, and inserting the stick in her mouth in such a manner as if I expected results would be produced; and also by the frequent absence of any effect when magnetized water was applied in in an indifferent manner. This was so palpable, that it was found necessary to seek for some explanation to meet the difficulty. On this, as on all other occasions, one was easily found, and this was, "that the sticks themselves *might* become magnetized by be-

* It was these experiments which were performed on the 21st June, and not those with the various solutions, as erroneously stated in the report, these latter experiments having been performed on the 20th.

ing held in the hand whilst using them;"—a phenomenon, however, which had never before presented itself, when the girl had an opportunity of knowing which water was magnetized, and which was not. Those who offered this explanation, apparently forgot also that it entirely overturned the whole of the experiments which had been before performed with these hitherto unoffending pieces of wood, and in which the most striking results had been obtained; and that the long list of substances, with *si* and *st* after them, which appear in your journal, thus suddenly lose all claim of being concerned in the production of magnetic phenomena, on account of the sticks with which they were applied having become magnetized. This instance will furnish a fair specimen of the manner in which the experiments are conducted, and of the reasons which are brought forward to account for their failure, but which are not "*considered by every observer to supply a reason which meets the difficulty of the case.*"

The most important experiments, however, were those performed with drinking the magnetized water, as they overturn one of the strongest arguments which the supporters of magnetism bring forward in support of their theory (viz. the necessity for allowing the existence of some power for producing these effects), by proving that these effects are produced when there cannot be the slightest doubt that no magnetic influence was concerned. It is these experiments which are alluded to in so *candid* a manner by your reporter, when he states, that "the drinking unmagnetized water, on one occasion, was followed by stupefaction." It was not on *one occasion* only that this unmagnetized water produced so unfortunate a result, but I am confident that at least six times, and I believe much oftener, was she sent to sleep by drinking unmagnetized water. I am fully aware, that, on many of these occasions, objections were offered as to the manner in which the experiments were conducted; and when these objections were done away with, and still the phenomena did not take place properly, other objections were brought forward: such, for instance, as, "the glass becoming magnetized by O'Key's sister having held it in her hand for a few seconds." When a clean glass was used, and the experiment did not succeed, then "the water in the jug had become magnetized, by standing so near the water in the glasses which was magnetized." When water fresh from the spring and clean glasses were used, then "the hand of the person who gave her the glass magnetized it." When this source of fallacy was avoided, and fresh water and clean glasses were used, which she was allowed to take from the table herself; and yet, when, under these circumstances, she went to sleep, on drinking unmagnetized water, it was advanced that the unmagnetized glasses were too close to those which had been magnetized, and had thus obtained a slight tinge of magnetism from them. This was attempted to be avoided by placing the glasses at a considerable distance from each other: for this purpose, two clean glasses were taken, and placed at a distance of five feet apart: into each of them, water which had been freshly drawn was poured, and one of them was then strongly charged with magnetism. The girl was then brought into the room and desired to take some water from the glasses; the first she took up happened to be the magnetized one, and scarcely had it been applied to her lips before the magnetic phenomena manifested themselves in the most perfect manner. The experiments were then discontinued, it being stated, that owing to the manner in which they had been interfered with, and the many sources of fallacy, it would be necessary to repeat them. Since this time they have been duly repeated, and when proper precautions have been taken, always with the same results. I state this on the authority of some gentlemen who assisted at them, as I was not present myself.

I shall now proceed to notice the fallacies which, in my opinion, do not account for the failure of these experiments, but which, when they succeed, are the cause of their success; for it is a curious circumstance, that the greater the precautions taken to avoid sources of fallacy, the less seldom do the experiments succeed.

The first great source of error is the perfect faith placed in the subject of these experiments. So far has this been carried, that Dr. Elliotson has declared

that he hardly thought it worth while attempting to convince any person of the reality of these phenomena, who could entertain an idea that the girls are impostors. Owing to this perfect belief in their sincerity, the experiments which are about to be performed are often talked over with the utmost freedom in their presence; or should any very great precaution be taken, they are sent into a state of supposed insensibility, in which state it is thought that they are perfectly unconscious of all that is passing around them, but in which state I feel confident they are awake to every word that is uttered, and are employed in forming plans for keeping up the deception. The most striking proof of this is afforded by the almost constant manner in which experiments with magnetized water failed, when attempts were made to deceive the girl by saying in her presence, whilst in this supposed state of insensibility, that in the next experiment magnetized water would be used, unmagnetized water being had recourse to. This was more particularly remarked in some experiments which were performed on the 29th of June, at which I was not be present; but the results of which were furnished me immediately after their performance, by a gentleman who assisted at them.

The next great source of fallacy is the anxiety shown by many to obtain results, when these results ought to take place. I have already noticed one instance of this in regard to the sticks. Another one which I observed, and of which I availed myself, in order to deceive the girl, was the following:— When the girl was drinking a glass of water, which she was at a loss to know if it were magnetized or not, she would cast down her eyes, and hold the glass motionless to her lips for a second or so: if the water were magnetized this would be immediately followed by an exclamation from Dr. Elliotson or Mr. Wood, that she "was fixed," when she was sure to become fixed accordingly; but should the water not be magnetized, no such exclamation would escape from these gentlemen, and the ingenuous young lady would be perfectly sure that she might still retain her sensibility. It was by making use of this expression when she was drinking the magnetized water, that I succeeded in fixing her on two or three occasions with this powerless agent; and it was by requesting Dr. Elliotson to make use of it when she drank the unmagnetized water in the experiment last detailed, that she was fixed in so remarkable a manner on that occasion. I should occupy too much space were I to attempt to enter into a detail of the various sources of fallacy. There is not the slightest hint or sign which will not be immediately seized by these girls, whose whole time I feel convinced is occupied in considering how they can keep up a deception, which, whilst it lasts, renders them objects of so much interest and attention, and which furnishes them with an asylum in an hospital where they are on the best of terms with the greater number of those who frequent it.

I cannot conclude without offering a few remarks on one or two paragraphs contained in the report. I have already noticed the misstatement in regard to the girl being sent to sleep but once on drinking unmagnetized water, and have only to add that the succeeding part of the paragraph is in perfect accordance with its commencement. In regard to the miserable attempt made to answer the question of, why did the "magnetism fail?" I have only to add that I consider it one of the most shameless subterfuges ever made use of in the course of any inquiry professing to be conducted in a scientific manner; and certain I am it is one at which every scientific supporter of magnetism must blush. The fact of a gentleman, on one occasion only, having for a few seconds attempted to produce magnetic phenomena behind her, and which attempt was immediately desisted from on being noticed, is to be brought forward, forsooth, as accounting for the continued failure of experiments which were conducted during two hours.

I shall now conclude by bringing forward a rather striking illustration of the assertion that in the case of the O'Keys "*there has never been forgetfulness of the part to be acted even for a moment.*" A gentleman having ascertained that Jane O'Key was in a state of susceptibility to the passes of the hand, by having sent her to sleep by a single pass, kept her in conversation for a few minutes, in the course of which he stated that it had lately been discovered at Paris that red spots could be removed from the face by means of magnetism, but that it required a quar-

ter of an hour's manipulation to do this; at the same time offering to remove a mark of this kind from her face —an offer which was gladly accepted; and during a quarter of an hour was she submitted to the magnetic passes, without the slightest effect being produced, although not five minutes before a single pass of the hand had been sufficient to produce a perfect state of insensibility.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Manual of British Botany; in which the orders and genera are arranged and described according to the natural system of De Candolle: with a series of analytical Tables for the assistance of the student in the examination of the plants indigenous to, or commonly cultivated in, Great Britain. By D. C. MACREIGHT, M.D., &c. London, 1838. Small 8vo. pp. 296.

THE reason why so many British Floras have been published, is obvious enough. It seemed natural to suppose that indigenous plants would be the easiest to find, and consequently those to which the attention of the student should be first directed. This theory, however, though true in many instances, fails in so many others, that it may be doubted whether the rule has not more exceptions than examples. Thus the native grasses are far more common than any exotic plant; but, on the other hand, the cultivated species of the rose are more easily met with than half the plants of the British Flora. Hence, although it is quite proper for the sake of completeness, that a manual of British botany should contain all the plants growing wild in this island, it must often have struck many persons that it would be equally desirable to have a compendium of those exotic plants which are to be met with in every garden, and in myriads of *horti imaginarii*, as Pliny calls flower-pots. This desideratum has been in a great measure supplied by Dr. Macreight; and the reader will find in his useful manual an account of the laburnum, lilac (*corchorus kerria*, or *corchorus Japonica*), magnolia, syringa, virgin's bower, rhododendron, jasmine, Virginian creeper (*ampelopsis hederacea*), Spanish broom, bladder senna (*colutea arborescens*), phlox, dahlia, hollyhock, lavatera, sunflower, mignonette, lavender, hyacinth, jonquil, balsam, marvel of Peru, nasturtium, pheasant's eye, cardinal, Chinese primrose, musk plant, China aster, coreopsis, zinnia, passion flower, fuchsia, cockscomb, marigold, French marigold (*tagetes patula*), African marigold (*tagetes erecta*), globe amaranth, fleur-de-luce, hydrangea, camellia, petunia, Clarkia, and scores of other common plants,—all of which are fully as interesting as our field southernwood, or our spiked speedwell, with the additional advantage of being far more easily procurable; unless indeed we should allow, as perhaps we must, that rarity is of itself a circumstance of interest, in which case no one of the list of foreign flowers just cited can compete with the two natives of England.

We wish that some good botanist—our author, if he has leisure—would publish a manual of the trees and shrubs growing in England, including a few hundred of the commonest exotics; giving, of course, not merely the dry definition of the genus or species, but the differential characters by which they may be distinguished when not in flower, with woodcuts of the leaves, &c. Such a work could not fail of being popular. There is, indeed, an *Arboretum et Fruticetum Britannicum*, in eight octavo volumes; but, alas! the price of the book is 10*l.*; and who can raise such a sum in these hard times? Dr. Macreight's Manual is well executed, and will be extremely serviceable to the student in the shrubbery and the garden, as well as in the field and on the mountain.

Palmer's Edition of Hunter. In 4 vols illustrated by a volume of plates, in 4to. Vol. iii. 1837.

IN this volume we arrive at the citadel of Hunter's mind. Several years ago we carefully analysed this work ourselves, and, with the help of an active imagination, chiselled out the fancied scheme of his intentions. Larger reading and later experience have made us doubt our own imaginings, and brought us to a closer, perhaps to the real, view of

this stupendous production. More is not to be attributed to Hunter than is his due: and his meed and merit lie in having investigated the arterial branches of that tree of which Harvey had laboured at the root. Beyond these arterial branches Hunter did not proceed; and, in observing, noting, and explaining, the manifold operations of these laborious vessels, he arrived at the true doctrine of the adhesive inflammation. But no one will affirm, that the adhesive inflammation with all its products is the whole of pathology; neither can the incidental observations and generalizations distributed throughout the work, however valuable in themselves, be received in any other light than that of rich jewels appended to the casket. The adhesive inflammation is the soul of this volume, and beyond the adhesive inflammation it does not proceed. The Hunterian school build upon the adhesive inflammation; and there requires another spirit to arise and open our vision to other operations of life, and to reveal the still deeper truth that disease has its manifestation as much in the nervous as in the arterial system. Many of the inflammations have their rise in diminished vitality; and many organic diseases, especially strumous deposits in the glandular structures grow, and proceed in an inverse ratio to the column of blood, and the apparent quantities of vital force. Here is a field for observation, which even the telescopic eye of Hunter had not reached.

We must be permitted to speak a few words more of the *author* before we proceed to the *editor*. Hunter's view was so especially comprised within the range of the vegetative or organic functions—the formation of blood, and its distribution throughout the body—that he had not time to look up to the particular functions of the brain as the nervous centre, nor even to glance at the wide relations of the ganglionic system. This defect (a defect not supplied by other investigators of his day) forms a large hiatus in the Treatise on Gun-shot Wounds, and an impassable vacuity to a comprehensive physiology of disease. Dissectors and observers of a late date have grown up by the side of Hunter, and carry arms, on this point, of greater weight and metal than those of the giant.

With this intimate knowledge of the Treatise on Blood, Inflammation, &c. we were anxious to see in what manner it had been edited by Mr. Palmer. A very little obliquity of judgment would have spoiled the whole, and marred the text of the author. No evil can befal the writings of a great man worse than that of an imprudent editor. He is made responsible for another man's errors; and theories are woven out of the text which the unlucky author himself might have apprehended was too plain and too stubborn for any thing—except one interpretation. In the present instance the most friendly commentator might have been mistaken; glimmerings of thought might have been struck out into unexpected assertions, and remote views of life (the very error of Hunter) dragged forward into the light of a favourite theory, while the public could have been easily appealed to on the ground of the *argumentum ad verecundiam*. The edifice which Hunter had raised might have been mutilated by an officious hand.

The chance of this disaster is now removed. Mr. Palmer has shewn himself to be intelligent, discreet, and perspicuous. The text of the author is carefully pointed; and the notes at the foot of the page appear at the very moment when the reader expects them, and contain all that the reader expects to find. With the utmost propriety Mr. Palmer never obtrudes himself on our notice; and while he points to the deficiencies or errors of the work which he edits, he explains, supplies, or corrects it, without allowing himself to be seen. Without lifting our eye from the page, we feel that we are reading a difficult book under the surveillance of a skilful preceptor. In Mr. Palmer, the writings of John Hunter have received the inestimable blessing of a prudent friend.

The doctrine of a "vital principle" is the distinguishing feature of all Hunter's writings; it is the *point d'appui* of his intellect. In itself it is highly philosophic and correct. But Hunter was not a logician; for instead of inducting the primal cause from the manifestation of its phenomena, he assumes it once for all as an axiom; whereas in his mind it is nothing more than a mere *petitio principii*. A clear judgment and a steady sight were here requisite to guide the untutored through the mazes of an engaging hypothesis. It is in notes to passages of this kind that Mr. Palmer shines, and exhibits an

understanding prepared to meet the ambiguities of the text, and cultivated so as to disentangle the truth from the semblance of truth, in the midst of words and phrases. We would refer to notes at the foot of pp. 277, 273, 422, each of which is short, and cleverly seizes at the point and meaning of the author. The practical notes are not less worthy of notice. See pp. 261, 241, 243, 374, 387. The last note contains a summary of the treatment of inflammation. We would suggest a word of caution as to carrying out the principle of the antiphlogistic treatment to the utmost. It is the business of the surgeon to understand and act upon the subduing of inflammation: the success of his practice turns upon it. But the physician becomes less exact and severe; for he tempers the unrelenting practice of the antiphlogistic school with a due regard to the powers of life and the paramount process of sanguification. Surgical and idiopathic inflammation do not rest upon the same base; and the pure surgeon halts at a station much less elevated than that to which the practical physician eventually ascends. The concluding paragraph of the note at the foot of p. 277 ought to be printed in Roman capitals for the admonition of "*those who build the knowledge of their profession too exclusively on the investigation of diseased appearances, and on mere anatomical and physiological acquirements.*" They forget that they they are to reason from the bed-side down to the dead-house, not from the dead-house back to life. This is the prevailing error of the day. Pathology, without previous clinical observation, is a solecism in medicine.

One of the most interesting, and certainly best executed, portions of Hunter's Treatise has always appeared to us to be Chap. IX.—Effects of Inflammation, and its Consequences on the Constitution—namely, Hectic Fever and Dissolution. This short essay is a clear draft from bed-side practice, drawn by a mind intimate with its subject, and filled with the great importance of thoroughly understanding it. Like all subjects fully understood, it is condensed and concise. We have perused it again and again, nor are we aware that any thing can be added to or taken from it. With just discernment, the editor has not intruded a note, a line, or a word, on the attention of the reader.

They who watch at the bed-side must suffer some anxiety when they behold a patient droop, fade, wither, and perish, without any assignable cause. Hectic is usually imputed to local organic disease: in general, it is the sign of the disorganizing process having been set up. But Hunter makes a remark, which could be made only on the strength of actual experience—that "it is possible for it to be an original disease in the constitution;" that "the constitution may fall into the same mode of action, without any local cause whatever, at least that we know of." This remark accords with our own experience. We have seen the constitution disturbed to the very centre, by some accidental shock of mind or body, and the diurnal functions of the heart and nervous system slowly deranged, then accelerated, and finally thrown into the periodic accession of nightly fever and matutinal perspiration, uncontrolled by the intervention of medicine, and certainly precipitated towards a fatal goal. Minute dissection after death might detect some local disease; but the quantity of local disease during life is inapplicable to the tact and sagacity of a skilful physician, while the total derangement of the functions of life, *en masse*, is alone discernible. In some constitutions, a very trivial accident is the exciting cause of shivering and fever. An inflamed corn, for instance, or a slightly punctured wound. Females, and men whose nervous system seems to partake of the feminine construction, are the usual instances of this prompt susceptibility. But in Sir A. Cooper's Lectures, by Tyrrell, vol. i. 1824, p. 17, there is a case of fatal irritation, which ensued from a small wound in a strong man; and the following pages (18—24) of the same work strongly support the present observations. There are likewise several apposite instances to be found in Mr. Travers's work on Constitutional Irritation, (Part I.) But there are other causes of more note which act upon the total functions of life, and slowly reduce a patient to a very precarious condition of health. Among the first and foremost of these are those depressing causes which act directly on the nervous centres. Profound cerebral exhaustion, and the imperfect arterial reaction consequent thereon, together with defective nutrition, arising from loss of appetite

through superinduced indigestion, become, not a mechanical or organic, but a vital cause, of hectic, of a character by no means trivial.

At p. 479, (note *a*) Mr. Palmer expresses his doubts as to the existence of any special law determining the course of abscesses towards the surface of the body. The experiments of Hunter, by which he meant to ascertain a final cause in the progress of foreign bodies towards the surface, is by no means conclusive (p. 287, note *a*); yet, though "there seems no ground for the analogy in the text," it is a very well-established fact, that when reeds are left at rest naturally in the soil, the root has a centripetal tendency, the stem a centrifugal: and to our apprehension, just conclusions are to be drawn from natural objects in their natural positions, and not from those unnatural conditions as when a seed is planted in a revolving tub. However, Mr. Palmer is inclined to regard the approach of an abscess to the skin as depending on the greater extensibility of the walls of an abscess on the side next the surface. His reasoning is intelligible, well explained by the diagrams, and stated with clearness. Now, from whatever final causes it may happen to arise, whether from a stimulus of absorption having a vital direction towards the skin, or from the mechanical resistance being less on the side next the surface of the body, we find, what we confidently expect to meet with, that both abscesses point, and foreign bodies advance from the centre to the circumference, and that on these points our confident expectation is seldom, if ever, disappointed. To us it appears, that, in this direction from within outwards, there is a vital law at work—a law essential to the safety of the animal; and though this law may be, in particular instances, suspended or interrupted by the intervention of mechanical impediments, yet it is practically the universal and prevailing law of the animal economy. Hunter himself is open to frequent correction: his opinions are upon the main right, but his views are so often bent towards his remote principle of life, that he overlooks the modifying circumstances of the case which lie palpably on his right hand and on his left. This exclusiveness of vision is not Hunter's fault alone: we all of us see too little, and the objects upon which we fix our attention are too transitory to allow of our examining them, not merely in detail, but in all their proximate relations. Thus Hunter attributes too much to the blood. In the process of adhesion, he ascribes to the blood those operations which belong exclusively to secerning vessels. The blood itself is not the bond of union. If a clot of blood be left interposed between the flaps of an amputated stump, secondary hæmorrhage, irritative fever, suppuration, and perhaps sinuses, are the consequences; whereas, if the stump be sponged quite clean, and left exposed for a time, until each vessel beyond the reach of a ligature has fully retracted, and all hæmorrhage has entirely ceased, then it will be observed that on its surface dew-drops of lymph begin to exude, and stand out as the forerunners and guarantees of the adhesive process being about to begin. Now this process does not begin from the blood; it is from minute vessels fitted for this peculiar office.

We can only make a few more references. One note, containing a condensed view of Hunter's opinion of *life in general*, is well condensed and well guarded from error; and the same may be said of a subsequent note on the *life of the blood*.

In this manner the editor has elucidated the chief work of his author, and fully discharged his duty to the reader. It was necessary to bring to such a task much reading and a thorough acquaintance with the discoveries of modern science. This part of his undertaking has been performed in the copious bibliography appended to the several sections of the work; and if a knowledge of what others have said and done on the same subject constitute a gentleman of learning, Mr. Palmer merits that title. The student, the scholar, and the practitioner, will find this volume a useful compendium of all that is known on the subject; a book which heretofore demanded close perusal, and even retirement, for its comprehension, is now laid open by a skilful hand, and interpreted to the reader as he proceeds through its pages. We would encourage among the rising men of the day the same spirit as that which breathed in Hunter; we would exhort them to study nature alone, and to understand disease, not as a mere alphabet of symptoms, but in its departure from the phy-

siology of health, and its visible development of the laws of life. Let them accept his Treatise, thus edited, as a lamp which may throw some light on the obscurity that still overhangs the functions of the living body.

MEDICAL GAZETTE.

Saturday, July 14, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

PLANS AND PROSPECTS OF THE LONDON UNIVERSITY.

EVERY day seems to make it more probable that the failure of the medical department of the London University will be complete. The Chairman, and several of its most influential members, have resigned, in disgust. The greatest confusion and alarm prevail, lest the labours, which amidst much interruption and turmoil, have been at last nearly completed, should now be marred just when it was anticipated that they would be made public.

For our own part, we believe that nothing more advantageous could occur than a complete *bouleversement* of the whole scheme of medical education which they have planned; for were it now carried into operation, it is not probable that there would be more than two or three candidates in each year who would deem it worth while to obtain their diploma on the terms at which it is intended to be offered, and which we pronounce to be more tedious and vexatious than those demanded by any University or College at present in existence. The proposed medical curriculum, though not yet actually published, has been pretty extensively circulated amongst the profession, and the greater part of its requirements are now generally known. In the first place, before a student can commence his medical studies, he must be a B. A. of this University, or have passed an examination in classics, mathematics, and general literature, equivalent to that for the degree in question. He will, therefore, be required to begin his education on nearly the same terms as those who take their diplomas at Cambridge and Oxford; and if the examinations in Arts be at all fairly conducted, and extend to all the subjects mentioned, no pupils will be able to enter upon their education for the new diploma, except those of a class equal to the present graduates of the older Universities. It would seem necessary, therefore, to ensure the success of this establishment, that some facilities should be offered to those who may commence their medical education for its diploma, greater than those which exist in the establishments to which it hopes to become a rival. But on the contrary, the difficulties of the education are far greater—the curriculum is loaded with compulsory courses of lectures on every subject that can be imagined to have even a distant connexion with medicine; or rather for which there exists any person connected with the University on whom it would be convenient or desirable to bestow a professorship. Will it be believed that there are demanded attendances upon separate courses on the Theory and Practice of Medicine, on General Pathology, and on General Therapeutics, each of which courses is to contain 100 lectures, and 20 examinations? Why hitherto the difficulty has been to lengthen out one of these courses to that length; and to do it at all, it has been necessary to draw in subjects from every quarter, and to poach on every adjacent manor. Now what may be meant by General Therapeutics? Does any member of the Council mean to say that he knows any general modes of operation of medi-

cines? or that he can with certainty state the exact action of any medicine in the Pharmacopœia? A course of lectures on Therapeutics, distinct from Materia Medica and the Practice of Medicine, can only be given by going over the dryest and dullest details of the most foolish and fanciful theories of the schools, which, it was to be hoped, had long since been utterly forgotten. We should have the *strictum* and *laxum*, the spasm and atony, the moving fibre and the nervous influence, again in all their vigorous nonsense; and if the London University ever confer degrees, it will be at the expence of degrading medicine for fifty years, by turning it again from the practical and philosophical direction which it has begun to assume, into a mysterious art of words and hypotheses. On the subject of General Pathology, as a separate course, we have already expressed our opinion, and proved, as we humbly think, its utter absurdity—unless for fulfilling its real intention, namely, that of bestowing a Professorship on a gentleman connected with some members of the University. Besides these courses, and all the old ones demanded by the Society of Apothecaries, there are also compulsory courses on Comparative Anatomy, Morbid Anatomy, Practical Chemistry, and Practical Pharmacy; and not only are they all compulsory, but the very order in which they are to be attended is irrevocably fixed. The courses are regularly arranged in the four winter sessions and the four summer sessions, during which the pupil is required to work, before, whether fit or not, he can endeavour to obtain his first diploma. Nay, more than this, his attendance on the lectures is to be noted, and his name to be marked as absent or present on each day; and the names of the absentees at the class examinations, which are to be held once a week, are also to be accurately recorded. Really, when we read all this, we could scarcely believe that the great principle upon which this University set out was, that men should not be compelled to tedious and expensive courses of education, but should have their diplomas conferred upon them for their knowledge, wherever obtained, in order that the industrious poor might be on terms of equality with the idle but opulent student. We have never argued in favour of making the examination the sole test of a man's fitness for practice, or for the honour of a diploma; but the scheme of this University surpasses all that was ever before heard or thought of in the demands for education which it makes and threatens to enforce. Four or five hours a day must be regularly employed in attendance upon lectures; as many more ought to be given to dissections and hospital practice; and then what time is left for the pupil's own thoughts, or for digesting either his learning or his dinner? The four years of what they are pleased to call education, will be just one confused whirl of lectures, in which the pupil will learn nothing, and will forget how to study any thing.

The system of medical education is principally and essentially theoretical; but there is some practice required to be scattered through it. The pupil *must* frequently attend clinical lectures, and the clinical lecturer *must* see his patients every day, and *must* have notes taken of all the cases, and *must* keep two clinical clerks, and *must* have a case-book which *must* be open to the public all day; and so on with so many compulsions, that we expected they would next say what epidemics *must* prevail, and what cases *must* be always present in the wards. All this farrago of hospital education shews clearly that the framers of it have no idea either of hospital practice or instruction. As if a pupil, by breathing the atmosphere of a

ward, and hearing a clinical lecture, *must* of necessity learn the nature and treatment of diseases! This is the fundamental error which they have committed. They imagine that education, which they conceive to be the teaching of separate and abstract facts, is all that a man requires to make him fit for practice, forgetting that the best education is not that which teaches a man what others know, but that which enables him to learn for himself. In no profession is this so necessary as in medicine. Every man must acquire it for and by himself, or he can never apply it in his own practice: it cannot be taught like a certain science, for it has scarcely a fixed rule; but to practise it, a man must learn it by experience; and to study it, further than for mere routine practice, he should not be crammed with technicalities, but be imbued with those general principles which are the foundation of all knowledge, and those rules of philosophy which are necessary to guide his investigations in whatever direction he may proceed. If the plan of education now proposed be actually carried into effect, the graduates of the University, if indeed there should ever be any, will be chiefly remarkable as possessing a smattering of every thing, and sound knowledge of nothing. The main design has been to increase the number of subjects for study to the greatest extent: the consequence *must* (to use their own term) be, that the knowledge of each will be the least.

In short, the result of the whole scheme of the London University has been hitherto a failure: ask whom you will, and he will say that in his department, at least, they have committed the greatest possible errors. To quote, for example, but one instance: they require for the masters of arts, (as a branch, probably, of general literature), that graduates are to be acquainted with animal and vegetable physiology, in return, we presume, for the great amount of "*literæ humaniores*," which the doctors in medicine are to possess, and which is to contain moral philosophy, natural history, "*et omnes res cum quibusdem aliis.*" It is lamentable to think, that with such full powers given them, and with all the experience of preceding institutions before them — commencing, too, as they did, with the professed object of establishing a University which, avoiding the errors of its predecessors, should amalgamate all the desiderata, with all their virtues— to think that they should have now completed a plan which avoids nearly every virtue and amalgamates every defect. With the exception of the clinical examinations, and the preliminary examination which they require to be passed after the second year's study, we look in vain for an improvement on former systems. But, on the other hand, they have all the multiplicity of useless and theoretical courses which have been the error of the Scotch Universities—the crowd of unnecessary lectures of the German—the want of practical study of the French— the long system of previous education of the English Universities. They have the compulsory attendance on fixed courses which the very same parties have decried in all other institutions; and they have all the requirements of the unnecessary and ill-studied subjects of the Apothecaries' Hall. It would be difficult to find an error which they have not adopted and increased, or an excellence which they have not contemned.

ANIMAL MAGNETISM.

We beg to direct attention to the paper by Mr. Blake, at page 644 of our present number. It points out extremely well some of the sources of fallacy with reference to certain of Dr. Elliotson's recent experiments.

"STRIKE" IN THE LONDON UNIVERSITY.

We have before alluded to the circumstance more immediately productive of the stop which has been put to all business in the medical department of the London University; it has consisted simply in the nomination of Dr. James Somerville to a seat in the Senate. His appointment was made by Lord John Russell, at the instigation of Mr. Warburton, without any communication either with Lord Burlington—the Chancellor of the University, or with other members of the Senate; and this proceeding, which, under any circumstances, could not fail to be regarded as very arbitrary and ungracious, was looked upon as still more extraordinary, because, when Mr. Warburton, some time ago, endeavoured to place his nominee in the subordinate situation of registrar, the proposal was met with an opposition so general and determined, on the part of his colleagues, as to compel him, however reluctantly, to abandon the attempt. The nomination, however, to a seat in the Senate, was subsequently made, as we have said, unknown even to Lord Burlington; and the consequence has been, that it has not been found possible to assemble a quorum of the Medical Committee since the 24th of April last. Some gentlemen have resigned their seats as members of the Faculty, and others have withdrawn from the University altogether. The Committee has since been dissolved, and letters have been addressed to the *ci-devant* members, inquiring whether they will consent to act if re-elected. We understand that answers have been already received from ten of them, to the effect that the same circumstance which has recently prevented them from attending, would, if it continued, still equally induce them to absent themselves even if re-elected. An excellent letter was sent to Lord Burlington by Dr. Roget some weeks ago, and a most spirited remonstrance was addressed to the same nobleman last week by Dr. Locock. Among the under plots in this drama, we may mention that legal proceedings have been threatened by Mr. Warburton, in the name of his *protegé*, against this latter gentleman, who has, in consequence, withdrawn his resignation for a time, to give them an opportunity of bringing their action. Verily, Lord John Russell and Mr. Spring Rice have cause to be proud of their handiwork.

COLLEGE OF PHYSICIANS—NEW FELLOWS.

Our readers we presume are aware that every physician now joining the London College becomes a Licentiate, wherever he may have been educated; the preference formerly given to the graduates of the English Universities having been withdrawn. From among the Licentiates some are annually chosen to be Fellows, and the week before last the following gentlemen were elected:—Dr. Clutterbuck, Dr. Back, Dr. Arnott, Dr. James Blundell, Sir James Clark, Dr. Theodore Gordon, Dr. Tweedie, Dr. Addison, Dr. Burne, and Dr. Thos. Davies. The five last on the list have already accepted the honour; Sir James Clark, Dr. Arnott, and Dr. Clutterbuck, have declined; the decision of the others is unknown to us. The three whom we have mentioned as having refused the Fellowship could not, we think, with any consistency have accepted it. But the College does not manage these things well. Why were only two Fellows elected last year? Are the gentlemen whose names are above given more fit to be admitted now than they were twelve months ago? If two years since, when the new method of election (or selection) was adopted, some fifty or sixty of the most respectable Licentiates had been invited to become Fellows, it would have given general satisfaction,

and they who have since refused through pique would then have gone to Pall Mall cap in hand, and been grateful for the compliment. There are also some who are evidently excluded from personal or political motives: why is not Dr. Birkbeck invited to join the College—why not Dr. James Johnson—why not Dr. Gregory—why not various others? We do not think that physicians of such long standing as those we have named ought to be passed over, and we certainly acknowledge that, when at length they come to be offered the Fellowship, they may well hesitate to accept the tardy honour.

As it is, however, a considerable addition of liberal members has recently been made to the College, and we earnestly call upon them to attend in their places, and give their support—not as levellers or destructives—but as intelligent reformers, to every measure calculated to improve, not their own department merely, but the medical profession as a whole.

DELAY OF PROMISED PUBLICATIONS.

To the Editor of the Medical Gazette.

SIR,

I THINK the subscribers to the Cyclopædia of Anatomy and Physiology have great reason to complain of the manner in which they are treated. After sundry delays in the publication of the work, up to the appearance of the last part, we have not been favoured with another since February, although, on the cover of that one, the next was announced for March; afterwards it was promised "shortly;" and finally, in the advertising sheet of Copland's Dictionary, a fortnight ago, we were assured it would be forthcoming the 1st of July, whilst arrangements were made to complete the work by October 1839. I have reason to know that the fault does not rest with Dr. Todd, as he has material for two or three parts quite ready; and I am satisfied the publishers are acting against their own interests, as well as unjustly towards the subscribers, by these vexatious delays.

Another edition of Cooper's Surgical Dictionary was promised a long time ago. What has become of it? As for the second part of vol. ii. of Arnott's Natural Philosophy, I despair of ever seeing it. Costello's Cyclopædia of Surgery is another instance of little less than imposition on the medical public. Who will subscribe to such works again, if they are to be so uncertain in their progress and issue?

Your constant reader,

CHIRURGICUS.

London, July 5, 1838.

BOOKS RECEIVED FOR REVIEW.

First Principles of Medicine. By Archibald Billing, M.D., A.M., &c. Third edition, considerably enlarged and improved.

Flora Medica; a Botanical Account of all the more important Plants used in Medicine, in different parts of the World. By John Lindley, Ph.D. FRS. Professor of Botany in University College, London; Vice Secretary of the Horticultural Society, &c. &c.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *July* 10, 1838.

Abscess	3	Hernia	1
Age and Debility	17	Hooping Cough	3
Apoplexy	6	Inflammation	5
Asthma	7	Brain	3
Cancer	1	Lungs and Pleura	1
Childbirth	4	Insanity	3
Consumption	39	Measles	1
Convulsions	20	Paralysis	2
Dentition or Teething	3	Small-pox	17
Dropsy	3	Stone & Gravel	1
Dropsy in the Brain	3	Thrush	2
Erysipelas	2	Unknown Causes	98
Fever	14		
Fever, Scarlet	5	Casualties	5
Fever, Typhus	2		

Decrease of Burials, as compared with the preceding week 31

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N Longitude* 0° 3′ 51″ W. *of Greenwich.*

July.	THERMOMETER.		BAROMETER.	
Thursday . 5	from 49	to 75	30·09	to. Stat.
Friday. . . 6	57	75	29·98	29·92
Saturday . 7	54	69	29 92	29·91
Sunday . . 8	54	71	29·88	29·98
Monday . . 9	57	76	30·06	30·08
Tuesday . 10	56	75	30 09	30·10
Wednesday 11	56	78	30 06	30.01

Wind, S.W.

Except the mornings of the 6th and 8th, generally clear; rain on the 6th and following day.

A very violent storm of thunder and vivid lightning, accompanied with heavy rain, on the morning of the 6th.

Rain fallen, ·3375 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 21, 1838.

THE CROONIAN LECTURES,

For 1838 (*continued*).

By John Clendinning, M.D., F.R.C.P.

EXPERIMENTS AND OBSERVATIONS RELATING TO THE PATHOLOGY AND PATHOLOGICAL RELATIONS OF THE HEART.

STATISTICS OF MORBUS CORDIS.

Distribution or frequency, according to sex and age.—The first topic that offers itself for observation under this head, is the relative frequency of Morbus Cordis, as compared with other grave diseases of a character sufficiently defined for numerical comparison.

As already hinted, the facts and figures laid before the College in the foregoing observations, are some of the principal results of above 500 autopsies made within a limited period. Of these, about half were cases included in the class *Varia*, or diseases of various classes and kinds, exclusive of pulmonary consumption and disease of the heart. Of the remainder, between 70 and 80 above puberty were cases of consumption, of which two-thirds were males above puberty; and the rest were cases of disease of the heart, of which likewise two-thirds nearly were adult males. The number, then, of cases in which the heart was diseased, occurring in a total of 520 to 530 inspections, was 170—80, or about 33 per cent.; and if we deduct from the total number of autopsies the cases of persons under puberty or 15 years of age, amongst whom but two or three cases of diseased heart were observed, we shall then have as the ratio of disease of the heart to the whole number, about 35 per cent. Unquestionably such a ratio is enormous; exceeding by far the calculation of those most disposed to estimate highly the mortality attributable to the heart; yet I have myself but little doubt that, with some explanation, the following table (which, it will be observed, understates the case) will be found less extravagant than it may perhaps at first appear:—

Table deduced from Table 1, *(Table of Hearts) to shew the Ratio of Phthisis and Morbus Cordis at each Interval of Age, to the whole of the Cases included in each Interval of Age.**

	MALES.				FEMALES.			
	Phthisis.	Morb. Cord.	Ph.	M. Cor.	Phthisis.	Morb. Cord.	Ph.	M. Cor.
	Cases.	*Cases.*	*Per Centage.*		*Cases.*	*Cases.*	*Per Centage.*	
15 to 30	11 in 24	5 in 24	45	21	8 in 33	5 in 33	24	15
30 to 50	19 in 67	24 in 67	28	36	8 in 47	8 in 47	17	17
50 to 70	17 in 84	34 in 84	20	40·5	9 in 55	16 in 55	16·4	29
70 to 100	3 in 31	13 in 31	10	42	2 in 39	13 in 39	5	33·33

The principal explanatory remark I think it necessary to offer, is this—viz., my principal field of observation receives all poor applicants from a certain district, provided only their complaints are severe, making no distinction as to sex, age, or disease, except small-pox, nor between

* The Heart table contains no case of valvular disease, nor more than 4 5ths of the simple hypertrophies.

curable and incurable cases, and in general entertaining all the latter until the end of life, unless the sufferers voluntarily withdraw. The effect of this system of admission is, that, compared with other asyla of sickness, the parochial infirmary, so governed, has, according to well-known laws of mortality, an annual loss of life much exceeding that of county infirmaries and city hospitals — a mortality, it is to be remembered, that is augmented very considerably by accessions of cases dismissed from other charities as incurable. For the effect of this comparatively indiscriminate admission, and unlimited retention of patients, of course a large allowance must be made. What the exact amount of that allowance should be, I am not prepared to say; but if it be assumed at the immense proportion of 50 per cent. of the fatal adult cases, which I am quite sure is much over the mark, we shall still have a ratio of heart disease for which probably few of my hearers will be prepared.

Ratio of phthisis to other diseases, according to authors.—The extravagance of the ratio will probably appear still greater when we compare the amount of cases of phthisis with that of morbus cordis. Phthisis, or tubercular pulmonary disease, is, and has long been, held by pathologists and medical statists to be by far the more frequent of the fatal diseases of these and all other temperate climes. The accomplished Dr. Young, writing in 1815, attributed to phthisis, as Heberden, Woolcombe, Wells, &c. had previously done, full 25 per cent. of the whole mortality of England. He begins his remarkable work on Consumptive Diseases thus:—"Consumption is, in almost all civilized countries, the most extensively and inevitably fatal of diseases." And elsewhere (p. 41) he says:—"The frequency of consumption in Great Britain is usually such that it carries off about one-fourth of its inhabitants. At Paris the mortality by consumption has been estimated at one-fifth, and at Vienna it is said to be one-sixth of the whole. But the mortalities at Paris, and in the south of France, from consumption, have frequently amounted to one-fourth of the whole." And seven years previously to the publication of Dr. Young's work, Dr. Woolcombe, of Plymouth, calculated that the annual mortality in England from consumption alone amounted then to 55,800 persons*. And Sir James Clark's estimate is apparently higher than Dr. Young's, being for the 121 years ending with 1821, as follows:—

For the year 1700	the deaths from consumption were to all the deaths of the year	·145
1700 to 1750	.....	·214
1750 — 1801	.....	·263
1801 — 1811	.....	·288
1811 — 1821	.....	·316
	General average for the 121 years	·245

or 1 in 4; and he adds, "It now appears (1835) to constitute one-third of the whole mortality."

The difference between my own conclusions and those of the distinguished authors just referred to—conclusions in which they are supported by the principal recent writers on the subject, as Benoiston de Chateauneuf, and Lombard, &c. will seem the more wide and irreconcileable for this reason: Drs. Woolcombe and Young, as is well known, wrote at a time when the name of phthisis or pulmonary consumption was extended to other pulmonary affections besides that to which it has been limited by Sir J. Clark, Dr. Lombard, and other writers who have published since the appearance of Laennec's great work; so that if, before 1819, tubercular consumption was the cause of but part of the 20 per cent. of popular mortality, attributed justly enough to chronic pectoral, and mostly pulmonary disorder, then, on that supposition, the proportion of deaths referrible to pulmonary consumption in the old and looser sense is now much increased, since the present mortality from true phthisis, in the stricter sense of Laennec, is held by the high authorities above named to amount to one in five, or thereabouts, being the same per centage of mortality as was previously attributed to several chronic pectoral affections, of which true phthisis constituted but one, though no doubt a principal species.

Now the doctrine maintained by Young is true, although, for practical purposes, perhaps, it is obsolete. In addition to the facts advanced by that very learned writer, in support of his opinion, others concur in the same conclusion; amongst the rest this hitherto unpublished observation.

Between May 1821 and May 1835, there were recorded in the journals of the Marylebone Infirmary, under twenty-six principal heads of disease, nearly 4000 (3990) deaths, viz.:—

* Remarks, &c. p. 73.

1. Encephalic diseases, amounting together to 489:—

Apoplexy	182
Paralysis	165
Convulsions	50
Phrenitic cases	71
Tetanus	5
Epilepsy	16

2. Pectoral diseases, amounting to about 2300, viz.:—

Phthisis		991
Asthma	733	1115
Morbus cordis, dropsy, and hydrothorax	382	1115
Inflammation of lungs and pleura		208

3. Abdominal diseases, 732, viz.:—

Cholera	102
Inflammation of bowels	65
Chronic disease of liver	59
——— stomach	16
——— kidneys and bladder	29
Dysentery	204
Tabes (infantum)	257
4. Pyrexiæ — Continued fevers	237
Exanthemata	70
Erysipelas	43
5. Scrofula (42), and old age (57); together	99
	3990

In addition to the preceding there were about 400 other deaths from minor sources of mortality, making with the former nearly 4400 deaths in the period. Now of the total 4400, as already stated, nearly 1000 (viz. 542 males, and 449 females) were deaths from phthisis, understood in the larger sense of the word, as used by Dr. Young,) which gives a per centage of 22½th on the whole mortality of the fourteen years. This observation fully bears out Dr. Young's estimate, as well as those of Drs. Wells, Woolcombe, and Heberden, and other English estimates anterior to 1819.

Such estimates questionable.—But the limitation of the term Phthisis, or P'thisis, by Laennec, to tubercular phthisis, on account of its superior fatality and more extensive distribution, as compared with other chronic and strictly pulmonary disease, while it has narrowed the field and rendered more precise the objects, has, at the same time, made more difficult the means of inquiry. And at the present time it is difficult to place confidence in any results not obtained by diagnosticians of unusual skill during life, or by careful post-mortem examinations by practised pathologists. So that admitting the general correctness of Woolcombe, Young, &c., I am much less disposed to coincide in the views of the medical statists that have written subsequently to the general promulgation of the discoveries of Avenbrugger and Laennec, in the great work of the latter. There is much reason, in truth, (and with the highest respect for the very able and distinguished writers above named, I say it,) to suspect the data on which their calculations were founded. How small a proportion, in fact, of the mortality included in those estimates has arisen from disorders proved strictly phthisical or even pulmonary, by competent diagnosticians during life, or by proper inspection post-mortem! For my part, I have no doubt at all that the greater part of the so-called pulmonary consumptions in persons above thirty years of age, have been in reality mere chronic catarrh, complicated with hypertrophy of the heart and air tubes. But whatever may be thought on that point as a general position, it is very certain that the proportion of morbus cordis included in the 520, and odd cases so often referred to, much exceeds that of phthisis. The cases of heart disease amounted to 170, to speak in round numbers, and those of phthisis to less than half, or about 80. Now this difference has not been caused by any selection at admission, or at the time of post-mortem inspection. Every proper case was admitted on application; and every case that proved fatal, and for which permission could be obtained, was examined without distinction of disease. That it was not mere accident, seems probable from the fact, that in the case of either sex the perponderance was in favour of heart disease. In the case of the males the ratio of phthisis to morbus cordis was as two to three; and in that of the females it was as nearly six to seven. I confess it seems to me more probable that the difference between the generally received proportion of disease of the heart to other diseases, especially phthisis, and that obtained by myself, is owing to this, viz. that on the one side the diagnosis has been, in a large proportion of cases, conjectural during life; while, after death, no sufficient examination has been made; and that on the other side the diagnosis has been always based on instrumental as well as anatomical examination. This, I say, seems more probable, than that the result at which I have arrived, with the aid of unusual facilities and precautions, should be so far wide of the truth as to represent a disease that really amounted to one-fifth of the whole fatal disease of the country, as one half less frequent, even in a single district, than another disease, of supposed comparatively rare occurrence. Whatever, also, it is to be noticed, may be the effect of indiscriminate admission and

protracted residence, it is common to both diseases, so far as my observations are concerned, and cannot be supposed to affect the ratio. So that on the whole, when I recollect the numerous diagnostic errors into which I have myself fallen, in pectoral diseases, and those I have known committed by other practitioners, and call to mind the difficulty in general of accurate diagnosis in the same class of disorders; remembering also the recency of the promulgation of the invaluable mechanical semeiology of Avenbrugger and Laennec, and the confusion not yet sufficiently remedied, of several distinct diseases, commonly classed under the one name of *Consumption*, or *Decay*, and its synonymes, viz.:—1. Chronic Bronchitis, concurring in scrofulous and cachectic subjects, with loss of flesh, and some fever—2. Chronic cough, complicated with, and rendered inveterate, by hypertrophy of the bronchial ramifications and dilatations of the air-cells—3. The same complicated with, and rendered incurable, by morbus cordis, generally on the left side, sometimes on both sides of that organ—4. True phthisis, which is always complicated with bronchitis, and pretty frequently with considerable hypertrophy of the heart—5. Glandular marasmus in children, without in many cases any strictly pulmonic disease;—when I recollect those facts, I cannot avoid feeling very sceptical as to the alleged paramount importance of true phthisis; and suspecting that in male adults at least, the most frequent of all fatal chronic disorders of these islands is disease of the heart. In this, perhaps, rash assertion, I have the satisfaction of finding that I go but one step in advance of one of the ablest practitioners, and largest and least fanciful observers that have especially studied the pathology of the heart. Baron Corvisart affirms, without hesitation, that the most frequent organic diseases, pulmonary consumption excepted, are those of the heart; and that death from cardiac lesion is much less rare, than from lesion of either the brain, the stomach, the liver, the spleen, or the kidneys; or perhaps from all those organs together.

Contrast between morbus cordis and phthisis, in their relations to age, further illustrated.—Before passing on to the next topic, I may be permitted to dwell for an instant on a remarkable contrast between morbus cordis and phthisis, in their relations to age, which is shewn in the first table deduced from the table of Hearts, and which is in some degree curious in itself, but is at any rate pertinent enough to my present argument. If on the male side of the table we compare the distribution of morbus cordis with that of phthisis, we find a striking difference. The per-centage proportion of morbus cordis, to the total number of cases, including phthisis and morbus cordis at each interval of age, is as follows:—For the first, from 15 to 30, it is 21 per cent. For the second it is 36; for the third it is 40½; and for the fourth it is full 42 per cent. Whereas, for phthisis, the distribution changes in an inverse manner; being for the first age 45 per cent.; for the second, 28 only; for the the third, 20; and for the fourth, 10 per cent. only, or 1-4th only of what it was before 30. Then, on the female side, we have results agreeing sufficiently with those just stated on the male to render it probable that there is something more than chance in the matter. On the female side we obtain the following facts:—Morbus cordis gives for the ages 15 to 30, 15 per cent.; from 30 to 50, 17 per cent.; and from 50 to 70, the ratio rises to 29 per cent. and above 70 years; to 33⅓ per cent.; while, on the female as well as on the male side, phthisis seems to decline with age, being, for the first interval of age, 24 per cent.; for the second, a little more than 17 per cent.; and for the third about the same, 16·4; and for the fourth only 5 per cent. Now the conclusion to which these facts lead, viz. the superior prevalence of morbus cordis as compared with true phthisis, at advanced ages, is confirmed by several passages in the classical work of Sir James Clark; especially by statements illustrating the influence of sex and age in the production of phthisis. In Chapter VIII. tables are given, exhibiting the mortality from phthisis above fifteen, in seven cities of Europe and America; which shew that in almost each city there is is a pretty uniform decline in the ratio of deaths from phthisis, from twenty years to extreme age; and the facts furnished by the excepted city—viz. Berlin—are at least a century old, being taken from Süssmilch. In Edinburgh the ratio declines from ·285 at 20 years, to ·052 above 60. At Nottingham, from ·416 to ·017, in the same period of time. At Chester, from ·245 to ·054; at Carlisle, from ·290 to ·097; and at Paris, according to Louis, from ·325 to ·042: while the general average decline was from ·285, or 28·5 per cent. at 20-30, to ·078, or 7·80 per cent. above 60 years of age.

The following unpublished table, deduced some time since by my brother, Dr. G. Clendinning, now not in the profession, from observations registered at the Marylebone Infirmary, confirms the results to which Dr. J. Clark's inquiries have led him.

Of 1044 deaths from phthisis, occurring in the workhouse and infirmary jointly, of the parish of Marylebone, between May 1821 and December 1835, the distribution according to age was as follows;—

Ages.	Deaths.
Under 5 years	70
From 5 to 10	17
„ 10 to 20	53
„ 20 to 30	247
„ 30 to 40	222
„ 40 to 50	164
„ 50 to 60	121
„ 60 to 70	97
„ 70 to 80	45
„ 80 to 90	7
Total	1044

The following table gives the per centage distribution of phthisis, according to the preceding observation :—

	Per Cent.
Under 10 years	$8\frac{1}{2}$
10 to 20	5 nearly.
20 to 30	$23\frac{1}{2}$
30 to 40	$21\frac{1}{2}$
40 to 50	$15\frac{1}{4}$
50 to 60	$11\frac{1}{2}$
60 to 70	$10\frac{2}{3}$
70 to 80	$4\frac{1}{3}$
80 to 90	$\frac{2}{3}$
	100

According, then, to the above, and Sir James Clark's table, the distribution of phthisis according to age is nearly such as I have stated, viz. phthisis declines in frequency soon after puberty, and has become comparatively rare in middle life, when it is for the most part superseded, as I conceive, in frequency and fatality by morbus cordis; and in extreme age, it disappears nearly altogether. It is to be regretted that, with respect to morbus cordis, I am precluded from producing a similar confirmation of my results, partly owing to the frequent exclusion of aged people from hospitals, and partly owing to the neglect of instrumental means of post-mortem diagnosis, and the confidence misplaced by pathologists in their manual and visual skill.

Causes of Morbus Cordis.

Influence of sex and age, and valvular defect.—The next topic for observation is the proximate and other causes of morbus cordis. It has been already cursorily stated that, of 170 to 180 observations of this disease in various stages and degrees, little less than five-sixths were cases of simple hypertrophy, and the remaining one-sixth only examples of hypertrophy with valvular disease.

Summary of facts.—In the cases of simple hypertrophy, nearly all had thickening or increase of muscular substance on the left side, principally in the ventricle; and in a large proportion of old subjects, the left ventricle alone was observed to be materially affected. In a considerable proportion of cases, which I have not recorded with sufficient accuracy for counting, there was enlargement on the right side, sometimes hypertrophous obviously, but often it appeared to me but an effect of distension by blood and mechanical dilatation in the final struggles. Of the cases, including unequivocal valvular disease, only three presented that morbid condition on both sides, or one in ten; while the remaining nine-tenths had valvular defect on the left side exclusively. In both classes of cases of hypertrophy, viz. the simple, and the complicated with disease of the valves, there was the same preponderance of males over females, viz. about : : 2 : 1. In both classes of cases there was the same preponderance in the advanced intervals of age over the first interval of from 15 to 30. In both sexes the proportion of cases of hypertrophy, whether complicated or not, increased with the age. On referring to the table of Hearts, it appears that of about eighty cases of simple hypertrophy of males, but five occurred before 30; twenty-four happened between 30 and 50; thirty-four between 50 and 70; and thirteen beyond 70. And on the female side, in a total amounting to forty-three, in the interval 15 to 30, but five happened; in the second, eight cases occurred; and sixteen are noted in the third; and fourteen in the fourth period. And of the complicated cases, but one male and one female were under 30. From these facts it would appear very decidedly that sex and age have most important influences, as predisposing causes of morbus cordis, and that in general, hypertrophy of the heart is, if at all, then but occasionally an effect of valvular disease of that organ. With regard to sex, it seems that the male is more disposed to the disease by one-half than the female; not only to the simple hypertrophous form, but also to the endocarditic or valvular.

Additional proof of increase of cardiac nutrition, &c. with age.—With respect to age, the tendency to augmented nutrition advocated in the previous lecture, when defining the normal heart, and proved by examination of a large number of healthy male and female hearts, is further strengthened by the facts of this section. The increasing intensity, so to speak, of the nisus nutritivus is proved by the constant advance above shewn in the normal weight of the heart with advance of life; and the diseased heart shews likewise the intensity of nutrition to be proportioned to the age. For the male the average for morbus cordis was, for the four intervals of age above 15, as follows:—

14½ ounces was the average of five cases under 30.

17 ounces the average of twenty-four cases under 50.

15 ounces the average of thirty-four cases under 70.

15 ounces the average of thirteen cases above 70.

While for the female labouring under morbus cordis, the following were the numbers:—

12·2 ounces the average for five cases under 30.

12·37 ounces the average for eight cases under 50.

14½ ounces the average for sixteen cases under 70.

12½ ounces the average for fourteen cases above 70.

So that amid the general shrinking from diminished nutrition that attends advanced age, the male heart was heavier and more voluminous over 70 than under 30; and the female heart increased uniformly up to 70, when the degree of hypertrophy considerably exceeded that of any former interval, advancing from 12⅕ ounces to 14·5 ounces between 15 and 70, and not declining after 70 below 12½ ounces.

Influence of valvular disease.—With respect to the influence of valvular disease in particular in the production of hypertrophy, it appears probable, *à priori*, that the law of muscular nutrition that determines increase of volume as a consequence of increased exertion applies to the heart in common with other muscles. The increase of thickness in the left ventricle attending advanced age might be supposed to result from diminished elasticity in the aorta, which is known to acquire normally a certain degree of rigidity through time. The same explanation occurs readily to account for hypertrophy attending aortic aneurism, rigidity, or shrinking of the semilunar valves, &c. But such a view will not account for such effects as the increase of volume and weight so often observed in the hearts of phthisical subjects.

Heart enlarged in phthisis.—In 23 males dead of phthisis, from 30 to 40 years of age, for example, I found the average for the heart to be 10 oz.; and in 20 other cases, between 50 and 70, I found it to be 10⅓ oz.; both averages exceeding considerably the corresponding averages for the healthy heart. Now as there was no mechanical impediment to the action of the left ventricle, which participated fully, and as much as in any other class of cases in the hypertrophy, such a fact requires an entirely different explanation from that referred to above.

No proof of prior disease of valves.—Then, again, it may be asked, what evidence is there that valvular disease is a frequent antecedent, and therefore may be a frequent cause of ventricular hypertrophy? And I know no facts that would justify a confident reply in the affirmative to that question, which I apprehend has been cut through hypothetically, not experimentally untied.

Author's view of the causation, with reasons.—The view of the matter that appears to me at present the most probable, is this:—Hypertrophy results from vital causes exclusively, and not from mechanical ones; and the more usual result of the action of those causes, where sudden and intense, is inflammation; but when moderate and gradual, they rather produce hypertrophy—a condition akin to inflammation, and one that augments enormously the susceptibility of the inflammatory process with which every part is normally endowed. Now to such supposed hypertrophy as the limit beyond which the operation of the vital causes referred to does not ordinarily pass, and to the morbid susceptibility of inflammation in every part too plentifully supplied with nutrient fluids, which such hypertrophy implies, I would refer almost all valvular disease occurring in mature years, especially in males, and such as could not be attributed to obvious causes, as falls, blows, or well-marked cardiac rheumatism, &c. I would thus in great part reverse the order of causation usually received, and attribute much of the valvular disease to inflammation, mainly induced, if rheumatism be excluded, by previous hypertrophy, as a most potent predisposing cause. The facts on which I found this opinion are shortly these:—

1. Most cases, the great majority, in fact, of those of morbus cordis, have no disease of the valves. (I speak now of my own observations only, as the neglect by pathologists of instrumental means of diagnosis has put it out of my power to adopt, with full confidence, their negative conclusions from their examination of subjects dead of heart disease.)

2. No case of valvular disease, with a trifling exception above alluded to (Med. Gaz., p. 504, c. 1), has occurred to me, uncombined with hypertrophy of the parietes, especially in the left ventricle.

3. Hypertrophy and inflammation are but different stages or degrees of the same process of over-nutrition by afflux and stasis of blood.

4. A large proportion, at least one-sixth or one-eighth, of the subjects of morbus cordis, are hurried off by pericarditis with or without endocarditis supervening upon an advanced stage of the disease, and attributable, I conceive, to the disease as its principal predisposing cause at least.

Pathological relations of the enlarged heart to the abdominal viscera.

Opinions of authors.—In the ætiology and pathology of the older writers, the influence of the abdominal viscera over those of the chest was considered very extensive and energetic. Asthma, dyspnœa, angina, syncope, hydro-thorax, chronic cough, and other pectoral diseases, were attributed to affections of the stomach, liver, spleen, &c., and many pathologists still consider that, amongst the ventral organs, several are frequent causes of organic and other grave disorders in the chest, as well as in the other visceral cavities.

Such opinions conjectural.—Now if tangible evidence of the views just referred to be asked for, I am not able to point out where it is to be found. In reply to the question, has any advocate of the abdominal origin of pectoral diseases, published pathological facts sufficiently numerous, various, and pointed in proof, that such morbific influence of the abdominal viscera is real and important, I am obliged to answer that I know of no such collection of facts and proofs. Ingenious explanations, probable opinions, and striking cases, have, I am aware, been published; but the two former have been in my judgment too hypothetical, and the latter too few in number and equivocal in meaning; and on the whole, after considerable attention to the subject, I have had forced on me the conclusion that pathologists must have often mistaken effects for causes, and causes for effects; and, that wanting convenient and accurate tests of cardiac disease, and wanting still more some ready explanation of striking symptoms and morbid appearances, they found themselves necessitated to fix on the liver, spleen, or other abdominal viscera, in the absence of apparent probabilities in some other direction.

But there seems to me to be little in disease to warrant such opinions. Let me ask, how should hypertrophy of the stomach or encephalon produce general cardiac hypertrophy? Or how should hypertrophy of any organ, the lungs not excepted, be capable of causing hypertrophy of the left side of the heart? I have given above facts of various kinds, and of an unequivocal character, as I conceive, in proof that hypertrophy of the heart is accompanied ordinarily by increase of weight and volume in the stomach and brain, as well as in every other viscus. Now assuming the correctness of these statements, how are such facts to be accounted for by abdominal morbic influences? In the physiology of health, the brain has apparently a more immediate and extensive influence over the heart, than any abdominal viscus; and of the abdominal viscera, unquestionably the apparent influence of the stomach exceeds in extent and energy that of the liver, spleen, pancreas, or kidneys; yet with a few exceptions, the brain and stomach have been excluded from the morbid causation by most advocates of the abdominal origin of pectoral diseases, and the whole of the supposed morbific power has been referred to organs inferior in apparent importance, energy, and influence. This might of itself beget suspicions—suspicions too, which when once entertained, must quickly gather strength from further inquiry. Under such circumstances, one consideration will probably be found of great weight with the indifferent inquirer; viz. that the functions of the alleged morbific organs are in some cases wholly unknown, and in almost every case but very imperfectly ascertained.

Abdominal visceral functions too little known.—Of the splenic function, so far as I am aware, nothing is known: of the hepatic function I apprehend very little is known; and beyond the secretion of bile, a fluid of which the uses are yet somewhat problematical, nothing at all is known. Of the kidneys also, our knowledge is very imperfect. In consequence, then, of the obscurity that hangs over the uses of the abdominal viscera, the stomach perhaps excepted, there is in that quarter a large field open for conjectural ætiology, alike incapable of rigorous proof or disproof. The facility of assumption without risk of experimental or other satisfactory refutation that thus is seen to arise from our ignorance respecting the abdominal functions, ought to put the cautious *naturæ minister ac interpres* on his guard, and will, with other considerations, in all probability satisfy many, if not all indifferent inquirers, of the utter inadequacy of any facts that have yet been made public to substantiate a doctrine in itself so little probable as the abdominal origin of organic cardiac diseases.

The visceral enlargements of morbus cordis probably dependent on the cardiac disease, and not causes of it.—If, then, we exclude abdominal visceral influence from the ætiology of morbus cordis, how shall we explain the almost constant connexion between cardiac disease and visceral enlargement in the abdomen as well elsewhere? To this question I see no satisfactory answer but this: that those visceral enlargements are, in some way or another, effects of the disease at the centre of circulation; and the grounds of this conclusion are summarily these:—

1. Morbus cordis is usually found to coincide with enlargement of all, or several of the viscera of the great cavities.

2. Some viscera which, from the nature

of their functions and other reasons, appear less capable of affecting, in an unequivocal manner, the nutrition of the heart, such as the encephalon and stomach, pancreas and kidneys, are nevertheless found to participate equally with others that have more intimate anatomical and physiological relations with the heart, as the liver and lungs, in the general visceral hypertrophy attending morbus cordis.

3. Abdominal visceral hypertrophy is often met with without any disease in the heart. *Ex. Gr.* of thirty and odd instances in the five hundred and odd cases from which my tables have been constructed, more than thirty cases had excess of weight in the liver of from ¼ lb. to 6 lbs. above the normal average, but without any decided abnormal development of the heart, viz. cases of phthisis amounting to nearly one-third of the whole,—cases of puerperal peritonitis amounting to four; as many cases of erysipelas, and of carcinoma uteri, and uterine hæmorrhage, each one case; and ten or twelve other cases. One of the phthisical livers weighed 6½ lbs.; and another enormous liver that occurred in a male, dead of ascites with black or rather dark-green jaundice, weighed 154 oz., or 9 lbs. avoirdupois.

4. Chronic bronchitis, emphysema, and œdema pulmonum, &c., although apparently capable of obstructing the circulation more effectually than any abdominal hypertrophy and enlargement or induration of equal amount, are, when unattended by morbus cordis, usually quite free from abdominal visceral hypertrophy. *Ex. Gr.* of eighteen cases of chronic bronchitis, nearly all the adult cases of that disease that I have met with, in which there was detected no enlargement of the heart, there was in no case any decided hypertrophy of any abdominal viscus, nor of the encephalon.

It is true that in phthisis, as I have stated in the paper in the Transactions of the R. M. C. S. L., I have usually found an enlargement of all the viscera; less considerable, indeed, than in well-marked morbus cordis, but yet sufficiently unequivocal. And this by itself would certainly appear to favour the opinion that morbid conditions of other viscera than the heart, might, if apparently capable of impeding the circulation, be reasonably supposed to produce more or less of the visceral enlargement and hypertrophy usually met with in morbus cordis. But in the same paper I have shewn, or made it probable, in opposition to Louis, and other authorities, that in phthisis the heart usually exceeds the normal dimensions in both sexes; and it is to this circumstance principally that I would at present refer the general visceral hypertrophy, commonly found in subjects dead of phthisis.

SOME REMARKS

UPON THE

DIRECT INFLUENCE OF NERVES UPON ARTERIES,

CHIEFLY IN REFERENCE TO THEIR REGULAR AND IRREGULAR PULSATIONS.

By William Faussett, A.B., L.R.C.S.I.

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It is with regret I find by a paper of Dr. James Johnson's, in the April number of the Medico-Chirurgical Review, that he, Dr. J., appears to have taken offence at some remarks which I lately had occasion to make in the Dublin Journal, upon a critique of one of his reviewers, in which, also, I happened to have felt myself interested. I should, perhaps, be wanting in courtesy towards Dr. Johnson, did I not now assure him, that I never for a moment imagined that *he* could have been the author of that critique, which, as it appears, expressed sentiments entirely at variance with his own. It is true that *caustic*, upon the authority of Dr. J., was supplied to my reviewer "*in minute doses*," as likely to have a "homœopathic effect" upon a writer in the Journal of Practical Medicine; but neither this, nor any other remark of mine, was intended to apply individually to Dr. Johnson. Constituted as the London Medico-Chirurgical Review has been by the talents of its editor, as an influential medium for suppressing error or establishing truth, I felt myself called upon to appeal as I did against what since, in substance, is admitted to have been *unjust* censure; and if the tone of that appeal was marked, as Dr. J. thinks, by "asperity," I can only say, that censure equally gratuitous would have been treated with less "*asperity*," or, it may be, have passed unnoticed, had its vehicle happened to have been a periodical of less character than Dr. Johnson's.

The confirmation which Dr. Johnson's thirty years' experience offers to the theory and treatment of epigastric pulsations which I have ventured to put forth is most gratifying, in coming from the pen of so able an observer, as well as creditable to its author's candour. "From an attention," says this writer, "of more than thirty years to abnormal

abdominal pulsations, I am free to confess that the doctrine which I have adopted on this subject does not differ materially from that of Mr. Faussett; and the same may be said respecting the practice."

The attention of the medical profession was first called to violent pulsation of the aorta at the epigastrium by Dr. Baillie; and from his brief notice on the subject it may be inferred that, at the time he wrote, the complaint was but little understood, as he states the entire ignorance of such men as Dr. William Hunter, Sir C. Hawkins, and Mr. Bromfield, both about its nature and treatment, as well as his own belief that when it once occurred it seldom after subsided—in short, his conviction that it was incurable. Thirteen years have now elapsed since these admissions of Dr. Baillie appeared in print; and having consulted the works of other authors since his time, and those of Dr. Johnson among the number, before I ventured to lay my own views before the profession, I discovered but little light thrown upon the subject in later years, and this little obscured by a multiplication of alleged causes, which, overlooking the effects that in so many cases were very much alike, led to methods of treatment that were ever varying, and often contradictory. Some of these causes have been alluded to by Dr. Johnson, and will be found still more amply enumerated by Dr. Copland, in his excellent Dictionary of Practical Medicine; and that many of them are capable of producing epigastric pulsations I never denied; wherefore it appears unfair to put me, as it were, upon trial for opinions neither entertained nor expressed. Looking to the uniformity of effect, my theory merely went to prove that the mode of causation was also uniform; in other words, that the proximate cause was, in a certain class of cases, generally the same, the remote cause consisting in some derangement of the viscera surrounding the aorta, and operating through the medium of those nerves which, in such a remarkable manner, were described as interlacing the aorta and its branches, and interweaving their filaments in these vessels' coats. The agency, then, of several causes was admitted, while it was attempted to be shewn that the nerves were, under each cause, the centre, and, as it were, medium of morbid action. Again, Dr. Johnson appears to apprehend that the same course of treatment was indiscriminately enjoined for every possible variety of pulsation in the epigastric region; but this is not the fact. A morbid condition was described as exhibiting itself by certain symptoms, the most prominent of which was inordinate pulsation of the aorta at the epigastrium. For this morbid condition—*yet for this only*—a particular plan of treatment no doubt was recommended*. With respect, therefore, to Dr. Johnson's notice of pulsation of the *heart* at the epigastrium, it appears to me that I have at present nothing to do further than as a matter of diagnosis; but that I was quite aware of the importance of attending to the condition of this organ, as well as of the circulation generally, in all cases of epigastric pulsations, may be seen by the paper above alluded to, where, in reference to one of the cases, the action of the heart is described as undisturbed, and the pulse at the wrist perfectly regular†. Generally speaking, a recognition of the double beat at the epigastrium—the auscultatory evidence afforded by the heart as examined *in sitû*—as well as a careful attention to the rational signs, will enable us to distinguish the two affections with some accuracy, and of the importance of keeping them distinct. I am the more convinced from a case having lately presented itself, where leeches, mercurials, and purgatives, had been here freely though injuriously prescribed, under the impression of a wrong diagnosis. Dr. Johnson's sentiments with respect to founding pathological opinions solely upon the therapeutical agency of drugs, and also the susceptibility of the masculine gender for what he terms the "hysterical diathesis," are, I believe, entertained by him in common with most other medical men; at least it was never my intention either to ridicule or gainsay them.

Again, with respect to the dubiousness of epigastric tenderness as a diagnostic sign, having already brought forward this subject, I cannot trespass upon the time of my readers by now particularly referring to it; but when it is permanent, taken in connexion with fulness in the same region, wasting, a sense of vital depression, and

* See Dublin Journal for July 1837, p. 452.

† See pages 448 and 450, Dublin Journal, July 1837.

other symptoms already fully described*, it will, if I mistake not, be found a useful symptom in guiding both the diagnosis and treatment.

I shall now venture to make a few remarks upon the general subject of increased and irregular pulsations in arteries, which, however, is so linked with the natural functions of these vessels and the doctrine of their vital contractility, that it seems impossible to give it a separate consideration. But without intending at present to enter at large into this field of controversy, I shall merely proceed to make such observations as appear more immediately connected with what I have already advanced in a former paper.

From an attentive consideration of that anatomical law by which arteries and nerves from the trunk to the branch are so constantly distributed together, it seems, I think, forced upon our conviction that nerves must possess some secret and inexplicable influence over the functions of arteries, which is either "beyond our ken," or which has never yet been fairly developed. The femoral artery, during the whole of its course, is accompanied by nerves which distribute their branches in intimate relationship with those of the vessels to their remotest ramifications. The brachial artery, from the axilla to the digital extremities, has its branches attended by nerves, and the trunk itself is ensheathed in one of the most remarkable plexuses in the body. The carotid arteries have several nerves in intimate connexion with them (so much so that in taking up the vessel there is always more or less danger of including some of them in the ligature), while branches from those remarkable bodies—the cervical ganglia, are also found wreathed upon their coats. In a word, plexiform arrangements, not to multiply instances, are almost always situated in the neighbourhood of arteries, often inclosing whole trunks more or less accurately; as, for example, in the case of the cœliac, coronary, hepatic, splenic, superior mesenteric, and renal arteries. Now when we contemplate these parts, and reflect that in many instances the nervous fibrillæ are seen to pierce the outer part of the arterial tube, and mix in the tissue of its coats, how is it possible to resist such evidence of nature's design to impart active vitality to arterial structure? in other words, to endow it with direct and immediate, not secondary, function? We must either admit this inference, or else reject *in toto* the powers and uses attributed to nerves, and regard the beautiful system of nervous agency as an idle invention, and the curious and complicated arrangement of nervous plexuses as the device of chance or the result of caprice, and a wasteful expenditure of workmanship by an All-wise Architect. Assuming, then, that these active agents were never placed here merely as ornaments and to excite the wonder of anatomists, where an important function was to be performed, it seems plain that the function of arteries was intended to be obedient to nervous influence; and keeping this position steadily in view, without affecting to call any of their tissues by any particular name—as, *e. g.* muscular or nervous—we find a substantial reason for the two structures being found in such close and constant relationship, and now proceed to examine some of the phenomena of disease, as well as of health, in reference to this subject. Inflammation occurs beneath the tendinous expansion of the finger; the morbid impression that is made upon the local nerves is here undeniably evidenced by the pain; the arteries leading to the part then begin to throb. How does this happen? Many physiologists pretend to explain it thus: the heart becomes excited—how? through the nervous centre; *i. e.* the nerves convey the morbid impression to the brain, the brain transmits it to the heart, and the heart, becoming excited, sends its blood with increased force to the vessels leading to the inflamed part. But why into these vessels, and not into all the vessels of the body, seeing that the heart's action, though excited, is still uniform? The explanation is inadmissible, as well as incomprehensible, until the heart is first proved to be an arbitrary independent existence, endowed with most extraordinary intelligence, wisdom, and power; and not a mere hydraulic machine possessed of vitality. How much more easy is it to conceive that the delicate nervous fibrillæ, so closely and inextricably linked with the equally delicate ramusculi of the artery, should become (who can pretend to say how?) the immediate recipients of the morbid impression; that these little vessels, taking on

* See Dublin Journal, July 1837.

a brisk operation, the branches from which they spring become likewise excited, and at length, by reciprocity and continuity of action, as well as by the collateral agency of nerves at the several points where fibrillæ come off to wreathe upon and pierce the corresponding arteries, the great trunk of the vessel itself is excited to abnormal pulsation, and so disturbance of the general circulation and of the heart will follow, if the originating irritation be adequate to such an effect. But here the objection seems to lie *in limine*, that the nerves of the extremities are nerves of sensation and voluntary motion, and therefore their functions connected with volition and perception, while the functions of the arteries are not so connected; but it must be remembered that the brachial plexus, as well as other nerves on the outer parts of the body, derive supplies from the sympathetic; and these, acting as conductors from this grand centre of nervous energy, may, perhaps, endow with additional properties the cerebro-spinal nerves *.

In the various instances of increased flow of blood to the small vessels that can be adduced, a proportional increase must occur in the large vessels, yet it is easier to trace this up to nervous influence in the former case, from the inosculations, if I may so speak, between nerves and arteries being more close and constant in the minute than the large branches. A decayed tooth will sometimes produce intense pain, followed by inflammation and abscess; and if the matter escape through the cheek, an ulcer will be thereby left, which will scarcely ever heal until the tooth is extracted. Here we have vascular action directly produced by irritation of the nerves, and kept up as long as that irritation remains, but disappearing when it is withdrawn. The healing of an irritable ulcer, on the application of a strong escharotic, is another familiar instance of healthy vascular action succeeding to the subsidence of nervous irritation, and evidencing directly the control of nerves over vessels. "Irritability of the gastric nerves is a cause," says Dr. Johnson, "and not a rare one," of epigastric pulsations, "so that whenever food is taken into the stomach the heart is excited to abnormal action, and frequently accompanied by the phenomenon under consideration." Dr. Johnson here seems to attribute the phenomenon directly to the heart's agency; but however this organ may at times be affected by irritation of the gastric nerves, it appears unnecessary, from what is already advanced, to have recourse to a circuitous and doubtful explanation, while a far more simple and probable one is at hand, viz. that the aorta itself has its vital action morbidly altered by the influence of those same nerves whose branches are undergoing irritation in the stomach. Let us for a moment admit the other hypothesis. The pulsation of the heart will, of course, accord with that of the aorta; there can be no change in effect without a corresponding change in cause, and the propelling power of this engine being increased, the whole course of the aorta, with its branches, subclavians, carotids, and iliacs; in a word, the entire circulation, must respond to its impulse; but we find this is not the case, and therefore the hypothesis goes for nothing. A small portion of the arterial system, a single artery, a part only of an artery, presents the phenomenon of increased pulsation; and I can see no possible or rational means of accounting for it without admitting the principle of active, though now morbid, vitality in that part of the arterial system wherein it is exhibited. In a limb affected with the gout, the arteries beat more violently than in the opposite limb which is not so affected. What has the heart to do with this? The blood flows through the aorta to its bifurcation as before. What change can we now conceive taking place in the action of the heart, leaving structural alteration out of the question, which impels the stream more into one tube than into the other? It is impossible to imagine any. We are, therefore, led of necessity to look to the vessels themselves, or something influencing their action, for an explanation of the fact. In certain conditions and habits of body the pulsation of the carotids being fuller and more frequent in proportion than that of other arteries, a strong pressure made on one, and more especially on both, will often be found immediately to diminish the frequency and force of the heart's action; and yet, upon the with-

* Besides these extra supplies, the sympathetic, in its course along the spine, regularly communicates with every pair of the spinal nerves, with each of the cervical nerves by one filament; and with each dorsal, lumbar, and sacral, by two.

drawal of that pressure, the heart and these arteries beat as before. If the excited action of the heart were alone sufficient to cause increased arterial pulsation in one direction, what is to prevent its now transferring it in another direction, where, under equal circumstances, other vessels should be equally susceptible of the impulse? but instead of this, the impulse itself is lessened.

In the excited state of the entire vascular system attendant on immense losses of blood, with vehement arterial throbbings simulating the most violent inflammatory action, what can the heart have to do? Is not such a condition dependent on a high state of nervous derangement propagated directly to arteries as well as heart? It appears, therefore, to be clearly demonstrable, that irritation can be propagated generally from the nervous system to the arterial, as well as locally, from a particular part of the nervous system to a particular part of the arterial; in the former instance without the intervention of the heart, in the latter without the assistance of either heart or brain; and thus it is we see how the intermediate tubes, both in health and disease, come to act in concert with the two extremes of the circulation, accommodating the impulse of the heart upon the one hand, and the function of the capillaries upon the other, nervous agency being the fountain and medium of this beautiful harmony. We find, also, in these principles, the basis of Andral's doctrine, that "a lesion of innervation may be followed by that of circulation," as well as the exact mode of the extension of diseases by sympathy through the local nerves to the local vessels.

We need scarcely here stop to notice the electrical rapidity with which certain moral impulses operating through the nerves will sometimes be seen to act upon a part of the circulation, as seen in the sudden pallor of the countenance from fear, the equally sudden suffusion of it from sense of shame, nor yet the rapid distension of the corpus cavernosum and spongiosum, from sexual desire*; nor the occasional manifest differences in the circulation of two extremities of the same body, to one of which the same fluid will feel hot, to the other cold, nor any of the other deviations from the regular distribution of blood through the vessels, which, to whatever cause subservient, whether moral, physical, or morbid, can never be attributed to any functional condition of the heart, without the gross absurdity of first imputing innate intelligence to this organ to render it cognizant of an infinite variety of impressions, as well as arbitrary power over the entire circulation, to regulate it at discretion*. Making all due allowance for the projectile impulse of the heart, and the organic irritability of the capillaries, something is still wanting to lead the intermediate tubes to act in unison with both, and this influence, it has been attempted to be shewn, resides in nerves which keep up a constant companionship and intercourse with arteries all through the system. If this position be once admitted, we have a rational means of accounting for every possible variety of irregular pulsations in arteries, by referring them to certain states of the nervous system, or some portion of it, which are not healthy, the nature of the morbid condition being sometimes apparent and sometimes not; the former being generally the case, when produced by a local congestion or inflammation, the removal of which will also remove irregular pulsations in the arteries; but how many are the shades and modifications of disease—at least of departure from healthy function—the origin, progress, and nature of which are entirely hid from us, which are either too subtle for our senses to apprehend, or which have never yet been so investigated as to be brought to light, and it thus appears to me we should consider those abnormal pulsations which are so often met with in hysterical and hypochondriacal people, which may not (yet sometimes) *have* their origin in congestion or inflammation; and which, in reference to the views above set forth, are properly enough called *nervous*. In such cases the co-existing derangement of the heart's action is literally a co-existing effect; though when once produced, no doubt acting as a subordinate cause; and this appears to be the proper solution of Dr. Johnson's very well founded remark about the constant disturbance

* It has been found, by experiment on a horse, that a division of the pudic nerves destroys the power of erection.

* In acephalous monsters, as is well known, the heart is almost uniformly absent, being reduced to the simple tubular form.

of the heart's action in hysterical patients and hypochondriacs. "Our physiological theories," says Mr. Abernethy, "should be adequate to account for all the vital phenomena both in health and disorder, or they never can be maintained as good theories."

REMARKS ON DR. ELLIOTSON'S STRICTURES ON SPURZHEIM.

To the Editor of the Medical Gazette.

SIR,

THE spirit in which Dr. Graves has responded to the attack upon Spurzheim, made by Dr. Elliotson in his last edition of Blumenbach's Physiology, deserves the thanks of all lovers of truth. Allow me sir, also, through the medium of your useful journal, to reply to the unfair conclusions of the Professor. Speaking of Spurzheim's published opinions, he says, "In his fondness for changing his names, his arrangement, and the numbering of the organs, he introduced confusion without advancing knowledge. To prove Spurzheim's speculative spirit, I may mention that, instead of giving the origin of any of his asserted discoveries, as Gall did, and adding a host of examples, he tells us, in regard to the organ of inhabitiveness only, that a gentleman much attached to his house had a particular spot of his head much hotter than any other; and in regard to the organs of hope, marvellousness, conscientiousness, size, weight, order, time, he neither tells us how he discovered them, nor adduces a single proof."

Does Dr. Elliotson mean to insinuate that Spurzheim was not justified in modifying his opinions, as experience gave him further opportunities of perfecting them? and was not this the very principle by which the founder of phrenology, the illustrious Gall, arrived at such important physiological truths, and which our ever to be venerated countryman, Bacon, pointed out so distinctly?

If Spurzheim's opinions and observations had not advanced knowledge, surely Dr. Elliotson's intellectual endowment would have found no difficulty in exposing their weakness, had he been so inclined, by direct reference to his works, instead of attacking by insinuation the character of the dead! I would ask Dr. Elliotson if he has ever read or understood the 7th section of Spurzheim's Phrenology, 3rd edit. 1825? There would be found the reasons for Spurzheim changing the names of organs, and for differing from his preceptor, Gall, without attempting to take from him a justly merited reputation as the discoverer of the physiology of the brain. This section being so long, I will only direct immediate attention to the following quotation from it, in order to shew Spurzheim's philosophical turn of mind as well as his candour:—

"In examining the fundamental powers of the mind and their organs, I shall always follow the same procedure. I shall first consider the individual actions, then give the history of the discovery of the organ, and add my remarks where Dr. Gall and I happen to differ in opinion. I shall describe the seat of each organ, and name it according to its essential function; and, finally, I shall examine its influence on the other faculties, and the effects of its inactivity. It is my intention rather to make known the philosophical spirit of these inquiries, and the manner in which I conceive they ought to be conducted, confirmed, or amended, than to quote the numerous facts I have observed. Dr. Gall is fond of quoting examples: these, be they ever so numerous, however, can never produce conviction. I have neither the wish nor the intention to persuade, and therefore invite every one to convince himself by personal examination. The few cases I mention, and the numerous instances brought forward by Dr. Gall, shew that we have observed; we have, therefore, acquired the right to demand that no conclusion be formed until our observations have been repeated. This is the only way of establishing new discoveries."

Thus may we see that Spurzheim's mode of following up the discoveries of Gall was different from what Dr. Elliotson seems to have wished. Spurzheim had a desire to look for truth, and the power also to find principles; his mind was too capacious to feel fully occupied with details, *though he never neglected them*, and he offered the results of his experience to direct others. Professor Whewell, in his Introduction to the History of the Inductive Sciences, makes some philo-

sophical observations on the "Successive Steps in Science," which so obviously apply to the progress of the sciences of geology and phrenology, that I cannot forbear quoting them :—

"But there is another remark which we must also make. Such sciences as we have here to do with are commonly not formed by one single act; they are not completed by the discovery of one great principle. On the contrary, they consist in a long-continued advance, a series of changes, a repeated progress from one principle to another, different and often apparently contradictory. Now it is important to remember that this contradiction is apparent only. The principles which constituted the triumph of the preceding stages of the science, may appear to be subverted and ejected by the later discoveries; but, in fact, they are (so far as they were true) taken up into the subsequent doctrines, and included in them. They continue to be an essential part of the science. The earlier truths are not expelled, but absorbed—not contradicted, but extended; and the history of each science, which may thus appear like a succession of revolutions, is, in reality, a series of developments. In the intellectual, as in the material, world—

"Omnia mutantur, nil interit,
Nec manet ut fuerat nec formas servat easdem,
Sed tamen ipsa eadem est."

"All changes, nought is lost; the forms are changed,
And that which has been is not what it was;
Yet that which has been is."

"Nothing which has been done is useless or unessential, though it ceases to be conspicuous and primary.

"Thus the final form of each science contains the substance of each of its preceding modifications; and all that was at any antecedent period discovered and established, ministers to the ultimate development of its proper branch of knowledge. Such previous doctrines may require to be made precise and definite—to have their superfluous and arbitrary portions expunged—to be expressed in new language—to be taken up into the body of the science by various processes; but they do not on such accounts cease to be true doctrines, or to form a portion of the essential constituents of our knowledge."

I would fearlessly appeal to the members of scientific bodies in Great Britain, Ireland, France, and Germany, as well as to the medical officers of our own charitable institutions, and to all who knew Spurzheim in public, for evidence of his candour and sound sense; and from personal knowledge, I may add, that for nearly four years his museum, at his house, in Gower Street, was publicly open once a week, where he attended, when in London, with a view to explain and illustrate all his opinions; and that he was at all times ready to give, to receive, and to appreciate any information for the elucidation of truth; as I was, during that period, constantly employed under his directions in dissections and observations upon the brain, and in collecting evidence from man in action, for the express purpose of proving his positions to such as chose to inquire. In common with his friends, too, I can bear testimony that Spurzheim was remarkable for his mildness, urbanity, and candour, as well as for his love for the science he had espoused, which terminated only with his life.

Though, at first, the magnitude and importance of the subject disposed many to doubt the truth of phrenology, still the greatest of all tests, Time, has not brought candid and sincere opposition to bear upon it; for the discoveries of Gall, as carried out by Spurzheim, and the principles deduced therefrom by the latter especially, are acknowledged to be correct in the main, by all who have investigated the subject with a view to find out truth. And of what value are the assertions on any subject, of those who neither profess knowledge nor wish for conviction? Had Spurzheim lived, nothing would have delighted him more than to see the interest now taken by the public in the science of his heart and the pursuit of his life. I have heard him repeat that he was not the first, nor would be the last, that was not generally understood during his life; that if man did not *die*, moral and intellectual improvement would be still more slow.

I would respectfully, but firmly, assure Dr. Elliotson, that his *fiat* certainly will not annihilate the labours of Spurzheim's whole life. The field of nature is broad enough for all her children to reap a plenteous harvest, without destroying each other's property in order to raise the value of the produce of individuals.

Another accusation of Dr. Elliotson's

is, "Spurzheim changed even the situation and extent of organs in his last plate. The space allotted by him to marvellousness was originally between imitation, mirthfulness, hope, and ideality; now it is more than twice its former size, and placed between these four and veneration; covetiveness (acquisitiveness) was placed by Gall, and admitted by Dr. Spurzheim, before cunning (secretiveness,) and between ideality and cautiousness."

Need I ask any honourably feeling or educated man, if acknowledgment of intellectual error be a crime? I would demand of Dr. Elliotson if he has not gained clearer and more extended views by further experience; or would he only deny that acquirement to others?

The phrenological-marked bust referred to by Dr. Elliotson is not intended to represent a real head, but only to give the student a familiar knowledge, and to facilitate his acquisition of the name, number, and general situation of the organs: these are represented on its surface as they might exist in some real head; but because in each individual there is more or less unevenness and irregularity of development, no abstract marking can, with exactness, be applicable to individuals. As each organ was at first discovered in the state of predominant development and activity (the lower propensities in criminals usually, and sometimes in the insane, the intellectual faculties and sentiments in geniuses and noble-minded characters) it would consequently, in each case, push aside the less well-marked neighbouring cerebral parts, and much time, therefore, elapsed before the usual relative size and situation of the cerebral organs could be at all determined. Each successive bust that was published, became, as the limits of the organs were more accurately defined, of more practical utility than the preceding; but no mere bust, that has been or ever will be published, can do more than indicate the general position of the several convolutions of the brain. Their actual situation is learned from observation of nature, which alone can give an acquaintance with the contour and external appearance communicated to each head by their relative development. It should also be clearly understood, that however differently Gall and Spurzheim might mark a plaster bust, they never disagreed respecting the *site* of any organ in the head of a healthy individual. It is true, that Spurzheim confirmed eight, and acknowledged the existence of two more cerebral organs, making in all thirty-seven, after Gall had ceased, from age, to exert the brilliant intellect and extraordinary vigour of his youth. It must also be remembered, that Gall marked the positions of his organs by circular or isolated borders, and left many parts of the brain with undiscovered specific functions.

Spurzheim's mode of marking was much improved in the last years of his life, by attentively considering the course of the convolutions; in fact, the anatomical proofs of phrenology were mostly followed out by Spurzheim, both during the more than ten years Gall and he worked together in common, and the nearly twenty years after, that Spurzheim laboured by himself: had he not been prematurely snatched away, we might have been still more benefited on the subject of anatomy establishing the truths of the physiology of the brain of Gall and Spurzheim. We all admit national characteristics, and as phrenologists we find national forms of head made up of different degrees of development of individual organs: in England, Spurzheim took the English form of head as a guide to instruct Englishmen.

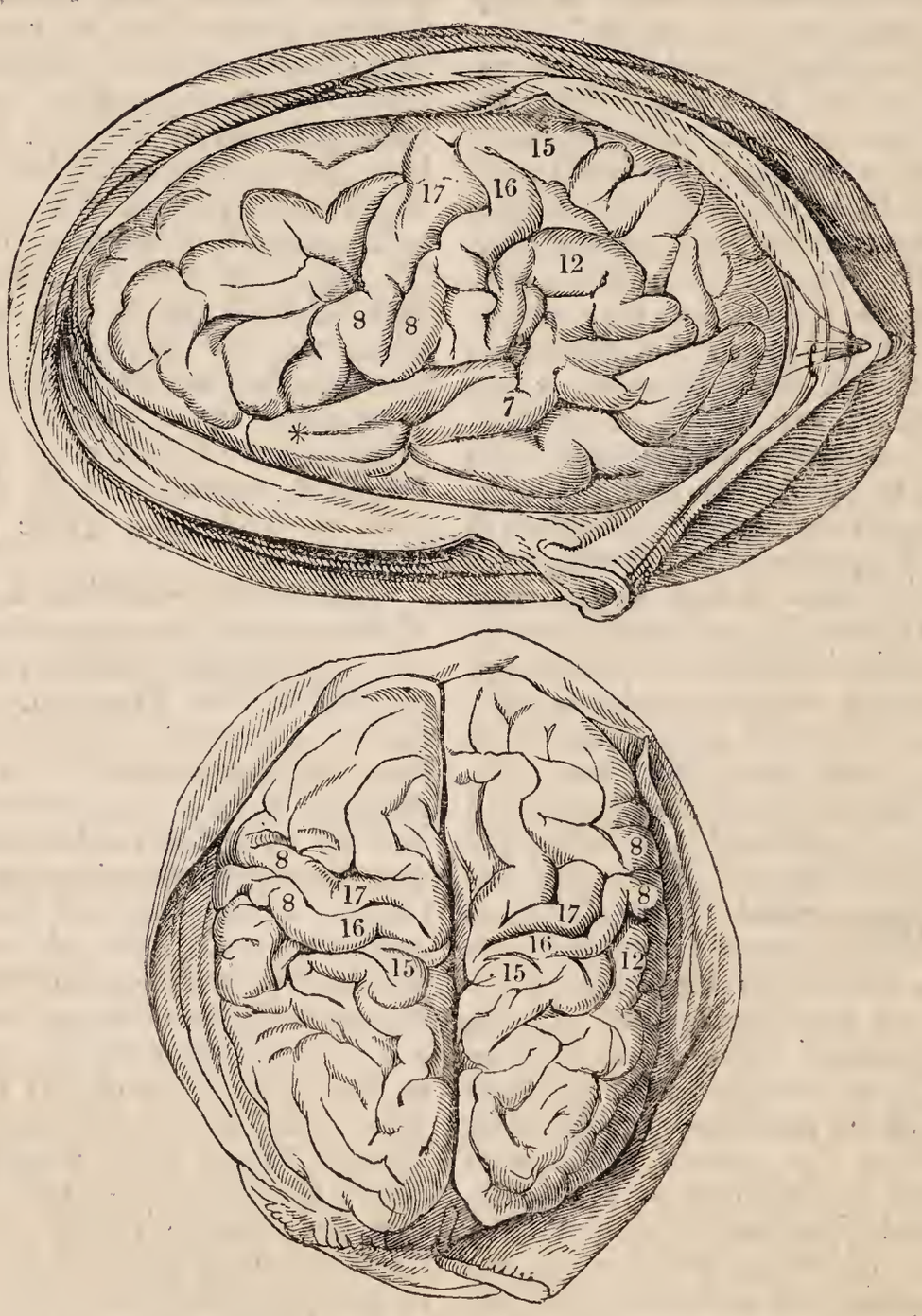
Gall has not published a clear view of the essential function of marvellousness; Spurzheim obtained much information in this kingdom, where the faculty is generally large, and takes a more important direction, viz. towards religion, than in France or Germany; and he modified its boundaries, from more attentively considering the convolutions of this part of the brain in connexion with external development, during his residence in this country, *though he did not change its situation.*

During life we can only approximate to truth; no phrenologist can exactly define the limits of convolutions on the exterior of the head, though practice, joined with principles, will give a facility for forming useful general data. The same map of the organs would as ill suit different heads as the proportions of the Apollo or Venus would be found to coincide with each individual of the genus man, though every healthy person has all the external and internal organs of the body relatively larger or smaller.

On the organ which Dr. E. calls

Covetiveness, he says that Spurzheim once agreed with Gall, previously asserting that the former changed the name, situation, and extent afterwards. Spurzheim did change the name of this organ, at the suggestion of the Edinburgh phrenologists*, because he considered the term Acquisitiveness expressed its essential function better than Covetiveness, which might be taken to imply bad motive. Gall, in 1818, 4to. edition, gives four names to this faculty, viz. *Sentiment de la propriété*, *Instinct de faire des provisions*, *Convoitise*, *Penchant au vol;* and in 1825, 8vo. edition, he adds the German terms, *Eigenthumsinn*, *Hang zu Stehlen*.

Spurzheim altered the situation and extent of Acquisitiveness on the marked bust, on precisely the same principle that he modified the marking of the organ of Marvellousness, viz. by corrected and amended observation of the direction and extent of the convolutions forming this organ. Dr. Elliotson appears to be ignorant of the regularity of the convolutions forming this organ, as well as of its exact place and connexion with the organs of Hope and Conscientiousness, as Gall was, ten years ago. Now to prove this accusation, I refer to the human brain*. The great fissure of Sylvius divides the lateral convolutions as far



up as Cautiousness, 12; below it is placed the convolution of Secretiveness, 7, joining Cautiousness at the upper and back part; below and in front uniting with Alimentive-

* See note to Spurzheim's Phrenology, 3d ed. 1825, p. 165.

* The annexed engravings were made from drawings of a human brain, by my friend, A. M. Macwhinnie, Esq., of Bartholomew's Hospital, to whom I thus publicly offer my thanks.

ness(*); above this great fissure of Sylvius is a well-defined and distinct fissure, about four inches in extent (call it the fissure of Spurzheim, as he first observed and described it), taking a direction laterally downwards, and slightly inclined forwards from Firmness, 15 (externally in front of the crown of the head.) This fissure of Spurzheim separates at its sincipital extremity the convolutions of Conscientiousness, 16, and Hope, 17; the former being behind, the latter in front of it; and these two convolutions merge gradually into those of Acquisitiveness, 8, the fissure of Spurzheim now dividing that organ into two convolutions until their union at its termination, directly above the great fissure of Sylvius, about an inch from its anterior extremity. There are variations in this distance, however, as the proportions of the organs around are differently developed. I have examined attentively the convolutions of many brains during the last twelve years, at first under the direction of Spurzheim, and since his death as circumstances gave me the opportunity, and I never saw but one human brain in which there was a junction higher up, either between Hope and Conscientiousness, or between the upper part of the convolutions of Acquisitiveness, and that was only in one hemisphere. I appeal to the greatest authority, Nature, for proof of this statement, which any one may verify by stripping off the pia mater (following the direction of the large vessels is the easiest way), and tracing the course of these convolutions; indeed, their regularity is so clear and constant, that if Dr. Elliotson had ever looked for, he could not have failed in discovering them. I would also refer for further evidence to Spurzheim's published Appendix to his Anatomy of the Brain, which contains the substance of a paper read before the Royal Society of London, in May 1829, and some remarks on Sir Charles Bell's animadversions on phrenology, pl. i. iii. v. and vi. To pl. i. the human brain under ordinary conditions; to pl. iii. the imperfect brain of an idiot, now in the possession of Mr. Stanley, at St. Bartholomew's Hospital Museum; and to pl. v. and vi. the brain of an ourang-outang, formerly belonging to Dr. Leach, now in the Hunterian Museum at the College of Surgeons. In these plates the convolutions are numbered, viz. Acquisitiveness, 8 (Covetiveness of Gall); Hope, 17 (not admitted a fundamental faculty by Gall); Conscientiousness, 16 (also a discovery of Spurzheim, and not admitted by Gall nor by Dr. Elliotson); Secretiveness, 7 (Cunning of Dr. Elliotson); Cautiousness, 12. From my own observations, since the death of Spurzheim, on the human brain, I incline to think the convolution marked in these plates 10, is the organ of Firmness, or part of it, and can hardly agree with Spurzheim when he refers it to Self-esteem, which organ appears to me to be that marked 11 in the first plate. I have not had the opportunity of convincing myself by observation on this subject, either in the brains of ourangs or idiots, so merely throw out this suggestion to induce further attention to the fact, in order to confirm or amend the positions laid down by Spurzheim.

I have thus endeavoured to prove that Dr. Elliotson's investigations *upon the facts and inferences in question* are not very profound; shall we at once agree with him in his insinuations that Spurzheim's grounds for his discoveries were insufficient; or, as gentlemen and seekers for truth, shall we courteously respect Spurzheim's positions and principles till we prove them to be incorrect?

"Fiat justitia ruat cœlum."

I am, sir,
Your obedient servant,
Hy. Haley Holm.

55, Upper Norton St., Portland Place,
July 12, 1838.

FRACTURE OF THE CLAVICLE.

To the Editor of the Medical Gazette.

Sir,

Permit me to make a few remarks further on fracture of the clavicle. From the letter of "Observer," appearing in your journal of the 30th of last month, I am led to conclude the opinion that gentleman is solicitous of substantiating respecting the discriminating symptoms of fracture of the clavicle, may not altogether be misunderstood from Medical Gazette, May 26th:—"The patient can easily raise his hand to his head without increasing the pain, and has considerable motion with the limb."

This journal, June 30th, p. 589:—"Mr. Moseley has been pleased to designate such cases as the one to which I have alluded of rare occurrence; but I must confess myself rather sceptical regarding the truth of this assertion, upon which depends the relative value of the symptoms in point."

In all matters claiming much consideration, we undoubtedly should endeavour to discard from our thought (as far as possible) the impressions made by the writings of others, in order that we may examine for ourselves, and draw those deductions that should naturally follow an unbiassed and impassionate inquiry.

The subject of fractures, taking all in all, may be said to comprise within itself an epitome almost of the science of surgery; for the highest, the most curious, and most difficult questions, as well as the most homely and every-day matters of practice, fall within the limits of its wide domain.

If we for a moment consider the form, the position, and the connexion of the clavicle, we readily account for the liability of fracture, and cessation of the extensive range of motion of which this bone forms so important a part. The clavicle, slender and elongated in form, situated at the upper and anterior part of the chest, where it is covered simply by the integuments, forms a kind of pivot, upon the trunk of which the upper extremity turns; being placed between the sternum and the scapula, serves to keep the scapula and through it, the upper extremity, at a proper distance from the sternum, so as to give to the arm a free range of lateral motion. Under these circumstances the clavicle is frequently broken by direct violence; that is, by blows inflicted immediately on the bone itself. It is also liable to be broken in consequence of violence communicated to it through the medium of the upper extremity. Thus, when a person falls to the ground, and stretches out the arm to save himself, if he fall on the shoulder or the elbow, or on the palm of the hand, in either of these cases the clavicle is included between two forces, and the bone gives way at its weakest part, which is generally towards its middle.

When the clavicle is broken, it depends on the situation whether there will be any considerable displacement. If the fracture take place at the scapular end, within about an inch or an inch and a half of the articulation, there will not be any displacement from the under surface of the clavicle being there closely connected to the root of the coracoid process by its ligament.

Likewise if the bone be broken near to its external end, it is generally not displaced. But these are the most unusual situations in which a fracture of the clavicle is likely to take place; it more commonly gives way towards its middle, and then there is usually considerable displacement.

When this bone is broken, the shoulder being no longer retained by the bone in its proper lateral situation, advances towards the sternum; and this carries the scapular end of the fragments under the sternal end; the shoulder sinking, the scapular end of the clavicle is dragged downwards. The principal displacement then is inwards, and towards the sternum, which necessarily occasions the sternal end to overlap the scapular end.

The under motions of the limb, those that are performed with the arm hanging down, I have usually found take place without much inconvenience to the patient.

Your obedient servant,
F. X. Moseley.

80, Connaught Terrace,
July 7, 1838.

ON THE MISUSE OF THE PHRASE "SPINA VENTOSA."

To the Editor of the Medical Gazette.

Sir,

In the "Discours sur le Progrès des Sciences, Lettres et Arts, depuis 1789 jusqu'en 1808, ou compte rendu par l'Institut de France à sa Majesté l'Empereur et Roi (Napoleon)," M. Dacier says, "La philologie est la base de toute bonne littérature." I hope there may be among your readers some who will, on that account, not disapprove of the observations I have made upon the import of a medical phrase. If you find them admissible to your Medical Gazette, they will contentedly wait till all worthier contributors have had undisputed precedence.—I am, sir,

Yours respectfully,
Henry Lee.

21, Charlotte Street, Bloomsbury,
July 12, 1838.

Spina ventosa is a phrase which has fallen into misapplication, because it is misunderstood. The wide diffusion of its erroneous interpretation is probably owing to the deference paid to opinions disseminated by the Anatomical School of the University of Edinburgh. When a student there, in 1814, I well remember hearing the third Dr. Munro explain the word spina, as referring to the thorny spicula of bone which he exhibited, and which he stated were found to accompany that swelling or inflation of the bone to which the epithet ventosa had been appropriated. It appeared to me, at the time, that this was evidently a mistaken etymology, suggested by the appearance of the spicula. The word spina was originally applied in spina ventosa as it is in spina bifida. It meant the vertebral column. It is true that in the disease to which the name of spina ventosa was first applied, there are in many cases spicula or thorns of bone, but not in all; and the evil arising from misunderstanding the words, is, that these spicula are supposed to have given origin to the name, and therefore to have been first noticed as the essential natural characteristic of the disease. That tubercles grow imbedded in bone, is a fact which has been well known as long as the Hippocratic books. The growth of tubercles in such a situation is generally followed by swelling of the bone. The tubercle often disappears, and leaves the walls of its empty cavity very smooth, if ulceration has not succeeded to the interstitial absorption excited by the pressure of the tubercle, and making room for its growth. From this smoothness of surface, observant pathologists inferred for these cavities a cause different from that to which they ascribed ulcerated cavities with rough parietes. I saw M. Delpech at the Hospital of St. Eloi, at Montpellier, in 1829, exhibit such cavities laid open by a section of vertebræ, as a proof that some changes of structure are not explicable by the process of inflammation alone. The older pathologists conveyed, in the epithet ventosa, their opinion that the agent by which these cavities were formed was neither a solid nor a liquid, but that which Hippocrates called πνευμα, and later writers spirit, or wind. The disease being most obvious in the bones of the vertebral column, the expression spina ventosa was applied to the spine when affected with tumors containing, or suspected of containing, windy cavities, the consequence of tubercles or hydatids. When erroneous etymology pointed to the bony spicula as the feature from which the disease had derived its name, pathologists felt no incongruity in applying the words spina ventosa to all tumefied bones with spicular projections. If they had understood the original meaning of the name they might easily have avoided this incongruity by substituting for spina ventosa the words *Os ventosum*, or *Ventositas ossium*, to accommodate the name to any or all bones in which the disease might occur. But the misapplication of the word spina to the thorny spicula made them gradually lose sight altogether of the original meaning of the phrase spina ventosa, as referring either to the vertebræ, in which chiefly the disease was noticed by early describers, or to its essential character as connected with unulcerated morbid cavities. That an explanation of the phrase was needed, to put a stop to growing error respecting it, may be felt by reading the following passage in the "Morborum Puerilium Epitome" of the second Dr. Heberden:—

"*De spina ventosa.*—Ad strumam quoque pertinet ille morbus qui spina ventosa appellatur in quo *genu* tumet doletque et tandem ossa ipsa corrumpuntur. His accedunt febris hectica et tabes."

Spina ventosa is here described as a disease of the knee, without any hint of tubercular cavities. Pathologists had already, under the delusion of an etymological blunder, consented to confine the meaning of spina ventosa to spicular tumors of bone, and they are now taught that the seat of the disease is the knee. In order that they may not be further bewildered, it may be well for them to understand that the word spina in spina ventosa was originally applied to the spine or vertebræ, not to the spicula which characterize only one condition of the disease; and the notion of the disease should be enlarged to take in all tubercularly tumefied bones, whether actually containing tubercles or not—whether exhibiting spicular asperities or not. In other words, a tumefied bone exhibiting a solution of continuity with spicular projections, may

differ in no other respect from a tumefied bone withou tspicula, than that in the latter case a latent tubercle has not excited by its approach to the surface of the bone a degree of absorption sufficient to create a superficial breach. Without wishing to raise a doubt that tuberculated or inflated tumor of bone may in every other particular except the existence of a tubercle or tubercular cavity be of a strumous or scrofulous inflammatory character, these remarks are offered in correction of an expression, which has been perverted from its original meaning till it perplexes the student, and has become to many persons almost unintelligible, and obscures what it was intended to explain.

SULPHUR FUMIGATIONS AS A THERAPEUTIC AGENT.

To the Editor of the Medical Gazette.

Sir,

I beg you will allow me space in your valuable Gazette for a few observations, as connected with a case of Impetigo sparsa, detailed by Dr. Ronalds in your number of the 16th ult. That case, in all particulars, is as related by Dr. Ronalds; but he might have added, as stated to me, that his own skill had been exhausted, as well as that of his medical friends, and the materia medica also, in vain attempts to cure the said disease. I have in former communications asserted the almost specific effects of the sulphur fumigations in impetigo. The length of standing of the complaint seems to be of no consequence; indeed the more ancient the case, the more certain has seemed to be the benefit that was derived from the fumigations. Neither has it seemed to matter whether the disease was one extending over the whole surface, or confined to a part or region. I do not know that this observation has been made by any of the continental writers on diseases of the skin; but many years ago, the fact was forced on my attention, and has been recorded by me; and also, that in this particular form of cutaneous disease, the general functional health of the patient was seldom or never interfered with.

This latter statement is borne out by the detail of Dr. Ronalds, but is not noticed either by foreign or domestic writers on skin diseases. On the contrary, writers of both countries agree in speaking of a certain questionable deranged state of the general health, as the cause of this as well as some other cutaneous maladies, such as lepra and psoriasis.

In conformity with this consideration, these writers generally agree, that it is not advisable to attempt the cure of these diseases, when of long standing, inasmuch as they form a part (though an acquired part) of the economy of the patient's constitution. In this particular, too, I have ventured to rely on my own experience, and can assert, that of such cases, I can produce numerous instances, where the disease, though of long standing, was cured by a well-advised course of sulphur fumigations, without congestions, or any other bad consequences having followed. I have, it is true, often known relapses occur, but these were slight in degree, when compared with the original state of the disease; and when they were severe, they could generally be clearly traced to some excess, or aggravating cause, to which the patient had been subjected. In all such instances, however, the same remedial means proved adequate to restore the patient to comparative health. Where, then, lies the evil of curing these troublesome diseases? Certainly only in the imagination of those who entertain such idle fears. I can readily conceive such caution needful in attempting the cure of an acute eruptive disease, as small-pox; but for chronic diseases of the nature of those particularly alluded to, no such reserve is necessary. When patients labouring under chronic eruptions of the skin are advised to submit to their grievance, it seems to me but an acknowledgment of want of skill in the adviser, and ignorance of one of the most active means we possess of curing them, and a variety of other chronic maladies.—I am, sir,

Your most obedient servant,
Jon. Green, M.D.

40, Great Marlborough Street,
June 28, 1838.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

First Principles of Medicine. By ARCHIBALD BILLING, M.D. A.M. &c. &c. Third edition, considerably enlarged and improved. Highley: 1838.

DISEASE, treated empirically, apart from the facts of physiology, affords no data on which to establish a scientific treatment; but as soon as those facts are ascertained, we proceed on sure grounds, and cannot be mistaken in our conclusions, provided we reason on them as we ought to do. On so vast a subject as that of practical medicine, every one will enter with diffidence. In its study, he will be inquisitive no further than to obtain sufficient evidence of particular truths; and he will only investigate the testimony on which they rest, without perplexing himself with vain or speculative theories, irrelevant to the main object.

In illustration of this, the Baconian philosophy of medicine, we will adduce two books which stand unrivalled in their respective departments: the one is Sir A. Cooper's Treatise on Dislocations, the other is Laennec on Diseases of the Chest; in both of which, especially in the first named of the two, the diagnosis is clearly made out, and the right treatment follows as a matter of course. They are as free as any writing can be from all preconceived and *à priori* opinions, and rest their stability, we should rather say their celebrity, upon the practical application of simple inductive science.

In the practice of medicine, however, it is not always so easy to make out a clear diagnosis as it is in the practice of surgery: the elementary diseased action of diabetes, or typhus, for instance, cannot be demonstrated with the same tact and readiness as a dislocated hip. The elementary operation of disease is often beyond the focus of vision, both by reason of its minuteness and the depth of its locality. In these cases we have to arrive at right conclusions through the medium of general symptoms; and often when the general symptoms have guided us to a satisfactory result as to treatment, we must after all repose our judgment as to the proximate cause upon inference or analogy. But inference or analogy can no more be substituted for fact, than a simile can be accepted as an argument. We presume that inflammation of the hidden tissues (pia mater, peritoneum, &c.) may be illustrated by pathological reference to the visible inflammation of the external tunics of the eye; but at the best this is only a strong presumptive evidence, and does not arrive at direct proof of the fact any more than a dislocated wrist would point out the particular signs of a dislocated hip. It is thus that practical medicine falls short of the exact sciences.

In the book which we propose to notice, the author has attempted to deduce right treatment from a previous knowledge and recognition of the intricate nature of diseased action. In illustrating the nature of diseased action, he avails himself of the discoveries of modern science, foreign as well as British, as to the functions of the cerebro-spinal and ganglionic systems of nerves, and the capillaries of the circulating system. Many of these discoveries of modern science have been proved, and are received as true; but some of the functions, both of the minute nerves and vessels, yet remain unproved, and can be ranked only among the many truths which we infer from analogy or presumptive evidence.

The object of the work is highly praiseworthy. Medicine can never exalt itself above what it is now, so long as disease is to be studied by symptoms only, and treated according to its name. For the best practitioners are undoubtedly those who are the best informed on the functions of the living body at large, and regard disease, not according to a certain nosology, but in its physiological relations and its general bearing on the total powers of life, organic and animal. Under this view disease is simplified: the physician no longer comes to the bed-side to visit one of the thousand-and-one diseases mentioned in the nosology, but to investigate the pain, the inconvenience, perhaps the danger, which the erroneous play of a single function exercises over all the others. Thus disease begins to be studied anatomically as well as physiologically; and the inquiry is prosecuted upon the same plan as that by which the surgeon seeks to learn what is the particular mischief inflicted on a

certain joint recently injured. This is a great advance in medicine—it is the first step from empiricism to a practical medicine based upon the laws of life.

Dr. Billing has thus stepped forward and endeavoured to rescue the practice of medicine from the entanglements of systems, nosologies, classifications, and hair-breadth subdivisions. He regards disease, at large, as having its joint and several manifestations through the medium of the sensorium, acting on the minute capillaries, and assuming different phases according to the structure attached, the predominance of vascular or of nervous excitement, and the greater or less intensity of the morbific poison or diseased action. He views life as a whole; and, without bewildering himself in the delusive phrases of "life," "principle of life," "vitality," &c., he investigates the particular vital functions of particular organs, attributes to them their particular vital endowments, and surveys them as they perform or falter in their allotted parts under the vital control of the governing organ of the body—the brain and spinal marrow, with its (mysterious) companion the ganglionic system. A summary of all that is known and received as true concerning motion, sensation, and organic function, in obedience to the great sympathetic, the medulla spinalis, and the sensorium, is related (pp. 108—116) with the brevity and perspicuity of one who is master of the subject. We have read works of physiology which thus treat of these organs and their functions; but it is only of late that we have perused books on practical medicine which have treated these functions in this manner; and very few, if any, have so successfully applied them to clinical subjects as the present author.

The third edition has now appeared, much enlarged. The letterpress and "getting up" has been much attended to; several typographical errors, especially of punctuation, have been corrected, and the whole is produced in a comely shape. If the work have no other merit than this, it will have quite enough to claim the attention of the student—namely, the care with which the functions of the nervous centres have been studied and explained, and, above all, their immediate application to pathology and bedside practice. Indeed, it is the leading virtue of the author never to lose sight of the bedside; and the numerous points of treatment in regard to the exhibition of remedies, distributed throughout the work from the first page to the last, would, if extracted, alone form a catalogue worthy of being committed to memory. We again revert to the pathological observations on the nervous system, without an accurate and comprehensive knowledge of which the right treatment of paraplegia, tetanus, hydrophobia, neuralgia, and the extensive family of neuroses, must for ever remain vague and indefinite, obscured by the still more indefinite and vague terms of "nervousness," "nervous irritability," "asthenia," "debility," &c. which are nothing more than the masks of ignorance.

We should be ungrateful to any gentleman who has made such advances into a field of inquiry so unknown and so extensive as that of the nervous system, if we did not acknowledge our obligations for what has been produced, as affording to the practitioner more clear and enlarged conceptions of disease, and in all probability lending a seasonable aid to a more full recognition of the particular laws of life at the bedside.

MEDICAL GAZETTE.

Saturday, July 21, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

SELF-SUPPORTING DISPENSARY AT DRESDEN.

THE various schemes which have been proposed in this country for enabling the poor to provide themselves, in part, with medical assistance, have naturally excited much attention; and we think, therefore, that it may not be uninteresting to review the Report of a Self-supporting Dispensary on the continent*, from which our readers may deduce, as far as the difference of manners will permit, some hints that may be applicable to similar institutions in England.

* *Schmidt's Jahrbücher*, April 30, 1838.

It seems that Dr. Klose, now resident at Dresden, had founded a self-supporting dispensary at Leipsic, in the year 1830, which has been in existence ever since; this, however, was only for workmen and apprentices.

The one which he set on foot at Dresden, and which was founded on the 15th of January, 1837, was destined not for workmen only, but for persons of all ranks. It was thought that many respectable families and single persons were thrown into a state of pecuniary embarrassment by sickness, and its attendant expenses, added to which there was often a stoppage of the trade by which their livelihood was obtained; and that these difficulties were so great, that years of painful economy would be required to get free from them: that from habitual parsimony they not unfrequently delayed calling in medical assistance betimes, so that ailments originally slight, became difficult, or even impossible, to cure: and, lastly, that even in cases of extreme urgency, persons of this kind were desirous to keep up a decent appearance among their fellow-citizens, and were therefore most unwilling to crave assistance from a public institution. The distinction between a dispensary of this sort, partly supported by honorary contributors, and a public institution, seems rather nice; but much lies in a name, and if the men of Dresden subscribe more readily to a private institution, a private institution let it be called. The Dresden establishment was arranged in the following way:—

1. The *members* (or what in England we call honorary members or contributors) founded the institution, and carry it on, supporting it by contributions of at least two dollars (six shillings) a year; but they leave the transaction of ordinary business to the Committee, which consists of seven or more persons, of whom three at least must be physicians, but not those acting for the Dispensary.

2. The subscribers pay six *pfennigs*, or three farthings a week each, which is to be paid quarterly in advance, for which they obtain advice and medicine. In some cases, when the Committee think fit, they are also allowed a nurse and food; and if this, too, is insufficient, they are placed in some respectable family, or in a suitable public institution for the sick.

We should imagine that these greater aids have hitherto existed only in theory; at any rate they are not specially set down in the detailed account of the expenditure, which we shall give further on; and they cannot be included under the head of sundries, the charge for which is evidently next to nothing.

The subscribers are persons of either sex, habitually living in Dresden, and concerning whom the Committee feel convinced that, in case of sickness, the means at their disposal are very limited. Persons are not admitted who live out of Dresden; or who, in case of illness, can obviously pay for advice; or who have claims on any other public or private assistance; or who are in a permanent state of bad health.

We do not quite understand the third ground of exclusion; for it is not to be supposed that Dresden is without a great public hospital, and every poor person must necessarily have a claim to be admitted into it; some still more stringent claim must be meant, such as that of a soldier to be admitted into a military hospital.

The subscription is to be paid in full for each person, with the exception, however, that when the subscription is for more than two children under 14 in the same family, only one *groschen* (three halfpence) is to be paid for them all.

A detailed "Notice of the Relations

and Duties of Subscribers to the Society," is given to each person with his receipt.

The first Annual Report of the Society has been published, giving an account of its proceedings from January 15 to December 31, 1837; in which the following are the chief points:—

The King and Queen of Saxony, as well as the Princes and Princesses, have subscribed to the institution, on condition of sending patients not previously named. The Committee selected an apothecary's shop and three physicians, for whom Dresden was divided into three districts, as nearly equal as possible, each having the charge of one. If this Dispensary should become very popular, it would, of course, be requisite to add largely to the number of physicians, as Dresden contains 70,000 inhabitants.

All the previous arrangements having been made, the first subscribers came forward in the beginning of June, and the first patient was treated on the 8th. The total number of subscribers, up to the 31st of December, was 66; of whom 18 withdrew in the course of the year, by discontinuing their subscriptions. Dr. Klose, however, thinks that the speedy retirement of so many subscribers is not surprising, considering that the institution has not yet the advantage of an established reputation; and he is satisfied when he reflects that far more than two-thirds have remained.

Of the sixty-six subscribers, eight were literary or scientific persons (*privatgelehrte*), forty-two were mechanics, two tradesmen, three single women without any particular occupation, one an apprentice, three day-labourers, and seven servants (for six of whom the subscription was paid by their employers).

Of these, twenty-five required medical assistance, and four were sent by the royal family; making a total of twenty-nine treated.

Of the twenty-nine there were—

Cured	14
Relieved	5
Stayed away	2
Dead	1
Remaining at the end of the year	7

The cash account was as follows:—

RECEIPTS.

	Dollars.	Gr.	Pf.
Subscriptions from the Royal Family	29	21	0
——————honorary members	139	0	0
——————ordinary subscribers	36	18	0
Extraordinary donations	10	0	0
Sundries	0	5	0
	215	20	0

Making a grand total of about £32. 8*s*. No great sum, to be sure; yet far exceeding the expenses, which are as follows:—

	Dollars.	Gr.	Pf.
Establishing and arranging the institution	46	18	6
Fees of the three physicians	30	0	0
Apothecary's bill	19	19	9
Apparatus for the leg of a boy	7	0	0
Messengers' wages, and sundries	8	6	0
	111	20	3

Thus the whole of the expenses amounted to about £17; a sum which, in England, we should have considered hardly sufficient to cover printing, advertising, stationery, and other miscellaneous items. The apothecary's bill amounts to rather more than two shillings a patient, which is probably quite as much as it would do in London; the physicians come off with a fee of thirty shillings each, for their half year's attendance.

From the small number of patients during the first year, there remained a balance in hand of nearly 104 dollars, or about £15. 8*s*.; which will be useful to the institution, as the benevolent founder observes, in case of a large but temporary increase of expenditure, as during an epidemic.

The names of the physicians who are members of the Committee, are Von Ammon, Hedenus, Siebenhaar, and Klose; the last being also president of the Society.

One of the first objections that a captious critic might be disposed to make to the working of this German Dispensary, is, that though the sum raised and the sum spent are both exceedingly small abstractedly, yet that they are large when compared with the number of patients; for if we compare the sum actually levied on the rich and poor of Dresden with the roll of patients, we shall find that each sick man, woman, or child, costs more than a pound sterling; or, if we confine ourselves to the sum actually expended, each patient costs about 11s. 6d., a sum which at Dresden is certainly equal to a guinea in London. But to this we would answer, that great allowance must be made for the circumstances of a recent institution; and that it is probable that the next year double the number of patients may be relieved with nearly the same expenditure, the charges consequent on the establishment of the institution being diminished almost as much as the fees of the physicians and the bill of the apothecary are increased.

We may here observe that though the fee of each physician for half-a-year's attendance—namely, £1. 10s.—appears at first ludicrously small, yet it is at the rate of a little more than three shillings per patient, which must be better than the remuneration which our country surgeons receive for their pauper-patients, especially if we do not lose sight of the superior value of money at Dresden, as well as the fact that the English contract includes the medicine. The Saxon physician, too, probably does not get more than sixpence a visit from his poorer patients in private practice, so that the difference between these fees and the Dispensary ones cannot be very great.

Another point worthy of notice is, that patients of all ranks are received, provided they are unable to pay for medical advice. But since *unable* means very different things in the vocabulary of different persons, we should have been glad to learn what degree of indigence is supposed to qualify an inhabitant of Dresden to become a member of the *Kranken-Hülfs-Verein*. Some of our philanthropists think a woman too rich to belong to a Self-supporting Dispensary if she has an income of nine shillings a week; and it would be interesting to know if the same exclusion is affixed on the sum which would produce a corresponding degree of starvation at Dresden.

LONDON UNIVERSITY.

On Monday, the 17th instant, Mr. Gibson asked Lord J. Russell whether he had received any information that the Committee of the Faculty of Medicine of the University of London had for a considerable time suspended their labours, and that several members of the Senate had announced their intention to the Chancellor of not resuming their duties on that committee?—whether considerable dissatisfaction had not been expressed at the present defective nature of the charter by certain right reverend prelates, themselves members of the Senate?—and whether the Bishop of Durham had not stated his opinion in a letter to the Chancellor that if the present scheme were not materially altered it would prove abortive, and was calculated to make the University of London anything but an object of praise or imitation to other learned bodies in the empire.

Lord J. Russell had certainly been informed by the Chancellor that for some time the Medical Committee had not met in sufficient numbers to enable them to act, but, having conferred with several members of the University, he had every reason to believe that the difficulty would be put an end to; and that in October next proper persons

would be elected, so that no obstacle to the good working of the institution should exist.

Mr. Warburton inquired whether there would be any objection to the production of the minutes?

Lord J. Russell could not agree to that.—*Times.*

ANIMAL MAGNETISM.

To the Editor of the Medical Gazette.

Sir,

I had the honour of addressing you on the 26th ult., with the view of publishing my opinion of what is called Mesmerism, or Animal Magnetism; but I said, at the close of the letter, that if you thought it better not to publish it, I should wish to submit to your better judgment, and I should not consider it, in the slightest degree, as an act of incivility or disrespect. The letter has not been published, and the practice at the chief scene of action (the London University College) has got into some disrepute. I am of opinion, however, that it ought not to be allowed to pass by without censure; and the details contained in your last No. (40), as an extract from another periodical, are so disgusting that they deserve the expression of indignation from every medical man who has at heart the honour and respectability of his profession. I am therefore induced to trouble you again; and, as an old and original supporter of your excellent journal, I request the publication of this and my former letter.

I have the honour to be, sir,
Your obedient servant,
H. Terry,
Surgeon to the Northampton General Infirmary.

Northampton, July 11, 1838.

Sir,—I have been accustomed to see your journal from its earliest commencement. Several years I took it, and subsequently I have had it from a Medical Library. I have always held it in very high esteem, considering it most excellent, and useful both in its professional and moral character. I have thought it a safe book to put into the hands of young men, inasmuch as it could not fail to inspire them with sound principles, and thus lead them on to an upright and honourable practice of their profession. I have never before troubled you with correspondence, nor should I do so now, were I not really annoyed by the number for June 16, 1838, which I have just looked through. I find fault with you for so perseveringly troubling your readers (and, judging from my own feelings, I should say disgusting them) with the details and practice of what is called "the science of mesmerism." Surely, sir, the term science was never so sadly misapplied. Can any good possibly arise, theoretically or practically, from these ridiculous, unbecoming, and unprofessional exhibitions. One good, indeed, and one only, I will admit, has been produced, and that is, the calling forth from the pen of Dr. Cowan, of Reading, one of the most able, interesting, and satisfactory papers I ever read, refuting or explaining as it does calmly, yet most conclusively, in elegant language, and with the powerful arguments of common sound sense, all the trash we read about this pantomimic performance. Still, after this death-blow to mesmerism, and after a most splendid article previously published in the Quarterly Review, you recur again to the subject, filling up pages which might be much better employed, and disturbing many unsettled minds. I will make no further comment upon the paper which has called forth these remarks than this, that I earnestly request every sound sober-minded man who takes the Medical Gazette, to read it through deliberately, and then to favour the profession with his opinion of mesmerism. Should you do me the favour to publish this letter, I shall feel happy in having thus openly stated my opinion of mesmerism; but, on the other hand, should you think it better to decline publishing it, I shall most readily bow to your better judgment, and I shall not in the slightest degree consider it an act of personal disrespect.

I have the honour to be, sir,
Your most obedient servant,
H. Terry,
Surgeon to the Northampton General Infirmary.

Northampton, June 26, 1838.

[We think it is our duty, as public journalists, to record the passing events of the day connected with medicine, whether they be in accordance with our own private opinions or not. If Mr. Terry does us the honour to read our editorial articles on the subject, he can scarcely, we think, suspect us of favouring mesmerism.—Ed. Gaz.]

COVENTRY SELF-SUPPORTING DISPENSARY.

To the Editor of the Medical Gazette.

Sir,

I have troubled you, on two former occasions (vols. xii. p. 426, and xiv. p. 234), with statements of the origin and progress

of the Coventry Self-supporting Dispensary, which, I believe, will enable any one who is pleased to refer to them, to understand the principles and management of our institution: and in consequence of observations contained in some recent numbers of your valuable periodical, I am induced to trespass again on your pages, with a further communication on the same subject.

For the purpose of saving the reader's time, as well as my own, I have thrown my observations into a tabular form; which, with the aid of a few explanatory notes, will, I trust, be easily comprehended. The inferences deducible from them are obvious.

I. Tabular Statement of the Annual Income and Expenditure of the Coventry Self-supporting Dispensary (founded in the year 1831); together with the number of Patients treated in the Institution during each year:—

1. *Table of the Annual Income and Expenditure of the Honorary Fund*.*

ANNUAL INCOME.	£.	s.	d.
1831. Donations and Subscriptions	328	12	0
1832. Ditto	140	4	6
1833. Ditto	145	2	7
1834. Ditto	117	16	2
1835. Ditto	127	18	4
1836. Ditto	106	15	5
1837. Ditto	138	5	9
	£1104	14	9

ANNUAL EXPENDITURE.	£.	s.	d.
1831. Out-fit, including furniture of Dispensary, surgical instruments, &c. in addition to the current expenses of the year—viz. rent, taxes, salary of clerk, &c. &c.	207	8	9
1832. Current annual expense	152	7	9
1833. Ditto	116	16	0
1834. Ditto, including repairs, 21*l.* to Dispensary	113	3	8
1835. Ditto	90	2	0
1836. Ditto	88	8	7
1837. Ditto	84	13	9
	853	0	6
Balance in hand	251	14	3
	£1104	14	9

2. *Table of the Annual Income and Expenditure of the Free Members' Fund†.*

ANNUAL INCOME.	£.	s.	d.
1831	126	7	11
1832	366	3	9
1833	400	12	0
1834	415	13	1
1835	397	9	3
1836	392	2	1
1837	339	5	3
	£2437	13	4

ANNUAL EXPENDITURE.	Expenses of Medicines, and Dispenser's Salary.				Paid over to the Surgeons.		
	£.	s.	d.		£.	s.	d.
1831	45	10	3		80	7	8
1832	103	2	4		263	1	5
1833	132	12	0		268	0	0
1834	128	11	1		287	2	0
1835	135	6	3		262	3	0
1836	130	7	1		262	15	0
1837	94	3	7		245	1	8
	769	12	7	+	1668	0	9

= £2437 13*s.* 4*d.*

* The Honorary Fund is formed from the donations and annual subscriptions of the wealthier inhabitants, who, of course, do not avail themselves of the assistance of the Dispensary; and it is applied to the disbursement of rent, taxes, coals, candles, printing and stationery, clerk's salary, and, for the first two years, to the dispenser's salary. The latter salary is now paid out of the free member's fund.

† The Free Members' Fund is formed by the

3. *Table of the Number of Patients in each Year*.*

	No. of Patients.
1831	1505
1832	2437
1833	1668
1834	1629
1835	1500
1836	1610
1837	1482
	11831

My remarks on the above authentic data, extracted from the published annual accounts of the Dispensary shall be brief.

1stly. Seven years cannot fail to be thought a sufficient period of probation. The institution constinues in all respects efficient and flourishing. The accumulations from the Honorary Fund amount to 251*l.*; and the income from annual subscriptions exceeds the expenditure. But I beg to direct the reader's particular attention to the fact, that the *charity* of the *public* is taxed only to the extent of about 90*l.* a year, for the relief of about 1500 patients annually; and request him to compare it with the expenditure in eleemosynary Dispensaries.

2dly. I would note the amount of remuneration paid to the surgeons, which, in seven years, has amounted to 1668*l.*; and although this sum is, of course, below the deserts of the gentlemen who have received it, it cannot be considered degrading, or otherwise than respectable. But, perhaps, of all the *facts elicited* by our system, this is the most important and admirable—that, during seven years, 2437*l.* has been voluntarily and gratefully paid for medical relief, by a class of persons certainly not in better circumstances, nor higher in social position, than those who ordinarily are relieved by eleemosynary Dispensaries.— I am, sir,

Your obedient servant,

ROBERT ARROWSMITH, M.D.

Coventry, July 3, 1838.

weekly payments of the members of the institution, who avail themselves, whenever they require it, of the assistance of the surgeons. The number of free members is limited to 2500, and their eligibility for admission is tested by a committee, of which the surgeons are *ex officio* members. This fund defrays the charge for drugs, and the Dispenser's salary since 1832; and the residue is divided among the surgeons. It may be as well to repeat, that the Dispenser's salary, previous to 1833, was paid out of the Honorary Fund, which was objected to by those who thought the institution, as regarded all medical expenses, should be completely self-supported; as, in fact, it has been since that period.

* This table includes midwifery cases, and, for the first year or two, cases of vaccination also, I believe.

INEFFICIENCY OF THE CORONER'S COURT.

To the Editor of the Medical Gazette.

SIR,

THE question has been now before the public for several years, whether it be best for the interests of justice and the good of the community that the office of coroner be filled by lawyers, or by medical men?

I was invited to attend an inquest last night by a friend of mine, who assured me that the case to be inquired into would interest me as a medical jurist not a little. I did so. The result was painful and disgusting.

Eugene, *alias* Hugh James, *alias* Owen M'Mahon, was found dead, in the room occupied by his wife, on Monday night and Tuesday morning, by three parties, the only three examined at the inquest, except the deceased's wife, viz. a woman, residing in the same house, named Phillips; Bradley, a policeman; and Mr. Bachelor, a surgeon.

The evidence of the woman was, that the deceased had been drinking with his wife, and, when attempting to go out for "more beer," fell down a flight of narrow stairs—and, when picked up, was found dead. The evidence of the policeman was to the effect that, having heard of a broil in Mermaid's Yard, where the wife lodged, and also a rumour of *murder* having been committed, he proceeded to the place—found the deceased bleeding from the head—and fetched "a doctor." Mr. Bachelor swore that, at a quarter-past two A.M. on Tuesday, he found the deceased stripped to his shirt, and apparently "half an hour dead." The corpse was warm. There was blood on the forehead and back of the head, and a mark of something having been tied round the neck. Mr. B. bled him, and obtained a table-spoonful of blood; blood was issuing at the time from the ears, but none from the mouth.

The case for the crown was conducted by Mr. Stirling and his clerk; that for the ends of justice and humanity—by nobody.

Permit me, through the medium of the MEDICAL GAZETTE, to call the attention of the public to this case, as strongly illustrative of the impotence of coroners' inquests as at present conducted to elicit truth, or pronounce righteous judgment, in cases of sudden death; whether by the visitation of God, the occurrence of unavoidable circumstances, or by violence.

When the jury had been sworn, several of them desired to have, in the first instance, the testimony of the "medical gentleman." This was overruled by the

coroner's clerk, who told the jury it was for the coroner, not them, to decide upon the necessity of calling a medical witness. It was then proposed, and acceded to, that the jury should inspect the body, which was found in its coffin. The head was enormously swollen; a fluid, tinged with blood, and emitting the peculiar odour of a drunkard's breath, still oozing from his mouth, and the process of decomposition rapidly going on. No wound was visible on the forehead; and no opportunity was afforded of ascertaining whether any wound existed at the back of the head. The jury held their noses to keep out the stench, and returned to the jury-room. The aforesaid coroner's clerk remarked, examining the man's scrotum, "There is a large rupture here," which, however, was nothing more than hydrocele; and, with the return of Mr. Stirling and his factotum to the Rose and Crown, the examination of witnesses began. The character and substance of the evidence they delivered are before you. Considerable doubt attached to the credibility of the first witness. The evidence of the policeman said little of consequence; and the surgeon's was manifestly defective, because there was no one competent to interrogate him upon the medico-legal bearings of the case.

Two very singular circumstances, however, occurred on this occasion.

1st. The wife of the deceased—*being intoxicated*—was examined upon oath, although, as was stated at the time, herself suspected of being accesosry to her husband's death. Upon one occasion this witness exhibited an indecent levity, and, "*for fun*," swore to that which was false.

2nd. The earnest and repeated entreaties of several of the jury to let the medical attendant be sworn were resisted to the very last by the coroner and his clerk, and only conceded to when four gentlemen declared that they would sign no verdict until his evidence was taken. It was taken accordingly, and the sum and substance of it I have given above. But it would not be doing him justice were I to omit that he was examined by the clerk, not by the coroner; that this examiner had declared, when opposing the reception of his evidence, that the jury would know no more after it had been given than before; and that the same examiner, after verifying his own prophecy, turned with an expression of triumph to the jury, and inquired whether they were any wiser for it.

The verdict, I have since understood, was—"Accidental death, from a fall while in a state of intoxication, and accelerated by the neglect of the parties about him in not applying for medical assistance."

Upon the verdict your readers will place no reliance. Out of the proceedings themselves, the following questions arise:—

1. Did the deceased come by his death from accident?
2. From congestive apoplexy?
3. Or from external violence inflicted by his wife or some other person?

Mr. Bachelor said there was blood before and behind, and a wound on the back of his head. Where did the blood come from? How was the wound received? Had the wound been inflicted by a poker or other deadly weapon before his fall? or was it occasioned by his head coming in contact with the stairs, or any thing else, when he fell, or even after his fall? An examination of the head, and a comparison of the wound with the stairs themselves, its position, its size, its character—evidence as to the position of the body when first found as it lay after the fall, and the internal evidence supplied by the the state of the brain and its blood-vessels—were all necessary to the establishment of the truth, were all wanting, and were all alike uncalled for!

Such a thing as an inspection of the body beyond the surface seems not to have entered into any man's mind who was competent to meddle with the matter; and whether the man died drunk, apoplectic, or by a fall; or whether he was a murdered man, remains in as great doubt as before the inquest had taken place at all. And should circumstances hereafter transpire to render the last event a probable one, and one requiring judicial inquiry, such inquiry must be neccessarily impeded by the deficiency of the medical testimony—a deficiency the whole blame of which attaches to the old age of the coroner, and the incapability for examining medical witnesses of his clerk.

The reluctance of both these functionaries to let the jury have the assistance of the medical attendant's evidence, is altogether inexcusable. This is the second time that it has fallen to my knowledge to witness an inquest held under the direction of Mr. Starling—this is the seeond time I have known the purposes of justice defeated by the negleet to examine sufficient witnesses.

In the first case to which I allude, I had been called in to see a man who had come to his death suddenly. He was swollen, his skin discloloured, the foam fresh upon his mouth, his breath smelling strong of laudanum. An empty bottle, marked "Laudanum—Poison," was found upon the mantle-piece in the room where he lay dead. A memorandum, in his handwriting, written with a pencil, and dated the evening before his death, with this, among many expressions equally strong,

"I have evidently lived too long," was laid upon his table. The jury visited the room—looked at the body—expressed a conviction while there that he came to his death by poison—returned to the coroner—received evidence—and was asked if they would wish the testimony of the medical attendant. One juryman wished it exceedingly; another pressed for it, not so importunately; the coroner was willing; the clerk was unwilling; a majority of the jury decided that it would be an unnecessary expense. No medical evidence was received; but this was the verdict—"Died by the visitation of God!"

What, in either of the above cases, allow me to ask, was the use of an inquest at all, when such are the results? And why should the country be burthened with coroners' expenses, when their efforts are only directed to stifle inquiry—their authority of use only to preserve the parishes from paying for medical testimony.

I have brought forward these cases in the hope that your attention will be called to the subject; and, through you, that of the profession at large throughout the country; if haply, the office of coroner may be revised, and only those chosen to perform its functions whose previous education has prepared them to do so with advantage to the public, and with a reasonable protection to society by making inquiries into the death of its members something more than a solemn farce and formal mockery.

I have the honour to be, sir,
Your most obedient humble servant,
W. Barrett Marshall,
Surgeon, R. N.

6, Cheyne Row, Chelsea,
July 12th, 1838.

P. S.—As I intend to bring this subject under the notice of the Noble Secretary for the Home Department, may I request the favour of your giving it insertion in the Gazette, at your earliest convenience.
W. B. M.

MARISCHAL COLLEGE AND UNIVERSITY, ABERDEEN.

REGULATIONS RESPECTING THE DEGREE OF DOCTOR IN MEDICINE.

Instituted April, 1838.

I. Every candidate for the degree of M.D. shall produce to the Senatus Academicus satisfactory evidence,

1. That he bears a good moral character.
2. That he has obtained the degree of A.M. in this or some other University.
3. That he is twenty-one years of age, or that he will be so before the day of his graduation.
4. That he has attended medical classes for four years, and in each year at least two courses of lectures of six months each, or one such course with two of three months each; of which four *anni medici* one at least must have been spent at this University, and two others at this or some other University where the degree of M.D. is conferred.
5. That at such University he has regularly attended, once at least, the following classes, each for a course of six months, viz.—Anatomy, Practical Anatomy—Chemistry—Materia Medica and Pharmacy—Surgery — Midwifery, and Diseases of Women and Children—Institutes of Medicine—Practice of Medicine; and the following, each for a course of three months, viz.—Practical Chemistry—Botany.
6. That he has attended, for at least twenty-four months, the practice of an hospital containing not fewer than eighty patients; and, in such hospital, lectures on clinical medicine for at least six months, and lectures of clinical surgery for at least three months.
7. That he has attended, for at least six months, the compounding and dispensing of medicines, in the laboratory of an hospital or public dispensary, or of a licensed general practitioner, or of a regular dispensing druggist.

II.—The examinations of candidates for the degree of M.D. shall take place in April, and in the first two weeks of October; and the degree shall be conferred on the last Tuesday of April, and the third Tuesday of October.

III.—Every candidate shall deliver his certificates to the Professor of the Practice of Medicine, at least three weeks before the day of his examination.

IV.—Every candidate shall undergo two examinations before the Senatus Academicus, on two separate days, the first on anatomy, surgery, chemistry, botany, materia medica, and pharmacy; and the second on the institutes of medicine, the practice of medicine, and midwifery.

V.—Candidates shall, if they desire it, be admitted to their first examination at either of the two terms for examination, immediately preceding the term of their graduation.

John Cruickshank, Sec.

BIRMINGHAM EYE INFIRMARY.

Report of Cases, by R. Middlemore, Esq.

Ossification of the Crystalline Capsule.

Mary Larkin, æt. 60, the wife of a publican residing near Sutton, received a blow upon the right eye about eight years

ago, which deprived the organ of sight, but did not leave behind any manifest defect of any other description. About six months she complained of great pain in the eye, and, on examination of the part by her surgeon, Mr. Oates, of Sutton, the lens, surrounded by an ossified capsule, was found to be dislocated.

Nov. 1, 1837.—She complains of intense pain above the eye-brow, upon the cheek bone, and towards the nose. The forehead is acutely painful, and also the back of the head on the affected side. This intense hemicrania, or darting pain from the fore to the back part of the head, is usually complained of in those instances where ossification of any of the textures of the eye has begun to produce active irritation.

There is a slight zonular arrangement of vessels around the cornea, which is occasionally much increased, and it is manifest that she has suffered for some weeks past from chronic iritis. Immediately behind the cornea there is a globular body obviously covered by a white membrane, interspersed with dense yellowish-white spots; the iris is pressed backwards by the presence of this body in the anterior chamber: it was evidently the lens within the anterior chamber and surrounded by its capsule, the anterior hemisphere of which was converted into bone, but being more perfectly ossified at one part than another, the mottled and irregularly and densely dotted appearance I have mentioned was perceived. I scarcely know how to describe these appearances, but a person who has once seen them, has no difficulty in recognizing them when presented to his notice a second time. The removal of the ossified part was proposed and acceded to.

Operation.—Nov. 18, 1838.—Assisted by my friend Mr. Willcox, I made a section as for extraction at the lower part of the cornea, and, with little difficulty, removed an ossified capsule, which is now among my preparations. The lens was of an amber colour, and was not very opaque; the posterior capsule was scarcely thicker than usual, and nearly transparent; but the anterior hemisphere of the capsule was almost entirely converted into a smooth plate of bone, except near the margin of the union between the anterior and posterior hemispheres of the capsule, where it constitutes a rugged ring of bone.

Treatment—The lids were carefully closed after the operation, and a narrow bandage was lightly passed over them. The patient was directed to lie in bed, and to have her apartment darkened; a little aperient medicine was administered, and the diet was lowered. By these means, perseveringly adopted for about a week, the patient was enabled to return home, and in about a fortnight afterwards she called upon me, when the following was the condition of the eye:—Nearly free from inflammation; pupil clear, but slightly drawn towards the incision of the cornea. The cornea is in no degree staphylomatous, and the wound inflicted at the time of the operation has healed very perfectly. The sight of the eye is entirely destroyed.

Staphyloma of the Cornea—James Shephard, æt. 24, sustained a severe injury to the face some months ago, which produced collapse of the right, and occasioned the following condition of the left eye:—Two-thirds of the cornea at its lower part has become prominently staphylomatous; the pupil is closed, and the iris is adherent to the upper part of the staphyloma. The eye-ball is a good deal inflamed. The objects it was desirable to accomplish in this case were, first, to lessen the size of the staphyloma; second, to remove the ophthalmia; and third, to form an artificial pupil.

As one-fourth of the cornea, and a corresponding portion of the iris, were healthy, it was, I repeat, desirable to make an effort to form an artificial pupil; but, of course, before this was attempted, it was necessary to cure the staphylomatous projection by some method which would not endanger the occurrence of atrophy of the eye-ball. The use of the seton was improper on account of its great liability to produce a degree of inflammation adequate to affect injuriously the corneal or irital texture; and the removal of the projecting part by ligature or the knife was improper, by reason of their direct tendency to cause collapse of the eye-ball. The repeated tapping of the part, by means of a fine iris-knife, was not open to this objection, and, although a mode of treatment not generally to be recommended for the treatment of staphyloma, was in this instance adopted, and with perfect success; so that this person's eye is now in a fit state to be operated upon for artificial pupil.

In all cases of partial staphyloma of the cornea where it is desired to leave the eye in a condition to permit the formation of an artificial pupil—in all instances where it is specially important to avoid the displacement of the lens, and the risk of producing atrophy of the globe—the operation of tapping is to be preferred; but, on account of its tediousness, and its frequent inadequacy, it is not adapted to the cure of the large and extensive variety of staphyloma, or, indeed, of any form of staphyloma the walls of which are much thickened. These last varieties of staphy-

loma are best treated by the removal of a small portion of the most prominent and attenuated part, as formerly explained*; but, instead of using Beer's extraction knife, I now prefer to employ one the blade of which resembles the ace of spades, only narrow in proportion to its length, and having a cutting edge on either side—a sort of double Beer's knife, one edge of which when the point is introduced within the cornea, is opposed to its upper, and the other to its lower margin.—*Trans. of Prov. Med. & Surg. Association.*

DEATH OF DR. SIMS.

We regret to announce the decease of Dr. Sims, which took place at his house in Cavendish Square, of fever, and after a very shortillness, on Thursday night, (the 19th). Dr. Sims was one of the physicians to the St. Marylebone Infirmary, and a member of the Senate of the London University, in which he took rather an active part; and where he was present at a meeting so lately as the 11th instant. He had contributed several papers of value to the Transactions of the Medical and Chirurgical Society; and had considerable business among the fraternity of Friends, of which he was himself a member.

KING'S COLLEGE, LONDON.

DEGREE OF M.D.

The University of Edinburgh has determined that three out of the four years of study required by the University for its degree of M.D. may be passed in studying at the medical school of King's College, London. The regulation thus made places this college on the same footing as the Universities of Dublin and Paris with regard to Edinburgh medical degrees.—*Times.*

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO HAVE RECEIVED DIPLOMAS.

May 1838.

J. F. Goude, Dorset Street, Salisbury Square.—W. W. Williams, London.—R. Hills, Maidstone.—F. B. Tuthett, Newport, Isle of Wight.—G. H. Whimper, Tillingham.—W. Griffin, Deddington.—J, Snow, York. — J. Jones, Carrick-on-Suir. — H. Snowden, Hull.—R. Jones, Denbigh, North Wales.—H. Lonsdale, Carlisle. — G. Allender, Londonderry. — W. Shuttleworth, Manchester.—W. Symonds, Ilminster, Somerset.—J. Larcombe, Ilminster, Somerset.—F. Fogarty, Drogheda.—W. Hiron, Campden. — F. W. Pittock, Elham, Kent.—J. Hitchman, Fairfield, Gloucestershire.—C. Braddon, Crediton.—P. Whitcombe, Cleobury Mortimer.—T. B. Barrett, Shrewsbury.—W. H. Campbell Sherborne, Dorset.—T. P. Vawdrey, Camborne.—R. P. Irwin, Dublin —R. B. S. Bowker, Appleby, Derbyshire.—C. Tayler, Coulston.—J. F. Steinhaeuser, London. — R. Geere, Newhaven. — S. R. Kay, Bolton-le-Moors. — W. Sturton, Greenwich.—G. B. Childs, Liskeard.—W. R. Cooke, Northampton.—R. H. Bowness, Poulton.—R. Hole, Wivelescombe. — O. M. P. Clayton, Percy Street.—E. Hare, Stanhoe, Norfolk.—S. S. Stanley, London.—J. S. Bartrum, Bath.—C. M. S. Babington, Woburn Square.—T. P. Tarrant, Dean Street, Soho.—W. G. Atherstone, Cape of Good Hope.—J. W. Swinburn, Liverpool. — D. Davies, Merthyr Tydvil. — W. Lyon, Sherborne.—W. N. Richards, Bath —W. Duckworth, Addingham, Yorkshire. — W. H. Rogers, Shrewsbury. — C. F. Jenkins. Thames Ditton. — W. J. Lunn, Sculcoates, Hull. — E. Newton, Hexham.—J. E. Mathias, Sandbach.—H. A. Cleaver, Camberwell.—W. Wilson, Allringham .— W. Taylor, Mortlach, Bamfd.—R. V. Shuter, Bengal. — J. Christie, Aberdeen. — A. Crabb, London. —A. O'Mullane, Buttesvant, Cork. — W. Blakeley, Kington, Herefordshire. — J. D. George, Romsey, Hants. — G. E. L. Bampton, Plymouth. — A. Ball, Horncastle.—W. F. Franks, London.—C. B. Parkinson, Northampton. —T. Wooks, Parson's Town, King's Co.—C. Sawer, Ipswich.—C. L. Cox, Kildare. —R. B. Colhoun, Letter Kenney, Donegal. —J. W. Lowry, Raphoe, Donegal. — T. Davis, Manahamilton. — C. F. Robinson, London.—B. Townson, Liverpool.—D. Lloyd, London.—S. Briesley, Ashton-under-Line.—C. F. Watts, London.—J. Russell, Thurles, Tipperary.—E. M. Prynn, R. N.—J. Stephens.—J. Tomlinson, Leeds.—W. Ward, Kendug, E. I.—E. Ethelston, Cheetham Hill, near Manchester.—W. R. Bayes, London.—C. W. Pickering, Pallemcotta, E. I.—G. Mottley, Portsmouth. —M. D. Thompson, Staleybridge.—C. G. J. Wasbrough, Bristol.—C. J. Martyr, Greenwich.—J. W. Todd, Woburn.—S. W. Shaw, Belfast.—R. Matthews, Norwich,— J. Anningson, Partington, Yorkshire.—A. Martin, Edgbaston.—H. Pennington, Liverpool. —J. Cook, Clydach, near Swansea. — H. Cotes, London.—E. Jones, London.—R. Hickson Dingle, Kerry.—G. Williams, Tralee, Kerry.—R. Turnock, Leek.—J. M. Burton, Rochester.— R. Frilshaw, Kirby Mallary, Leicestershire. — D. Lucas, A. — J. H. Gibson, Hull. — J. C. T. Nicoll, Worcester. — H. J. Carter, Budleigh Salterton.—T. Wetherhead, Kentish Town.—T. Lewis, Carmarthen. — G. Bond, Yarmouth. — J. S. Charlton, Rochester.—J. P. Fitzgerald, Rathmines, Dublin. — J. Brunton, West Bromwich.—A. R. Smith, Chertsey. — D. Burrow, Prescott.—W. M'Dermott, Newton Forbes, Co. Longford.—F. Smyth Bonington, West Meath. — Henry Howard, Tullamore, King's County.

* See my remarks upon this subject in the second and third volumes of the *Transactions.*

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *July* 90, 1838.

Age and Debility	23	Heart, diseased	1
Apoplexy	4	Hooping Cough	4
Asthma	4	Inflammation	5
Childbirth	1	Bowels & Stomach	5
Consumption	36	Brain	2
Constipation of the Bowels	1	Lungs and Pleura	3
Convulsions	20	Insanity	1
Dentition or Teething	3	Liver, diseased	1
Dropsy	8	Mortification	2
Dropsy in the Brain	8	Paralysis	5
Epilepsy	1	Small-pox	24
Erysipelas	1	Thrush	1
Fever	12	Tumor	1
Fever, Scarlet	5	Unknown Causes	44
Fever, Typhus	10	Casualties	4

Decrease of Burials, as compared with the preceding week . . . 31

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BEING A

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OF

Medicine and the Collateral Sciences.

SATURDAY, JULY 28, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton-Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXIV.

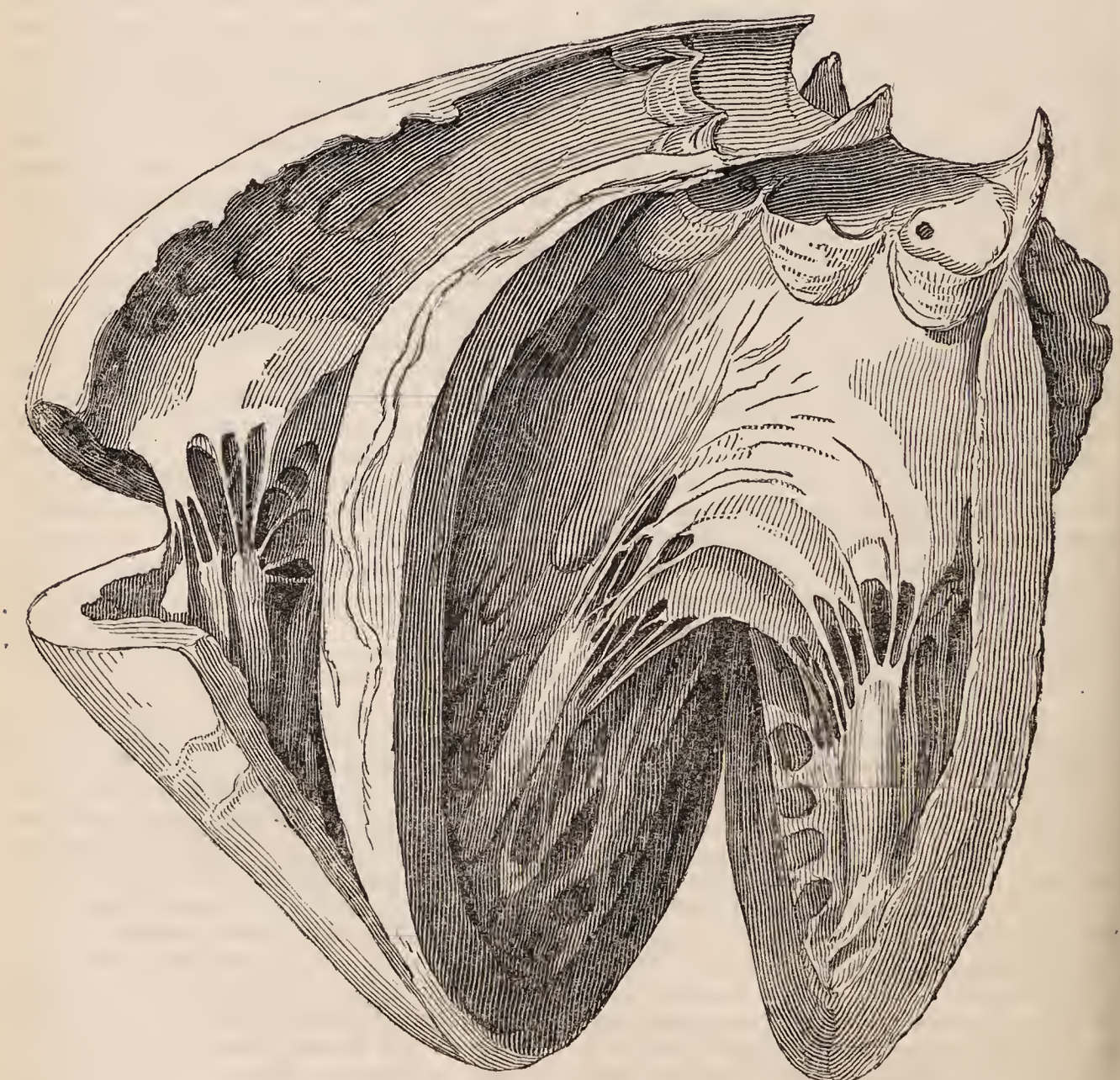
On the Heart, *its Structure and Mechanism.—Its Position.—Its Vital Properties and Motions.—Physical Examination of the Heart.—By Feeling, Impulse, Varieties; by Percussion; by Auscultation.—Description of the Sounds of the Heart.—Causes of the Sounds.—First Sound.—Second Sound.*

Having endeavoured to give you a connected and intelligible view of the physiology and diseases of the organs of respiration, I shall now, as fully as our time will permit, explain to you in the same manner the natural and diseased properties of the other organ of the chest—the *heart, and its appendages.* Four or five years ago I could not have conscientiously undertaken this task, for I found so much in the phenomena of this organ, in health and disease, inexplicable and inconsistent with the views and descriptions of even the most recent writers, that the subject was on every side beset with difficulties and obscurities, which defied all attempts to exhibit it in a rational manner. Since that time, however, it has been investigated extensively in different quarters; and having myself devoted much of my time to it, I think that the physiology and pathology of the heart now admit of the same rational and connected exposition which we have attempted with regard to the other organs of the chest.

The heart, you know, is a compound hollow muscular organ, consisting of its four compartments, lined inside with a serous membrane, and enveloped outside by a proper fibrous capsule, besides a serous covering, which is a part of the pleura, whilst another fold of the same membrane forms the external pericardium or sac; this has also beneath it a fibrous layer, which seems to be continuous with the cervical fascia. Now, that we may understand how the heart contracts, let us see what is the origin and insertion of its fibres. It is not difficult to trace these after boiling the heart for a long time, which dissolves the gelatine of the connecting cellular membrane, whilst it hardens the fibres. By this means it has been ascertained that the greater number of the fleshy bundles arise from, and are inserted into, the strong fibrous rings which form the mouths of the great arteries, and similar rings which form the auriculo-ventricular openings, or into tendinous prolongations from them. Some of these run obliquely downwards, and around both ventricles, and are inserted into opposite parts of the same rings. Others pass around the left ventricle only; and the outermost and longest run obliquely towards and around the apex, and then passing into it form the fleshy lacework of the interior, and the columns to which the tendinous cords are attached. There are a considerable number of fibres, especially near the base of the heart, which encircle the ventricles, especially the left, without any definite origin or termination; these, by their contraction, tend more directly to diminish the transverse circumference of the ventricles, as the longer fibres diminish their length by drawing the apex towards the

base. The auricles are formed more simply of loops variously crossing each other attached chiefly to the tendinous rings which open into the ventricles. The contraction, then, of all these muscular fibres, is towards the auricular and arterial orifices, which are the most fixed parts of the organ, and the effect of the contraction must be to press the contents of the cavities towards these openings. And it is thus that we see the heart act, on opening the chest in animals that have been deprived of sensation by a narcotic poison, or by a severe injury to the nervous system. Attached by the vessels at its base, and with its apex free, it is drawn together towards these vessels at each contraction; and the anterior surface being more convex, from the fibres being much longer than those behind, their contraction is greater, and the apex is also drawn forwards as well as upwards.

Now let us glance at the valvular mechanism of the heart, by means of which its contractions propel the blood in a determinate course. In order to shew this well, we must be careful how we cut open the heart, or we may injure the apparatus, and render its mode of action indistinct. [See the engraving.] I first make an incision in the left ventricle, from the apex close to the anterior groove, which marks the position of the septum, to the mouth of the aorta, taking care to cut between and not across the semilunar valves. This incision displays the arterial valves, and the arterial portion of the ventricle. But to see the auricular portion and mitral valve, I cut again from the apex, at right angles with the first incision or with the plane of the septum, up to the auricular ring; and thus you have both laminæ of the mitral valve fully displayed *in sitû*, without severing one of its tendinous cords. It is well to remember this mode of opening the heart; for if it is useful in shewing the natural structure, it is especially so in tracing the changes of disease in it; and I have often seen lesions overlooked, and fine morbid specimens destroyed, in consequence of the heart being opened at random.



The office of the semilunar valves is most obvious, from their mere mechanical structure. Attached by the whole of their convex ventricular margins, they fall loose and unresisting against the sides of the arteries at each gush from the ventricles; but no sooner does this gush cease, and the distended arterial column press backwards, than their loose arterial margins are caught by the first turn of the refluent current, and they are distended into three sacs, the free sides of which being in close contact, completely intercept the passage of blood back into the ventricles. This action is merely mechanical, and can be produced in the dead body; it will be more perfect in proportion as the backward pressure from the arteries is greater. The auriculo-ventricular valves, on the other hand, will not act well after death: their office depends on the vital contraction of the fleshy columns, to which their cords are attached, as well as on the mechanical spread of their laminæ. You see that these muscular pillars draw down both laminæ of the valve, but they cannot bring them together with any force so as to close them. This is done by the lateral pressure of the blood, which, directed by the contracting ventricle on both laminæ, closes them one against the other, and effectually prevents regurgitation into the auricle.

The pulmonary or arterial portion of the right ventricle may be displayed in the same way as the corresponding portion of the left; but the different position of the tricuspid requires that the second incision should be made from the middle of the other cut [see engraving] instead of from the apex, and carried round to the posterior grove. You here see the three irregular triangular curtains of the tricuspid valve, which are drawn in succession one somewhat behind its neighbour, but all more across than in the axis of the ventricle, as in the case of the mitral, so that the valves close the orifice more in the manner of the semilunar valves, but still under the varying vital influence of the fleshy columns, which, by contracting more or less, may complete, or leave imperfect, the closure of the valves. Hence when the right ventricle is much distended, the curtains of the tricuspid valve do not entirely reach across the orifice, and regurgitation takes place. This seems to be a provision against an excess of pressure on the pulmonary vessels, and induced Mr. Adams, of Dublin, first to call this valve a safty-valve. The subject has been lately well illustrated by Mr T. King.

The auricles may be opened by a long crucial incision, which displays their interior, their ventricular and venous orifices, and their septum, in which is the semilunar groove of the foramen ovale. There are some other points in the anatomy of the heart that are worth adverting to; such as the roughness of the auricular and the smoothness of the arterial portions of the ventricles, and the remarkable adaptation of the structure for the transmission of the blood in one direction. We have no time to dwell on these; but I must particularly commend to your attention the anatomy of the human heart. It is an organ easily examined, and by attention to the directions above given, you may soon acquire such a familiarity with its common appearances, as may better qualify you to appreciate the change induced by disease, and without this familiarity no description, however minute, can suffice.

The heart, you know, is placed in the anterior mediastinum, rather to the left of the mesial line, and so oblique that the apex points forwards and downwards to the left, while the base lies back nearer the posterior centre, the spine. It therefore lies with its point on the diaphragm, underneath which are the liver and stomach; and it is bounded on other sides by the lung, except a small space of about two square inches, where, enveloped in its coverings, it is in contact with the walls of the chest.

Little need be said here about the *vital properties* of the heart. Its contractions are essentially periodic and involuntary, and perhaps independent of the nervous system; but this is a subject much debated: they are certainly, however, liable to be influenced in a variety of ways, both by the nervous system and by the blood. Any sudden impression on the nervous system may stop the heart's action, or it may accelerate it. Thus, crushing the brain or spinal marrow will stop it, and smaller injuries may quicken it; but both the brain and spinal marrow may be removed without either of these effects. In fact, the influence of these injuries to the nervous system does not appear to be different from that of injuries to other considerable parts of the body: thus, Dr. M. Hall found that crushing a limb had the same effect of arresting for a time the heart's action. I do not think that we should attach much importance to such difficult and equivocal experiments as those recently made by M. Brachet, who describes the heart's action to cease instantly on cutting the great cardiac plexus. If you only recollect the anatomical position of this plexus, and how impossible it is to reach it in a living animal without extraordinary disturbance of many vital parts, you may well question the conclusiveness of a result so obtained.

The influence of the blood in exciting

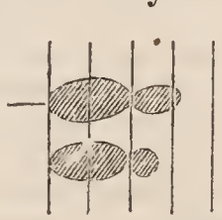
the contractions of the heart is more general than that of the nervous system, and there can be no doubt that it is the proper stimulus of this organ, exciting it by both its quantity and quality as it fills its cavities. At the same time we must not forget that the rhythmical contractions of the heart will continue for a short time when it contains no blood, when it is taken out of the body; and if we call to mind, besides, the numberless instances of disease in which the heart's action may be permanently increased both in force and frequency by causes which do not affect the quantity or quality of the blood, we must admit that there is some cause of the property and its varieties, independent of the blood which is its ordinary excitant. Whether this be a *vis insita* or a *vis nervosa* cannot, in the present of our knowledge, be determined.

Let us now see what we can learn by the physical exploration of the *heart in action;* and first, by the sense of *feeling.* On applying the hand to the left front of the chest, between the fourth and sixth ribs, the pulsations of the heart may be distinctly felt; but they vary remarkably both in strength and extent, according to the stage of the respiratory act and the position of the body, as well as from differences in their own strength. In a well-formed chest, the impulse of a healthy heart, is scarcely, if at all, perceptible when the individual takes a full breath, or lies on his back. On making a full expiration, on the other hand, or on stooping forward, especially to the left side, it becomes strong, and is spread over a considerable surface, being felt much higher than usual. You see, then, that the position of the heart is not fixed in relation to the walls of the chest, but that it hangs to a certain degree loose, and liable to displacement by change of posture and by the motions of the chest. The common position, however, is such that only the apex is felt beating at a very circumscribed spot between the cartilages of the fifth and sixth ribs; in males, commonly about two inches below, and in front of, the left mammilla, and about the same distance from the left margin of the sternum. A deep inspiration elevates the ribs without raising the heart in the same degree; hence it makes the apex beat below the sixth rib; the impulse is then, however, diffused, and scarcely perceptible, because the chest expands as the ribs rise, leaving the heart, and drawing the porous lung in front of it. A forced expiration, on the other hand, depresses the ribs, and transfers the strongest pulsation to between the fourth and fifth ribs, and by bringing down the walls into contact with more of the heart, makes its impulse perceptible over an extended space, as high as the third rib and on the lower half of the sternum. All these circumstances, although wholly unnoticed by Laennec and other writers, are of great importance, and should be known, not only to guard us against undue comparisons, but also to furnish us with the means of testing the free state of the heart and lungs, and of bringing the power more within reach of our examinations. Whilst you feel for the impulse of the heart, you should therefore desire the patient to vary his posture, by leaning forward on your hand, and first to take in and then to give out a full breath. The natural impulse will, for the same reasons, vary according to the form of the chest and the extent to which the lungs are developed in front of the heart. In narrow or distorted chests, and in those contracted after pleurisy, the impulse of the heart may be felt much more extensively than usual. So also circumstances displacing the heart, such as tumors, and effusions of liquid or air into the pleura, may greatly change the character and degree of the impulse, diminishing or increasing it according to whether the displacement of the organ is from or to the walls of the chest. Abdominal tumors, and even a distended stomach, may to a certain degree have a similar effect. You are to remember also that the impulse or perceptible motion of the heart is naturally intercepted or circumscribed by the soft porous tissue of the air-filled lung, and that it may be therefore modified by changes in that tissue. Hence pulmonary emphysema may still further intercept or circumscribe the impulse: consolidation of the lung, on the other hand, will propagate it over an extended space. The greater thickness and strength of the left ventricle make its motions more forcible and extensive than those of the right; and it is perhaps for this reason that this ventricle is placed to act chiefly on the soft cushion of the lung, which offers no unpleasant resistance to it. You see by the position of the heart that the apex is almost the only part of the left ventricle that comes far enough forward to strike the walls of the chest. The front is constituted by the right ventricle, which lies under and even a little to the left of the sternum, and it is chiefly the impulse of this ventricle that is felt there on a full expiration, or on leaning forwards. It is by the comparative degree of impulse to be felt there, and further to the left, where the apex strikes, that Dr. Chambers chiefly judges of the relative condition of the two sides of the heart.

The examination of the region of the heart by *percussion* should be immediately added to that by feeling, and will be found

to afford satisfactory illustrations of its results. In a well-constituted chest of a person who is not fat, there is commonly some dulness on percussion from the left margin of the sternum to the extent of between one and two square inches towards where the impulse is felt; but this dulness is remarkably diminished, if not quite removed, by a leaning back or supine posture, and by taking a full inspiration; and it is as notably increased by leaning forwards and to the left, and by a forced expiration. This is, then, another method of testing the free condition of the heart and of the lungs that involve it. For example, if such variations are not observed—if in common states of respiration, and especially if on full inspiration, or on leaning back, there is a considerable extent of dulness on percussion at and to the left of the sternum—it may be concluded either that the heart, from adhesions or from its bulk, cannot recede from the walls of the chest, in which case the impulse would also be increased; or, that there is considerable effusion in the pericardium, in which case the impulse would be diminished. But we shall come to these applications afterwards; I wish you now to understand the principles on which this mode of examination indicates the condition of the heart. To define the limits of the heart by percussion, we have to attend, not only to the *perfect dulness*, which exists where the organ is in absolute contact with the walls of the chest, and which, in disease, sometimes extends over an area four or five inches in diameter; but we must notice also the *shading off* of this dulness, where the lung overlaps the organ. To discover this we must use forcible mediate percussion, which gives the sound of the deeper-seated contents. By this means we may sometimes trace an enlarged heart when very little of it comes in contact with the walls of the chest. In emphysema of the lungs the permanently distended state of the anterior lobes nearly intercepts all impulse, and makes the region of the heart quite resonant; but strong percussion may still indicate that there is an enlarged dense organ beneath this resonant surface. In considerable enlargement, the sound of strong percussion on the left back, and in the left lateral region, is also somewhat impaired.

We now come to consider the examination of the *acting heart* by the sense of *hearing*. I shall first describe to you the chief facts of the subject, and afterwards enter into an explanation of those facts. If you listen either with the stethoscope or with the ear alone applied to the region of the heart, you hear at each pulse two sounds following each other in quick but regular succession, and succeeded by an interval of silence until the next pulsation. The first is a long, rather dull sound; the second is a short abrupt flap. Some French writers have used a very wrong sounding word for the double sound, by calling it a *tic-tac*. If it is to be given at all by the symbols of articulate sounds, the word *lubb dŭp* will best express it. If we would only be a little more consistent in our rules of pronunciation, I think we might often give the sounds of the heart, healthy and morbid, in language of this kind, with more ease and precision than by descriptions or comparisons. It is a very convenient mode of expression in taking notes of cases, and I probably shall have to use it in describing some of the morbid sounds. Laennec endeavoured to give expression to these sounds, by marking their rhythm or measure. Supposing the period of a pulse (that is, the time from the commencement of one double sound to the commencement of another) to be divided into four equal parts, he described two of these as occupied by the first sound, a little more than one by the second sound, and the remaining less than one by the interval of silence. These sounds present much variety, so that it is difficult to estimate their proper length; but I should note the average rhythm somewhat differently from that given by Laennec, who makes the second sound longer and the interval shorter than what I have generally heard. I will represent the measures by a diagram, which will be more generally intelligible than musical notation. The uppermost is that given by Laennec, the other is what appears to me to be the more usual rhythm in health.



This is the general character of the sounds in the healthy male adult. In females and in children the first sound is rather shorter and less dull, more like the second; and the same difference may be observed when the pulsations of the heart are frequent or weak.

The physical causes of these natural sounds of the heart have been the subject of much discussion. Laennec's opinion was, that the first sound was caused by the contraction of the ventricles, and the second by the contraction of the auricles; but he did not advance any proofs in support of this opinion, nor did he attempt to explain how these respective contractions could generate the sounds. The late Professor Turner, of Edinburgh, was the

first to test the matter by physiological experiment; and he was thus led at once to detect the error of Laennec, by observing (what had been before noticed by Harvey and Haller) that the contraction of the auricles immediately precedes that of the ventricles, and therefore cannot cause the second sound. This observation of Professor Turner was afterwards confirmed by the experiments of Dr. Hope, in which it was proved that the first sound distinctly accompanies the contraction of the ventricles, and that the second sound occurs at the moment of their diastole, and is in no way connected with the motions of the auricles, which appear to be too slight to cause any sound. But the immediate physical causes of both sounds were undetermined until February 1835, when, assisted by Dr. Hope and others, I conducted a course of experiments in the room below, which led to pretty conclusive and satisfactory results. These experiments were afterwards repeated and varied by the Dublin Committee of the British Association for the Advancement of Science; and more recently an extended series of experimental researches on the same subject has been conducted by Drs. Clendinning, Todd, and myself, in another Committee for the same Association. You may find the different reports of these investigations in the Medical Gazette; they have pretty thoroughly sifted the subject, and it is satisfactory to me to be able to say that they confirm, in all material points, the results and conclusions of my first experiments, which you will see fully given in the third edition of my little work on the Pathology and Diagnosis of Diseases of the Chest. We have not time to describe these experiments, nor indeed is it necessary. If we consider the structure and action of the heart, we shall be rationally led to perceive how it may produce the sounds, and I will merely adduce the experiments to guide and answer the questions which arise from this examination of the acting mechanism.

It was decided unequivocally, by all the series of experiments alluded to, that the first sound accompanies the whole duration of the systole of the ventricles, which also causes the impulse felt on the walls of the chest; it was equally clear that the second, or flapping sound, occurs at the first moment of the diastole, and that the motion of the auricles was not accompanied by any perceptible sound.

Then, first, how does the contraction of the ventricles produce the first sound? Sound we formerly defined to be *motion of a certain force resisted with a certain force*: where is the resisted motion in the contraction of the ventricles? One of the first ideas that suggest themselves is that of Magendie: does not the heart produce the impulse by striking against the walls of the chest? and why should this not cause sound? I answer, that in forcible pulsations, and when the lung does not intervene, I have no doubt that the impulse does produce sound; and if you listen to the sound of the heart when it is beating strongly, or when, by leaning forward or by breathing out, the heart is brought in contact with the walls of the chest, you will hear the first sound has something like a knock in it, which you can scarcely help referring to the impulse. But this is an accessory, and not an essential sound; for you may hear the first sound when there is no impulse, as in a person leaning backwards, or taking a full breath; and in our experiments we heard the first sound at the origin of the arteries when the body of the heart was surrounded with the soft lung or with tow, or was allowed to hang out of the chest, and strike against nothing in its motions. It is well known, too, that liquid in the pericardium, or liquid or air in the pleura, although it entirely prevent the heart from striking the walls of the chest, does not annul the first sound. In moderate pulsations the heart makes a partial rotatory movement; the long fibres of its anterior convex surface drawing the apex upwards and forwards, and causing it to slide obliquely on the smooth pericardium, bring it to the walls of the chest too gently to produce sound, except under the circumstances before mentioned.

The first sound is, then, produced by something in the heart itself, either by its contents or by its own structure. Can it be by its contents? by the motion of the blood resisted by the inequalities of the interior of the ventricles? This was supposed by the Dublin Committee to be the chief cause of the sound. But observe, that the motion of the blood over these inequalities is not rapid, nor is their resistance considerable at the time of the production of the sound. These inequalities are, as Dr. Cowan has pointed out, chiefly confined to the lower or auricular portions of the ventricle, into which the blood has already passed before the systole begins; and the effect of the systole is to drive the blood, not over or across the inequalities, but from them, to those smooth and funnel-shaped parts of the ventricle which lead to the arteries, and which offer the least possible resistance to its course. The contraction of the ventricles propels the passive mass of blood gradually, that is, during the whole period of the first sound, into the arteries; and the motion is therefore too slow and easy to be likely to cause sound. Some of our experiments further settled this point, by

proving that the first sound continued when the ventricles contracted without any blood in them.

By excluding the blood we are thus brought to the conclusion that the cause of the sound must be in the solid structure of the ventricles: it is our next question whether it be in any part of them in particular. Several writers have ascribed it to the auriculo-ventricular valves, which, when they close, are supposed to produce a flapping sound. But the act of closing these valves is momentary, and takes place only at the commencement of the systole; whereas the first sound of the heart is prolonged through its whole duration. Further, in some of our experiments the first sound continued, although impaired, when the auriculo ventricular valves were prevented from acting, by fingers introduced into their orifices, or by some of their cords being cut. Still these valves may produce a part of the sound, for at each contraction they are suddenly tightened in a manner calculated to generate sound.

But are the valves the only parts which are tightened at each systole of the ventricles? Is not every muscular fibre in the ventricles suddenly tightened by this action? Here are the elements of sound, motion vigorous and rapid, suddenly resisted by the mass of blood to be urged forwards by the contraction, and the contracting motion and the resistance, although greatest at first, continue to act as vibrating forces during the whole systole; hence the prolongation of the sound. In other instances, abrupt and forcible muscular contraction produces a sound like the first sound of the heart. Apply the stethoscope to the adductor muscle of the thumb of your closed hand, and contract the muscle strongly and quickly. Or, to avoid the possibility of the joints being the seat of the sound, apply the end of a flexible tube to the abdominal muscles, and start them into sudden vigorous action: you will get sounds quite as loud as those of the ventricles, and very like them in character. By varying the mode of muscular action you may get different kinds of sound. When the contraction is slow or sustained, however strong, you get only the dull rumbling noise which Dr. Wollaston first described, and which he attributed to a vibration depending on a regular intermittence in the force of the contraction. When the contraction is gentle and slow it may cause no sound at all; as we have seen that the auricles produce no sound, neither do the ventricles, when their contraction is very feeble. But whenever there is strong abrupt muscular action in any part of the body, like that of the heart, there will be heard a sound which will resemble that of the ventricular systole, in proportion as the muscles in which it is produced resemble in thickness and density the tissue of the heart. The loudness of the sound is by no means in proportion to the thickness or strength of the muscle, but rather to its simplicity, and the abruptness as well as the vigour of its contractions: the transition of a thick muscle from slack to tight can never be so complete and sudden as that of a thin one; where there are many fibres they choke or muffle each other's vibrations; hence the sound is dull and prolonged rather than loud and clear. Many writers who have objected to my explanation of the first sound of the heart, have done so in ignorance of the principles on which muscular action causes sound; when these are known the identity of the phenomena becomes apparent; and in my experiments there was the best proof that we could have that the muscular contraction of the heart produced systolic sound, for we had the heart out of the body, without its blood, without valvular action, lying on the table, or on my hand, and its contractions were still accompanied with a sound, weak indeed, but in character resembling its natural first sound.

The walls of the ventricles appear to be peculiarly calculated to generate sound; their flaccid state when relaxed, the fineness of their fibres, and the harmony with which they suddenly contract on their contents, and become almost as hard as a stone (as we can feel in the living heart of a stunned animal) fulfil the conditions best calculated for the production of sound. The commencement of the systole, producing the tightening of the auricular valves, and thereby completing the resistance of the body of blood on which the contracting fibres have to act, is naturally its loudest part, and often has a flapping character; that which continues after is more dull, and is prolonged according to the quantity of blood to be expelled, and the continued strength of the contraction. This prolonged termination of the sound is, therefore, best heard when the heart acts slowly and vigorously.

And now what causes the second sound? That it is intrinsic, and not caused, as Magendie supposed, by the heart striking any of the surrounding parts, we proved by the same experiments, in which the intrinsic character of the first sound was shown: the sound continued when the heart was so completely isolated that it could strike against nothing. What is there, then, within the heart that can produce this short flapping sound at the moment of the diastole? Is there any thing that tightens at that moment? Not the walls of the ventricles certainly, nor the

aurieular valves, for they are then all loose and flaccid. What can it be but the semilunar valves at the mouths of the arteries, which are then suddenly tightened by the reaction of the arterial column of blood? And so it was proved to be, in my experiments first, and in many repeated since; for by hooking back these valves, or by pressure preventing the reaction of the column of blood upon them, the sound was stopped; and by releasing the valves, or discontinuing the pressure, the sound was as surely restored*.

So much for the causes of the natural sounds. We shall consider their variations in the next lecture.

LECTURES ON BLOOD-LETTING;

Delivered from time to time,
At the General Dispensary, Aldersgate Street.

By Henry Clutterbuck, M.D.
Formerly one of the Physicians to that Instituion

Lecture X.

On the Treatment of periodical Asthma.

Before going into particulars on this branch of our subject, it will be desirable to inquire how far the term spasm is really applicable to affections of the respiratory organs; or, in other words, to what extent the respiratory muscles are concerned in certain cases of dyspnœa; for it is to muscular structures only that spasm can be referred.

The only muscles found in the course of the air-tube are those of the larynx; but these, as before observed, have no share in producing genuine asthma. It has been conjectured, indeed, by some—I should not say proved—that the back part of the trachea that is placed between the extremities of the cartilaginous rings, and which has generally been considered as ligamentous merely, is in reality of a fibrous or muscular structure, and, consequently, that spasmodic contraction of these fibres may be a cause of periodical asthma. To this it may be replied, that not only has a muscular structure, as here supposed, not been satisfactorily shewn to exist, but contraction of the tube is particularly guarded againt by the cartilaginous rings themselves; a structure that is gradually changed into ligamentous, as the tube divides and subdivides. Where, therefore, muscular contractility could serve no useful purpose, as far as we can judge, but where, on the contrary, it could only be exerted to the detriment of the function concerned, it seems unreasonable to infer the existence of spasm at this part, in order to account for the asthmatic paroxysm.

But while I contend for catarrh being the true origin of asthma, I am not disposed to deny altogether the participation, to a certain extent, of the muscles of respiration, or rather I should say of the diaphragm, in producing the phenomena of the disease; whether the intercostal muscles have any share in the matter, we seem to be wholly ignorant.

Now the influence of the diaphragm, when morbidly affected, on respiration, is sufficiently clear, whether we appeal to actual experiment, or to natural phenomena, in proof. The diaphragm is a part possessed of great muscular power, and, though it acts quietly and imperceptibly in health, is yet capable of being convulsively affected, as in hiccup; or of being thrown into a state of fixed spasm, as appears to be the case in tetanus, as indicated by the painful constriction experienced across the body in the course of this muscle. It is liable also to paralysis; as observed in the experiment of tying the phrenic nerves in animals, which puts a stop to respiration, as far as it depends upon the diaphragm. This muscle is likewise under the immediate influence of the brain, so as to be readily disturbed in its actions, not only by mechanical injuries and diseases of this organ, but by mental excitement also. Thus, certain emotions of mind produce convulsive movements of the diaphragm—sobbing, as it is termed. The pain and sense of constriction felt in the course of the diaphragm during severe fits of asthma, lead one to suspect, therefore, that the muscle is in a state of fixed contraction or spasm, similar to what takes place in tetanus; while the laborious efforts that are made, in these cases, to elevate the ribs, seem to shew that, in this direction only, can the capacity of the chest at such times be enlarged. The relief that is occasionally, though by no means constantly, experienced from the use of opiates and antispasmodics in these cases, affords some corroboration of the opinion of the spasmodic nature of the paroxysm, to the extent specified. Admitting this, however, the spasm is only to be considered as an accessory symptom, and by no means disproves the general truth of the position I have contended for, namely, that the disease altogether originates in catarrh; in other words, in in-

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* According to M. Bouillaud, Dr. Carswell was the first who conceived that the second sound is caused by the flapping of the semilunar valves; but I cannot find that the opinion was made public. The first proposal of this explanation that I know of in print, is in a thesis by Dr. Elliott (De Corde), Edinburgh 1831. M. Rouanet, Dr. Billing, and Mr. Carlile, were later advocates of this view.

flammation of the mucous membrane lining the windpipe and its branches; the symptoms varying according to the particular part of the canal that is the immediate seat of the disease. Thus, stridulous coughing, hoarseness, and, sometimes, an entire loss of voice, sufficiently indicate the larynx (the organ of voice) to be the seat of disease. While cough simply, without difficulty of breathing, and without alteration of the voice, shews the trachea only to be affected.

On the other hand, slow and laborious breathing, with wheezing, such as we observe in the paroxysms of ordinary asthma, shews an impediment, not an entire obstruction, to the entrance of air into the lungs, and which appears to arise from a narrowing of the passages, but which, probably, does not extend to the minuter branches, or their terminations in the air-cells; for the air does, to all appearance, reach the lungs in this case, though slowly, and with great effort on the part of the muscles which elevate the ribs. The manner of breathing, in these cases, might be not unaptly compared with what takes place when an attempt is made to expand a pair of bellows, at the time that the nozzle is nearly closed: the whole weight of the superincumbent atmosphere has to be overcome, before the object can be accomplished.

There is still another variety of this affection, in which respiration is short and frequent, without pain, but attended with an intolerable sense of oppression at the chest, and which, like the former, often assumes a periodical character. In this case the immediate seat of the disease appears to be the smallest branches of the air-tube, and the air-cells themselves, which are in a state of temporary obstruction, so as not to admit of the entrance of the air into them. I quote the following as an example:—

A gentleman, 65 years of age, robust in appearance, and having enjoyed excellent health through life, has for some months past suffered occasional attacks of difficult breathing. These generally come on towards night, and often during his first sleep; when he awakes and is obliged to get out of bed, and to sit up during the remainder of the night. He suffers little or no pain at the time, but an intolerable sense of extreme oppression, so as to make him dread immediate dissolution. Yet, to a by-stander, he does not appear to suffer much. This paroxysm lasts some hours, and subsides gradually, with expectoration. The tongue is coated white; pulse rather frequent, between 90 and 100; not full, but tolerably strong. This variety has been called spasmodic; but with as little reason as the former. It appears to be a catarrhal affection, occupying the air-cells and minuter ramifications of the air-tube, by which the air is, in a great measure, prevented from entering the lungs.

Having convinced myself, by previous trials, of the total inefficiency, nay more, of the bad effect, of what are called antispasmodic remedies, I resorted with confidence to as active an antiphlogistic course as the circumstances of the case would warrant. On several recurrences of the disease, two or three bleedings to the extent of 10 or 12 ounces have sufficed to subdue the inflammation, which has commonly ended by a copious expectoration, as in ordinary cases of pulmonic inflammation. The blood drawn exhibited the usual inflammatory characters.

Having thus given you my reasons for thinking that what is called *spasmodic* asthma is, in most instances, founded originally in inflammation of the mucous membrane, it remains for me to point out what practical advantage is derivable from such a supposition, and especially the benefit, if any, that blood-letting is capable of affording in such cases. Now, to do this with effect, it will be useful to distinguish between the paroxysms and the intervals, just as we do in regard to epilepsy. There is, indeed, a remarkable analogy between asthma and epilepsy, both in the nature and in the treatment of those diseases, the seat of the disease constituting the chief difference between them. The object of treatment in both is two-fold: first, the immediate relief of the paroxysm; and, secondly, to prevent, if possible, its recurrence. The means of accomplishing these objects are nearly the same in both affections.

Upon the supposition that the paroxysm of asthma depends upon a spasmodic contraction of some undefined muscle or muscles, various remedies termed antispasmodic, such as the whole tribe of stimulants and narcotics, have been resorted to. The advantage derived from any of these, however, is so very equivocal, that I am not inclined to dwell upon them. The paroxysms are of limited, though unequal, duration; and, in almost every instance, will subside if left to themselves. You see, therefore, that it is very easy to be deceived in regard to the supposed effect of remedies in such cases. With regard to opiates I would observe, that although in some cases they appear to have afforded relief, I have in a hundred instances heard patients express themselves as injured rather than benefited by their use. They appear to lessen the power of the voluntary muscles, at the very moment when the greatest exertion is required from them, in order to expand the chest; and they

have the further great disadvantage of preventing expectoration, and thus of prolonging the paroxysm.

Considering, on the other hand, the disease altogether as originating in inflammation of the mucous membrane, it might be supposed that bleeding would be the most effectual remedy for the relief of the paroxysm, and so undoubtedly it sometimes is. If the disease be recent, the age of the patient not far advanced, and the habit tolerably strong, and if there be reason to believe, from the furred tongue, with other febrile symptoms, that inflammation still exist (which is frequently the case, especially when the disease appears to have originated in *taking cold*), a moderate bleeding is as decidedly useful as in any other inflammation. The immediate relief of the paroxysm is not, however, the sole, nor indeed the chief reason for the use of blood-letting under these circumstances; for the paroxysm will in almost every instance subside of itself after a few hours. The principal advantage derived from this source is, that by taking off the inflammation earlier, and more completely, than would otherwise be the case, the predisposition to a recurrence is prevented or lessened, as this predisposition appears to consist in the morbid irritability of the mucous membrane which inflammation is so well calculated to induce.

Other means of relieving the paroxysm of asthma are of no great importance. The producing nausea by antimony or ipecacuanha, or even by an infusion of tobacco, which I have known some patients resort to for the purpose, will sometimes give relief. Ammonia, and also æther, have occasionally been used, and with seeming advantage; but they have often done harm, and are at best equivocal.

The treatment in the intervals of the paroxysms is, as just observed, the most important, the object being to remove or lessen the disposition to recurrence, and which is best accomplished by lessening the irritability of the mucous membrane, as just observed. How far blood-letting may contribute to this, I noticed above. Continued counter-irritation, by blistering and the like, may be of some use; tonics, also, and cold bathing, where no febrile state is present—change of climate, mode of life, and occupation—have all been followed at times by apparent advantage; and it is not uncommon for asthma to cease gradually and wholly as life advances.

I may take this opportunity of adverting to the method of investigating diseases of the thorax by auscultation—that is, by listening attentively to the sounds emitted during respiration; and also by sounding the cavity, by tapping with the ends of the fingers on different parts of the chest. This mode of examination has always been resorted to more or less by physicians; though, from the employment of a load of new terms, invented chiefly by our ingenious neighbours the French, and introduced by some of our own practitioners who have enjoyed the advantages of the Parisian schools, one would be led to suppose that a new region of science had been discovered, not inferior to mesmerism or homœopathy. As a specimen of the new language introduced on the occasion, I may enumerate the following, indicating, it is supposed, as many various conditions of the organs in question. Thus, in the compass of a few pages you will meet with the following:—"Pectoriloquy, perfect and imperfect"—"bronchophony"—"pneumo-thorax"—"rhonchus"—"crepitation, fine and coarse"—"vocal resonance"—"tinkling echo"—"metallic tinkling"—"amphoric, or bottle-like sound"—"clicking"—"bubbling"—"gurgling"—"snuffling"—"whiffs of a cavernous respiration"—"fistular resonance, like that of a pan-pipe or key"—"pectoriloquy, forming a little island of voice"—*cum multis aliis*.

It is hardly necessary to say, with regard to this subject, that exploration of the chest by the ear, as well as by the eye, is a highly useful practice. Every cautious and attentive practitioner will, of course, listen to the sounds emitted during respiration, as well as observe the manner and degree in which the chest is expanded. He will thus ascertain with tolerable precision in what degree the lungs are pervious to air or otherwise, and in what particular part: whether a quantity of mucus lies loose and floating, as it were, in the air-tubes—what is the condition of the larynx, as judged of by the voice—and, by the tone of the cough, whether there is or not a great cavity in the lungs, the result of suppuration or ulceration. These, with many other valuable points of information, may be generally obtained by the unassisted ear; and certainly to the full extent that is required for the purposes of practice. If by the use of the ear-trumpet (or *stethoscope*, as it is less appropriately termed) the object investigated (the sound) is somewhat magnified, it is apt at the same time, as in the case of the telescope or the microscope, to become distorted, or less distinct than before, leading different observers (as, indeed, experience proves) to different and sometimes contradictory conclusions. The information thus acquired, too, supposing it to be correct, comes too late, in general, to be of any practical use. It serves to indicate the consequences of

disease rather than disease itself; and that at a period when they are far beyond the power of art to remedy.

Of the Use of Blood-letting in the Specific Inflammations of the Chest.

The *specific* inflammations that have their seats in this class of organs are, hooping-cough, measles, and influenza. Now blood-letting, though not in strictness possessed of curative powers in regard to either of these affections, is yet capable of rendering important services in all of them. In hooping-cough the inflammation is confined chiefly to the great barrel of the trachea, the lining membrane of which you will find highly reddened after death. This inflammation has a peculiar, and more or less protracted, course to run, which neither bleeding nor any other remedy, as far as I have observed, is able to arrest. It is to the casual and accompanying circumstances, therefore, that attention is chiefly due. Thus, inflammation frequently arises in the lungs, indicated by difficult respiration and other signs, which continue through the intervals of coughing. This seems to be of the nature of common inflammation, and is as readily relieved by blood-letting, and other ordinary means. On the other hand, the larynx sometimes becomes inflamed during the course of hooping-cough, and requires blood-letting for its relief. The brain, likewise, is apt to suffer from the violence of the cough in very young children; convulsions and actual inflammation of the brain arising. This, indeed, is the greatest danger attending hooping-cough in such subjects. Blood-letting, under the proper circumstances, and to the proper extent, is of essential service on such occasions. And I may add that it is powerful in preventing the occurrence of all these secondary affections, while it contributes to the mildness of the disease altogether.

The "measles" is a disease equally specific in its nature with hooping-cough; but produced by a virus of a different and peculiar kind, and governed by peculiar laws. Though incurable by art, it is nevertheless dependent for much of its mildness of character and ultimate safety upon the application of blood-letting. This disease, when the first febrile action has declined, frequently leaves behind it cough, pain in the chest, and other marks of pulmonic inflammation, which not unfrequently, if neglected, terminate in consumption. Small bleedings, from time to time, with other anti-inflammatory treatment, are of the greatest importance, and should be persevered in as long as any hope of attaining the object exists.

Epidemic catarrh, or *influenza*, may be properly ranked among the specific diseases of the chest, though it is to be considered a compound affection,—a combination of cerebral, with catarrh or pulmonic inflammation. The affection of the brain is manifest, in the headache, occasional delirium, restlessness, and great prostration of the voluntary power, which always exist in greater or less degree. In a great proportion of cases, the disease goes through its course so mildly, as to require nothing but time and rest, to ensure a safe termination. When, however, either the cerebral or pulmonary symptoms are unusually severe, antiphlogistic measures are required, and blood-letting among the rest; but always to a moderate amount, the object being palliation, not cure. The prostration of strength, attended, as it may be, with a weak and rapid pulse, forms no real objection even to blood-letting, provided it be resorted to very early in the disease, and properly proportioned to the general circumstances of the patient. The effect of the evacuation, so limited, is to produce an immediate abatement of all the symptoms, while the pulse lessens in frequency, and becomes fuller and stronger than before. I venture to state this confidently, from my experience during the late epidemic, as well as in several preceding ones. I hardly need add, I think, that the practice some have pursued, of giving stimulants, on account of the temporary depression that exists, is as unsupported by any just theory, as it is contrary to experience.

Of the Use of Blood-letting in Diseases of the Sanguiferous System.

This system of organs, comprising the heart, arteries, and veins, is, for obvious reasons, in an especial manner under the influence of blood-letting. The actions of these parts are readily weakened, disordered, or altogether suspended, by this agent; according to the manner in which it is administered, and the extent to which it is carried.

The sanguiferous system is very frequently in a state of preternatural excitement and disorder, not as a primary affection, but from sympathy with other organs. Inflammation, when either violent or extensive, and wherever seated, has this effect; and thus is produced pyrexia, or symptomatic fever, as before explained to you. This state, though in itself always secondary or symptomatic, may become a cause of inflammation in other organs: of which, a striking example is afforded in the case of ligamentous inflammation; as in acute rheumatism, where the general circulation is more excited than in any other disease. During the course of this, in-

flammation is apt to arise in the brain or lungs, and which constitutes, indeed, the greatest danger of rheumatic fever, as it is called. Such a state of system, also, gives a disposition to hæmorrhage. Blood-letting, therefore, by diminishing arterial action and the force of circulation, lessens the tendency to these secondary inflammations. When used for such a purpose, however, it only requires to be carried to a moderate extent; the object being not curative, but merely precautionary.

These organs, the heart and blood-vessels, like all others, are liable to inflammation, and that in all degrees. Inflammation of the heart and its investing membrane, (carditis) is the one with which we are the most familiar. Carditis may take place in all degrees, and under all circumstances; requiring, of course, that the treatment should be accommodated to these. This inflammation is not difficult of detection, if you bear in mind the physiological peculiarities of the organ, and the ordinary effects of inflammation on such a structure: pain or uneasiness in the part affected—a disordered state of its functions—with more or less of constitutional disturbance (pyrexia.) Acute carditis is less frequent than the milder forms of the disease. I have seen it brought on by running up hill, and by great muscular exertion of other kinds. The pain in this case is commonly acute, while the irritability of the organ is so much exalted, that it will not bear being distended, but contracts when the smallest quantity of blood is admitted into its cavities. The pulse consequently becomes extremely small and rapid, and generally, though not always, irregular also. In some instances that I have met with, the pulse was so small as to be felt with difficulty. This circumstance may lead to error, unless previously understood, by leading one to suppose that the patient is in a state of extreme weakness, approaching to syncope; we might therefore be deterred from using the lancet, where it could not safely be dispensed with. The existence of pain in the region of the heart, however, and the absence of other signs indicative of general weakness, will in most cases render the thing sufficiently clear. In such cases the pulse expands, and acquires additional strength, from bleeding. A tolerably large dose of opium is useful here, as a means of lessening the irritability of the heart, thereby rendering the pulse less frequent, and fuller and stronger than before. And this may be safely done, provided blood-letting be not neglected.

The heart is still more frequently the subject of slow or chronic inflammation. In many cases the disease is accompanied or preceded by inflammation of the lung on that side. These chronic affections of the heart are marked by the usual symptoms, but less violent than the former. The heart often enlarges under this disease, and may be felt, and even seen, pulsating much beyond its natural limits. On some occasions, the pericardium after death is found adhering to the heart, so as to obliterate the cavity. At other times, there is serous accumulation, (*hydrops pericardii*). General anasarca, too, often follows, and which is probably to be attributed to the termination of the thoracic duct becoming involved in the inflammation, and consequently obstructed. The treatment is very simple, and consists chiefly in small bleedings from time to time, aided by the digitatis; and, above all, absolute rest of body and quiet of mind. These cases terminate fatally in a large proportion, on account of the structure having undergone too great a change. I could, however, furnish many examples of cures effected by a long-continued perseverance in this simple mode of treatment.

The affection called by modern writers *Angina Pectoris*, in which a constant feeling of oppression about the heart is experienced, appears to me to be based in slow or chronic inflammation of this organ; for I have always observed it to be accompanied by more or less of a febrile state, especially a coated tongue. The paroxysms, which are brought on by quick motion of all kinds, and even by mental affections, appear to consist in a sudden spasm of the heart, putting a stop, for a time, to the circulation; so that the pulse is not to be felt at the wrist. Mr. John Hunter suffered in this way: and I need not tell you that a very short continuance of this state must end fatally.

The extreme suffering of the patient during the paroxysm, as well as the immediate danger to life, call for the use of the most powerful antispasmodic remedies, such as brandy, ammonia, or æther. These, however, are only palliative, and have no tendency to prevent the recurrence of the fits. You must recollect the dependence of the disease originally upon inflammation, so that if the slightest signs of this remain, which generally is the case, the removal of such is the great object of attention. The best means of accomplishing it are the use of small bleedings, the digitalis, and absolute rest; with tonic remedies, when all febrile action has subsided, I have witnessed several of the milder cases of the disease effectually relieved by this treatment.

We shall next inquire into the utility of blood-letting in the diseases of the nutritive system.

NEW OPERATION FOR CATARACT.

To the Editor of the Medical Gazette.

SIR,

IT is now about four years since I devised and described a new plan of operating for cataract. I did not attach much importance to it at that time; but, in the interval, as I have had occasion to perform it a sufficient number of times to form a fair estimate of its value, in comparison with that of other methods, and especially with that employed by my late master (Dupuytren), whose results were considered favourable, I am desirous of making it generally known through the medium of your journal, and shall be thankful for the insertion therein of the following account of it.

Your very obedient servant,
T. KING.

6, Maddox Street,
July 18th, 1838.

The patient should be placed in a bed with his head raised upon two or three pillows, near that side of it corresponding to the eye on which it is intended to operate. A shade of linen that can be easily turned up and down, is to be tied round the forehead. The operator is to stand on the same side as the eye to be operated on. Although he may use the instrument with his right or left hand, he will find it better to use the former for the left, and the latter for the right eye.

I pass the needle* through the sclerotic coat, not at the usual place, but at the inferior and external part of its corneal margin, and at full an eighth of an inch from the edge of the cornea. I introduce it at the lowest possible point; and if the patient's eye is in the position of looking upwards and inwards, it can be made to enter very near the inferior extremity of the vertical diameter of the cornea. In one hand I hold the instrument, as a pen, with the convex side of its blade upwards; and, if necessary, I can help to guide it with the index of the other hand, of which the three next fingers are employed to depress the lower lid, and to fix the eye. The assistant entrusted with the office of raising the upper lid and assisting to fix the eye, standing behind the head of the bed, should take care to hold the former against the osseous vault of the orbit, and not against the ocular globe; so as to have a fulcrum from which he can keep the eye steady, by a degree of pressure he is able to appreciate, and, when necessary, to modify at will. I am cautious to perforate the sclerotic perpendicularly to the tangent of the curve of its surface. Rotating the instrument on its axis one-fourth of a circle, so as to turn the convexity of its blade forwards towards the back of the iris, I pass it on to the pupil, immediately in front of the lens, by a gentle, measured effort, aided by slight rotatory motions. When it has reached the pupil, I give to these rotations such an extent, that the point, turned against the anterior layer of the capsule of the lens divides it into small fragments, or cuts it out, but without interfering with Petit's canal. In general, the only remaining part of the operation may be said to be the withdrawal of the needle, which I effect by gently drawing it with some slight rotations back through the same path by which it was made to travel on to the pupil; for, almost always, the lens immediately follows its point, in a manner which I cannot but compare to that in which a piece of steel obeys the magnet, and lodges in the inferior, external and anterior part of the vitreous humour, close to the little puncture made in the sclerotic, and just behind the posterior chamber of the eye. If it happens not to follow the needle, I have only to close upon it with the point and concave surface of the instrument, and to draw it into this place. If it ascended from its lodge, (a circumstance I have not yet witnessed,) I should place it a little further back, prior to the complete removal of the needle from the eye. It is well to pause a little before the instrument is removed, in order to see that the opaque body remains in the proper place. The needle is to be taken out, as it entered, with the convexity of its blade turned upwards.

CASE.—Mr. B., of 52, Gloucester Street, Queen Square, ætat. 65, coach-plater, of a full habit and sanguineous temperament, first consulted me on the state of his eyes in November 1834. Both were then affected with cataract, the date of the commencement of which I could not learn. Each lens was of a yellowish, dull grey or amber colour, apparently very voluminous and con-

* That of Scarpa slightly modified by Dupuytren.

vex, and of a nearly uniform opacity. Conjunctivæ slightly injected. Contractions of the iris rather slow. The patient was evidently much predisposed to cerebral congestion. I learnt that he had been cupped in 1832 for violent epistaxis, and that the year following he had a slight attack of apoplexy. On both occasions he recovered after blood-letting had been freely employed. I advised him not to have any operation performed until summer, and put him upon an abstemious diet. On the 2d of April, he came to me with severe conjunctivitis in the left eye, occasioned by the sudden exposure of his head to cold, after he had had his hair cut. This yielded to cupping on the nucha and temples, and to aperients; and on the 9th of April no trace of ophthalmia remained.

May 22d.—The eyes being in a good state, I caused the pupils to dilate, by means of the extract of belladonna applied in very small quantity to the conjunctiva, and brought into view the whole disk of the lens in each eye. This body, large and convex, appeared rather more opaque in the centre than elsewhere. I prescribed a regimen calculated to insure regularity of the bowels and prepare the patient for the operation.

24th.—General health good. Bowels regular. In the evening, application of belladonna to the conjunctiva of the left eye.

On the morning of the 25th, I performed the operation on this eye, assisted by Dr. Morris, of Henrietta Street, Covent Garden. The needle was introduced in the manner described; but such was the volume of the lens, and so small the space between it and the iris, that its point entered the opaque body a little, in its way up towards the pupil. I disengaged the instrument, however, without difficulty, and, passing it on, cut to pieces the anterior layer of the capsule by rotatory motions. The instant I drew back the instrument, I had the satisfaction to see the lens follow it, as if by attraction, and leave a perfectly transparent pupil. "Is it possible?" exclaimed the patient, "I see you distinctly!"

For an hour after the operation there was a good deal of weeping from the eye, with a slight sensation of pressure at the place where the needle entered. At four P.M. (the operation was performed at one) these symptoms had nearly disappeared. The patient had had an hour's sleep. As there was a little mucus glueing the lids together, I carefully moistened it, and washed it off with a linen rag dipped in warm water.

26th.—The patient is in a very good state; pupil bright and perfectly free; slight œdema at the lower part of the conjunctiva.

Weak coffee, with a piece of dry toast for breakfast.

At two o'clock, when I saw him again, his bowels had acted, and he was quite at ease.

27th.—He has passed a good night, and is in the same satisfactory state.

Soup for dinner.

28th.—I raised the light shade which had been kept over his eyes, and delighted him with a sight of his wife, whom he said he had not been able to distinguish for three or four years.

29th.—He sits up for a few hours, and takes a little more food.

June 2d.—The curtains with which the room had been darkened were quite removed.

4th.—He walked out for the first time since the operation, using his eye rather imprudently to guide himself.

11th.—He paid me a visit to know when he might lay aside his shade. With an air of triumph he told me he could read and write as well as ever. I warned him against the too early use of his eye, and advised him to habituate it to exercise only by slow degrees.

ON INCONTINENCE OF URINE.

To the Editor of the Medical Gazette.

SIR,

I SHALL feel obliged by your inserting the following remarks in your valuable journal. I am, sir,

Your obedient servant,

THOMAS WHATELY, M.R.C.S.

Berkhamstead, June 1838.

There is no complaint to which females are more particularly subject, the cause of greater individual distress, than that arising from paralysis or deficient action of the sphincter fibres surrounding the neck of the urinary bladder. Let the inability of this organ to retain its contents originate in whatever cause

it may, it requires in its treatment our most skilful and well-directed means of cure, in order to relieve the sufferers from that painful state which renders them not only uncomfortable in themselves, but also unable to mingle in, and partake of, the enjoyments of others. The mind here suffers in a certain degree as well as the body; the delicacy inherent in female nature is shocked; and life, instead of being only partially shrouded in cares and troubles, presents but one unvarying scene of inconvenience and unmitigated distress. A few cases which have of late come under my notice, and in which the means of cure hereinafter to be mentioned have been pursued with great benefit and success, must be my apology for bringing these short remarks before the public.

Experience in a mode of cure renders us less fearful in bringing it under notice; for, however well we may understand the varieties and accidents to be met with in the numerous diseases which come before us, unless we have a previous knowledge derived from minute observation of the action of the medicines we administer, either alone or in combination, we shall often be disappointed in finding them less successful in our hands than they have been in those of others.

When a patient consults us, by reason of being unable to retain the urine in the bladder, we should be very careful in our inquiries respecting the case, in order to our forming a just diagnosis of the cause to which this may be attributable. The nature of the complaint will in general be easily arrived at, if we bestow a due degree of discrimination in its investigation, and are not too hasty in drawing a conclusion from the facts placed before us. Many circumstances may be instrumental in preventing the bladder duly performing its proper function; and if we blindly refer all cases to paralysis of the sphincter muscle, we shall find the curative means we adopt in many instances useless, if not absolutely injurious. Paralysis is less frequent than, without due inquiry, we might at first be led to imagine. We shall find chronic inflammation of the mucous lining, producing change of structure, and altered capacity of this viscus, together with the presence of calculi, either in the pelvis of the kidney or in the bladder, the most common of the causes to which diseases of the class under consideration may be referred. After the middle period of life, true paralysis is but rarely met with.

When the mucous membrane of the bladder is primarily affected, we shall find that our patient always feels more or less pain and irritation about the neck of this organ; and the urine, when it can, in the recumbent position, or during sleep, be retained for a short time, deposits, upon cooling, a quantity of mucus, more tenacious or otherwise according to circumstances. The amount deposited does not in the least depend upon the degree of inflammatory action going on; but principally, I should say, as far as I have observed, upon the length of time the disease has existed, and the organic change the lining membrane has undergone. Nevertheless, in some instances we may have an altered secretion without any abnormal structure taking place, but merely from an incorrect influence of the nerves supplying the part. The state of the urine is mostly alkaline, and loaded with salts. The bladder also contracts spasmodically upon the last drop or two of water, and strains, as if something more were yet to be evacuated. Abdominal tenderness may be added sometimes as another accompaniment. Now in true paralysis of the neck of the bladder, we have none of these symptoms present; we have no pain or uneasiness, no abdominal tenderness, no altered character in the urine, no increased secretion of mucus, but merely a constant draining and incontinence of water, and a sense of uneasiness, weakness, and dull pain in the loins, and low down in the sacral region. Our means of cure, and the causes to which these affections may be attributed, are also different. I will now, from some of my notes, record a case of each description, together with the mode of cure successfully adopted.

Case I.—A married woman, aged 26, previously in good health, found herself, upon rising in the morning, unable to retain her water. Upon seeing her, she complained of complete incontinence of urine, but otherwise described herself as in perfect health. There was no pain about the neck of the bladder, but she felt great weakness in the lumbar region, and pain referable to the lowest part of the sacrum. Occasionally, for about half an hour, the dribbling would cease, and an ounce or two of water be retained. A blister was applied over

the sacrum, and she took a medicine composed of the tinct. ferri. muriatis and tinct. opii, with strong camphor mixture: such was the relief obtained by these means, that at the end of a week the bladder had fully regained its functions. If the means above recommended should fail, the tinct. lytta may be added to the above, with almost a certainty of success. This same woman experienced a similar attack about ten months afterwards, which quickly yielded again to the same treatment.

Case II.—A woman, aged 50, married, and the mother of three children, had for the last twelve months been suffering from inability to retain beyond a few minutes the contents of the bladder; and had submitted to a great variety of treatment without relief. She complained of pain and irritation about the neck of the bladder, and severe straining when small portions of water were evacuated. The urine was alkaline, and contained much mucus. In this case I directed the ung. hydrarg. fort., with camphor, to be rubbed every night over the sacrum, and internally a medicine composed of small doses of the bals. copaib. tinct. opii, and uva ursi in infusion. After persisting in these measures for about a month the irritability of the mucous membrane abated, and the organ gradually regained its power.

We thus see that incontinence of urine may arise from two very distinct and separate causes. It may have its origin from some affection of the inferior spinal nerves distributed to the neck of this organ, and thus causing, as we may say, primary or idiopathic paralysis; or from a chronic change going on in the mucous lining, prompting the frequent evacuation of its contents, and gradually overcoming and destroying the resistance of the sphincter muscular fibres. Many other causes may also act sympathetically in producing symptoms of this kind. Calculus in the pelvis of the kidney or bladder, ascarides in the rectum, or an altered state of the urinary secretion, may be enumerated as those most commonly brought into play. No class of diseases needs more minute investigation to form a judgment upon the parts primarily affected, than those to which the urinary organs are subject. In men we sometimes meet with the prostatic portion of the urethra unnaturally irritable; and upon change of position, or slight exertion, the fibres of the neck of the bladder relax, and about a tea-spoonful of urine escapes unrestrained. This does not arise from any undue fulness or distension of the organ, but solely depends upon the cause I have above mentioned; by the uneasiness it occasions momentarily relaxing the sphincter, and allowing a small portion of water to dribble away, without any effort or aiding of the will. We may in general relieve this uncomfortable state by the occasional and gentle introduction of an elastic bougie, bathing the perineum with warm water, or the hip-bath; and, if necessary, by the application of a few leeches. Medicine internally administered is but of little avail, but if any is advised, possibly the best that can be given will be the soda, with small doses of opium, at the same time keeping the bowels sufficiently soluble by mild aperients.

In those instances where irritability, or chronic inflammation of the mucous membrane, seems to be the primary cause in producing the symptoms of disease, I could recommend the balsam of copaiba, in very small and unstimulating doses, in combination with opium, and at times also, with the infusion of the uva ursi, as the means most efficient in leading to beneficial results. Large and free doses of the balsam are but badly born, and instead of relieving greatly increase the complaint. Eight, ten, or fifteen drops is as much as should usually be given. The liquor potassæ may sometimes be added to the above, or the patient directed to drink freely of lime-water, either alone, or when the stomach will bear it, with a little milk. If the nights are much disturbed, and the system suffers from want of rest, and there is much uneasiness about the lumbar regions, a pill composed of equal proportions of the extract of conium, and Dover's powder, about two or three grains of each, and given regularly at bedtime, will materially assist our other means of cure. The loins and sacral regions may also be rubbed every night with some stimulating liniment, or, what is still better, with an ointment composed of the ury. hydr. fort. and camphor. The strictest attention should be paid to the state of the bowels, and a mild aperient occasionally administered, carefully avoiding those of a mercurial character. The hip-bath used every third evening,

generally affords much relief. The diet of the patient must be light, and the drink consist of some mild and unstimulating beverage. If wine is allowed, it should be taken diluted with water.

For that incontinence of urine which arises from paralysis, and where the coats of the bladder are in a healthy state, a more stimulating mode of treatment must be adopted. Blistering should be had recourse to, and repeated more than once if necessary. The Tinct. Lyttæ, or the powder of the fly, in small doses, from its possessing a peculiar influence over the neck of this organ, is a valuable medicine in this affection; and if it fails, when given alone, may be combined with the muriated tincture of iron, and opium. The loins, hips, and pubic regions, may also be sponged with cold salt and water every morning, and rubbed dry, either with a coarse towel, or the flesh-brush. In those cases which resist this mode of treatment, possibly the strychnine may be of service; but I have no experience of this remedy, and only point it out as well worthy the attention of others. Opium in both of these varieties is of essential service: in the first mentioned its modus operandi is easily explained, but not so in the last: possibly it may act by subduing some remote irritation in the system, from which the local disease derives its origin. The fact of a local disease having some remote constitutional origin is too strong to be denied.

PUFFING QUACK MEDICINES.

To the Editor of the Medical Gazette.

Sir,

The infamous effrontery of some of the advertisers of quack medicines is becoming so outrageous as to require some judicial animadversion, and I should be glad to know whether the authors of some of the forgeries which are resorted to, in order to puff their contemptible nostrums, could not be proceeded against legally. I have now before me "*the Salisbury and Winchester Journal*," dated Monday, July 23rd, 1838, in which are several advertisements, professing to give favourable opinions upon quack medicines, by very eminent, but unfortunately for their friends and for society, *deceased* members of the profession; or if they be still living, an attempt is made to mislead the gullible part of the public by altering the name, so as to make it be believed that the medicine is really prescribed and employed by respectable persons. Thus we have belauded in most disgusting terms of praise, Sir *Ashley* Cooper's Botanical Pills; Sir *Astley* being alive, they have not dared to do more with his name than thus corrupt it; but poor Joshua Brookes is dead, therefore "that eminent surgeon, the late Joshua Brookes, Esq. F.R.S., Professor of Anatomy, &c. &c." is made to write a letter, dated from the "Theatre of Anatomy, Blenheim Street," and addressed "Dear Cooper," to give an attestation of the efficacy of these botanical pills. There is, likewise, another forged attestation; but as this professes to come from a living physician, his name is altered to "Borraghan," in order to deceive the readers of the paper, who are expected to believe that Dr. Boisragon is the writer.

Then again, "Dr. Cummins*, Lecturer on Medical Jurisprudence at the Aldersgate School of Medicine," and "Dr. Ley, Midwifery Lecturer at St. Bartholomew's Hospital," *being both dead*, are made to testify that they have been cured of corns by a trumpery corn-plaster: on this occasion the testimonials profess to be written in notes—"Dr. Cummins presents his compliments," and "Dr. Ley presents his compliments;" but in some other advertisements, poor Dr. Ley is made to attest the efficacy of some remedy for urethral disease, by a letter signed *H.* Ley—he having been *always* in the habit of signing his name *Hugh* Ley.

I see in the newspapers constantly, other respected names which the unprincipled quacks employ either surreptitiously or fraudulently; and it is high time that a stop was placed to such nefarious conduct. While, however, government derives so much advantage from the stamp on quack medicines and the tax upon advertisements, I fear but little can be effected.

An Enemy to Imposture.

July 25, 1838.

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* The name of this respected gentleman was Cummin—not Cummins; and we can confidently assert that he never used the plaster alluded to.—Ed. Gaz.

CASE OF CONCUSSION OF THE BRAIN.

QUESTION RELATIVE TO PARALYSIS.

To the Editor of the Medical Gazette.

Sir,

May I request you to correct a mistake made in spelling my name, in your last number? If you deem the subjoined case worthy a place in your valuable columns, it is at your service.

I am, sir,
Your obedient servant,
D. Culhane.

Croydon, July 16, 1838.

James Sharpe, aged 34, of sound constitution, was driving his master's waggon from market, when the horses took fright and started off at full pace: in attempting to jump out he fell, and the wheel (one of great weight) passed over his head, from before backwards, on the right side, causing the blood to gush from his ears and nose. He was taken (insensible) to a neighbouring public-house. When first seen a short time after the accident, he laboured under symptoms of severe concussion of the brain. I saw him about 9 o'clock on the following morning (the 27th of May), when sensibility had returned, and he complained of severe pain and heat at the lower and back part of the head; his mouth was drawn to the left side, and his utterance was so defective that it was with great difficulty he could make himself intelligible. The palpebra of the left eye was perfectly paralysed, but when it was raised he could distinguish objects as usual. The pupils were in their normal condition; the sense of hearing was very much blunted; there was no wound of the scalp, but some slight effusion, marking the passage of the wheel: no depression could then or now be discovered. Re-action having taken place, I took 12 oz. of blood from his arm, which did not appear inflamed, and ordered a calomel pill every four hours, to be followed by saline aperients.

28th.—Had a restless night; complains of much heat and pain in the posterior part of the head; the pulse 90, full and labouring; respiration natural; tongue parched, and brown; bowels scantily relieved.

Venesect. ad ℥xij. mane; Hirudines, vi. temporibus; Continuantur Medicamenta et Lotio refrigerans Capiti raso applicetur. Vesperi rep. Venesect. ad ℥x.

In the two last bleedings the blood was inflamed.

29th.—Has had some sleep in the night. The bowels acted freely, and he expresses himself relieved.

Repetantur medicamenta. Hirudines et Lotio capiti ut ante.

30th.—Frequent and copious stools of dark offensive fæces.

31st.—Complains of excessive weakness from the purging, which still continues.

℞ Ammoniæ Sesquicarb. gr. x. Conf. Aromat. ℈j. Tinct. Opii, ♏xl. Aq. Pur. lb. ss. Cap. Cochliaria, ij. Amp. 3tiis horis donec hypercatharsis cessaverit.

June 1st.—Has had a good night; the purging has ceased, and the heat and pain of head diminished.

Continuatur Mistura sine Tinct. Opii.

3d.—His family being urgent, he was allowed to be removed to his home, and ordered to continue cold applications to the head, and occasional saline aperients. At my visit on the 7th there was a fœtid purulent discharge from both ears, which he says has given him great pain.

14th.—Has a violent attack of erysipelas of head and face.

℞ Hirud. xij. partibus applicand. Pil. Hyd. gr. v. Ext. Hyosciam. gr. x. ft. Pil. 3 sumat. j. 3tiis horis et Haust. Salin. aperiens donec alvus bene soluta sit.

17th.—The erysipelas extends down the neck, and he complains of difficulty in swallowing.

20th.—The inflammation of the head and face much abated; the submaxillary gland swollen and painful. Ordered to foment and poultice. In a few days this tumor discharged its contents into the mouth, since which he has gone on progressively improving; and this day (July 16th) I had the pleasure of seeing him walking out, having a good appetite, and gaining flesh. The mouth is still drawn to the left side, and the paralysis of the left palpebra still exists, but much improved. The hearing also remains defective.

The prognosis in this case was decidedly unfavourable. My opinion was, that there was fracture of the base of the skull, with some effusion or spiculum of

bone affecting the seventh and ninth pair. How far I was justified in this conclusion, I am anxious to solicit the opinion of any of your readers practically conversant with such cases, and also as to the best mode of treatment for the paralysis still remaining.

TREATMENT OF VARICOSE VEINS *.

A. B. has had an extensive varicose enlargement of the right saphena veins for the last ten years, which commenced at Malta, when on service in 1828. The disease did not give him much uneasiness, unless on great exertion, or when attacked with cramps in the limbs, to which he had been long subject, until about eighteen months ago, when considerable swelling of the vessel took place at its termination, causing acute pain in the groin, occasionally extending to the right side. This was followed by a rupture of the smaller superficial veins on the inner side of the ankle, and a tendency to ulceration: latterly the vessels became so extensively thickened and convoluted, that they could be distinctly traced throughout their whole course, even when emptied in the horizontal posture, and an impulse was felt from the groin to the foot, proving that the valves were completely destroyed. Bandaging and compresses gave him temporary relief, and enabled him to do his duty; but the disease was gradually increasing, attended with great pain on exertion.

On the 25th April last he determined to try the French mode of cure, strongly recommended by Staff-surgeon Melvin, which consists in passing small sewing needles under the diseased vessels, and obstructing their circulation by firm compression with a figure-of-eight ligature, in the view of inducing adhesive inflammation of their lining membranes, followed by ulceration of the included parts, and division of the veins; attending, at the same time, to the general health, and enjoining absolute rest.

He began by operating in six places under the knee, and allowed the compression to continue for twelve days in *three* of them, but the others caused great pain in the limb, and were withdrawn on the sixth day. He also passed a small seton through a large tumor situated on the inner and lower third of the leg (which was composed of tortuous veins and effused blood), with the view of causing absorption and obliteration of the parts. This was allowed to remain five days, then withdrawn, and pressure applied.

The result of this treatment has been a decided improvement in the limb in general, and the circulation in the diseased vessel is completely obstructed from the knee to the groin, with total disappearance of the inguinal tumor, and relief from pain in the thigh. The veins are still pervious in the leg, but much diminished in size, and the tumor has nearly disappeared; leaving, however, great discoloration and weakness, which render the constant use of an elastic bandage indispensable.

On the eleventh day, considerable pain having attacked the groin and situation of the tumor, lecches were applied, which gave great relief, but was followed by erysipelas, and a number of small troublesome abscesses that protracted the cure, and the ulcerated places did not heal rapidly. The pain in this operation is absolutely nothing if the needles are introduced with a rotatory motion, and the ligatures applied gradually; but in future I would use needles with a cutting edge and a small round silk ligature, which will facilitate the division of the vessel, without, in my humble opinion, causing any danger, as consolidation of the parts takes place in two or three days. I also passed several threads through two of the smaller veins of the foot, and allowed them to remain several days, without any pain. These vessels were obliterated; but I do not think this mode would answer in longer vessels, as the setons must be thick to induce adhesive inflammation sufficient to close the canal, and might cause serious phlebitis.

I have lately seen, in a periodical, that in America they pass a needle through, as well as under the vessels, before applying the ligature; but I do not perceive what advantage this has over the latter mode, if the needle has a cutting edge, and the thread is small, round, and firmly applied.

N. W. C.

July 20th, 1838.

* This case, which is authenticated to us, may be considered as a supplement to the paper in our last number.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Flora Medica; a Botanical Account of all the more important Plants used in Medicine, in different Parts of the World. By JOHN LINDLEY, Ph. D. F.R.S, Professor of Botany in University College, London, &c. &c. London, 1838. 8vo. pp. 656.

THIS work contains an account of 1350 plants, drawn up with the botanical tact for which Dr. Lindley has long been celebrated.

The following extracts may give some notion of the practical information which is diffused over the book, independently of its botanical merits:—

Panax quinquefolium, or Ginseng.

"Root an agreeable bitter sweet, with some aromatic pungency. Has a prodigious reputation among the Chinese as a stimulant and restorative, under the name of 'Gin-seng,' but by Europeans and Americans considered nothing more than a demulcent, approaching liquorice in its qualities. This, however, requires further investigation, for we cannot believe that all the Chinese say, believe, and practise, is fabulous or imaginary. Is the Tartarian plant the same as the North American? Under what circumstances are they each collected?"

Lagenaria vulgaris, or Bottle gourd.—"In the wild state this plant produces poisonous fruit. Some sailors died, at one of our outports, a few years since, from drinking beer that had been standing in a flask made of a bottlegourd. Dr. Royle says that he learned from a very respectable and intelligent native doctor, attached to the gaol hospital at Saharumpore, that he had seen a case of poisoning from eating of the bitter pulp, in which the symptoms were those of cholera."

Tilia Europæa, Lime or Linden Tree.—"Flowers, separated from the large leafy bracts, are used as an infusion, in Austria, with much success in vertigo and spasms; they promote perspiration and alleviate coughs. But if the bracts and fruit are mixed with the flowers, the infusion then becomes astringent and confines the bowels. *Host.*"

Brucea - antidysenterica. — "Considered, in Abyssinia, a most valuable remedy in dysentery and severe cases of diarrhœa, but not known in Europe. It has been supposed that a poisonous bark called *False Angostura*, was yielded by this plant; but it now turns out that it is nothing but the plant of Nux vomica (Strychnos). *Guibourt*, ed. 3, i. 4. All the statements, therefore, that have been made concerning the danger of Brucea bark and Brucine, belong to Strychnos, and have nothing to do with Brucea itself."

Colchicum autumnale.—"The dried cormi and seeds are used extensively in the manufacture of Veratria, and in various pharmaceutical preparations. Colchicum is found to increase the secretions of the intestinal mucous membrane, and of the kidneys, and in some cases to act as a sudorific; it is also emetic and purgative, and in large doses is a powerful narcotico-acrid poison. It is used extensively in dropsy, gout, rheumatism, and also as an anthelmintic. The energy of the cormus, and consequently of the preparations from it, is often much impaired by the collection of the plant at a wrong time of year, or by keeping it after it has been collected until the flowers sprout forth, which they will do quickly if taken into a warm place. When the leaves are quite withered is the best time for taking up the cormi, of which use should be made without loss of time. Many of those sent to the drug-shops for sale have already pushed forth their flowers, which are broken off so as to prevent the circumstance from being observed. I have seen many cwts. sent to town in this state, which nevertheless found a ready sale, and at the best price."

The various species of cinchona are described and discussed at considerable length from p. 406—427, and the following are the conclusions at which Dr. Lindley arrives:—

"The result of the preceding inquiry, so far as it affects the barks ordinarily used in this country, is shown in the following table of officinal names referred to their species:—

(*a*) *Pale Barks.*

Crown or Loxa Bark	C. Condaminea.
Silver, Grey, or Huanuco Bark ..	C. micrantha.
Ash Bark	(*not ascertained.*)
White Loxa Bark	(*not ascertained.*)

(*b*) *Yellow Barks.*

Yellow Bark	C. lanceolata, chiefly; also C. hirsuta, and nitida.
Calisaya	C. lanceolata?
Carthagena Bark	C. Cordifolia?
Cusco Bark	(*not ascertained.*)

(*c*) *Red Barks.*

Red Cinchona Bark of Lima	(*not ascertained.*)
Cinchona nova	C. magnifolia.

(*d*) *Brown Barks.*

Huamalies Bark	C. purpurea.

*** No doubt this genus will be largely increased by future discoveries.

We hardly anticipate that the ordinary student of medicine will have time to make himself master of so minute and elaborate a work as Dr. Lindley's, though he may advantageously use it as a book of reference; but there are two classes of persons to whom it will be eminently useful; the one being the lecturers on botany and materia medica, and the other the host of practitioners who go to tropical climates, and who will find our author an excellent guide when they are deserted by their usual manuals and dispensatories.

MEDICAL GAZETTE.

Saturday, July 28, 1838.

" Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

PROCEEDINGS OF THE LONDON UNIVERSITY.

Our readers will have perceived from the note of the proceedings in the House of Commons, which we published last week, that Lord John Russell tacitly acknowledged the accuracy of all we had asserted in a former number regarding the lamentable state of disorganization to which the University of London was reduced. In fact, its constitution is radically defective, and it carries within itself the principles of incurable and speedy decay; or, as the Bishop of Durham—himself a member of the Senate—expresses it, the institution, as at present constituted, " is more likely to become the laughing-stock than the object of praise and imitation to other learned bodies in the country."

Now one of the great vices in the constitution of the University is, that the Chancellor and Senate are kept in a state of the most degrading dependence. No regulations touching the examination of candidates, or granting of degrees, are good for any thing till they have received the sanction of one of the principal Secretaries of State; nay, the very fees to be charged must receive the approval of the Commissioners of the Treasury, before they can be levied! The immediate result of this subjection to the minister of the day is, that it opens a door to " back stairs" influence at the Home Office, and thus a little intrigue may at once reverse the most mature decisions of the Senate, or arrest the whole machinery. Not many months ago the arrangements on the subject of religion, framed by the majority, were set at nought in consequence of a member of the minority betaking himself to Downing Street. But further: if a member of the Senate, who happens to have a vote in a certain other Senate House, should find himself habitually in a minority, he may diminish the majority against him, perhaps even change his relative position altogether, by inducing the noble Secretary for the Home Department to add certain docile friends of his to the

Senate! Nay, a very cunning person might have all this so managed, that the new nominations might not be for one moment suspected, until the sign manual had been attached to the document, and the appointments consequently rendered final and irrevocable. It will of course be understood that we allude to the addition, *sub rosa*, of Dr. James Somerville to the Senate of the London University, made by Lord John Russell to oblige Mr. Warburton. Now we do not at present refer to the abstract propriety or impropriety of the nomination itself, but to the manner in which it was made; and this, we hesitate not to assert, was in the highest degree insulting to Lord Burlington, as Chancellor, and to every other member of the University. Let us call to mind the circumstances. Mr. Warburton proposes to have Dr. James Somerville appointed Registrar, a kind of secretary, to keep the minutes of the Senate. A great majority of his colleagues at once declare against the measure; but he, instead of yielding to their decision, perseveres in the struggle, so that meeting after meeting is occupied in resisting his efforts to carry this particular point. Compelled ultimately to abandon the attempt, he goes quietly down to the Home Office, to confer with Lord John. Meantime Lord Burlington and the members of the Senate—innocent souls! —continue their meritorious labours, till, to their astonishment, a new charter is one day laid upon their table; it is announced that they have had no existence as a University since the demise of the late King, and that all their recent proceedings have been illegal. It had not been thought necessary, either with a view to obtaining his Lordship's advice and assistance, or even as a matter of common courtesy, to inform the Chancellor of his official demise, ere the new charter was drawn up and finally executed by the attachment of the royal signature. But there was one member of the Senate in the confidence of the Secretary for the Home Department —one who participated in a knowledge of the discovery that a new charter was required—one who had influence enough to have the name of a friend added to the Senate: that individual, it is almost superfluous to add, was Mr. Warburton.

Now we put it to Lord John Russell, whether this was treating the Chancellor of the University, or the gentlemen who compose the Senate, with the respect which was due to them? He must feel that it was not.

But there is another light in which the transaction may be viewed, and which can scarcely be very flattering to the noble Secretary himself; it is this:—Mr. Warburton induced his Lordship to place clandestinely upon the Senate a gentleman whom the others refused to meet, and whose absolute retirement was rendered a condition without which no quorum of the Medical Committee could be assembled! Lord John Russell may gather from this how far Mr. Warburton is to be trusted as a guide in such matters.

From the dead-lock in the machinery of the University, owing to the cause above mentioned, it was contemplated that no further meeting of the Medical Faculty could take place till a new charter had been drawn up; but Dr. James Somerville having at length resigned, business has been resumed: Dr. Roget has been re-elected Chairman, and every thing restored to the same state as anterior to the obtrusion of the new member.

Nevertheless, a new charter is still found to be indispensable—we presume, to prevent the University from becoming a "laughing-stock" to other institutions. Under these circumstances we earnestly recommend to Lord John

Russell to shew a little energy—a scintillation of spirit; and as Dr. Somerville is not to have a seat in the new Senate, let him omit his friend also. Let him have the courage to strike out Mr. Warburton's name, and we can promise him he will obtain more credit than he has yet done for any part he has enacted in connexion with the University. True, Mr. Warburton's position is not what it was: his nominee compelled to retire by the all but unanimous voice of his colleagues, and Dr. Sims removed by death, there remains only Dr. Billing (how on earth came *he* ever to be appointed?) to support him. There is thus a seconder of any thing Mr. Warburton may choose to move; but here, we suspect, his support will end. One might suppose that, so situated, he must of necessity be innocuous; and certainly it will be the fault of the Senate if they do not now at once put an extinguisher upon any of his measures of which they disapprove. Yet we again advise Lord John Russell, if he really wish well to the embryo institution, to remove him. Busy, pertinacious, and imperturbable, his resistance to being put down exceeds belief, and it requires men of more energy to control him than many of the Senate possess; though we most willingly allow that some of them have recently acted in a most spirited and effective manner.

There is another point we would suggest to Lord John Russell: it is, to make the University independent of ministerial authority. Are we to receive the clause which makes it absolutely subservient to the Home Office, as a specimen of Whig liberality? Supposing that his Lordship's opinion of the present Minister for the Home Department should lead him to place the most implicit reliance on his wisdom; supposing it to be very proper at the present moment, that a word uttered by official lips should annul the most matured arrangements of the Chancellor and Senate, regarding education; yet the time may come when we shall not be so blessed as now, and thus the ministerial fiat devolve on less infallible men. Let the noble Secretary for the Home Department, then, in pity to future generations, sacrifice a little of the benefit which we concede would accrue to the present race, from the existing arrangement; the ample mantle of his Lordship can scarcely be expected to descend to his successors, and, at all events, it is just a possible contingency that a time may come when the decisions of an assembly of thirty or forty learned men may be as much entitled to respect as the dictum of a Secretary of State. We protest, then, against this portion of the Charter, and we implore the members of the University to exert their utmost influence to have this altered, if they would save their institution from contempt.

Our readers may perhaps remember that we denounced the University from the very commencement. We saw in the provisions of the Charter, and the men appointed to carry them into effect, a combination leading to inevitable failure. Our predictions, placed on record the week after the Charter appeared, and which at the time were looked upon by many as unnecessarily gloomy, have already been fulfilled. We now as confidently prophesy that the University will never come to any thing, so long as no change is made in the persons entrusted with carrying its objects into effect, and so long as the decisions of the Senate, regarding education and degrees, can be at any time set aside by a breath from Downing Street. But our former predictions are more than verified; for not merely has one Charter been found inefficient—not one University

only has broken down, but we are already at the end of the second. The first was founded by William IV. It is extinct. The next by Queen Victoria ;—it is moribund, or rather is convicted, and lies in the condemned cell for execution in October. The hour of its decease is to give birth to a third Charter, and virtually to a third University. Three charters in two years! Three charters before a single arrangement has been completed!! Three charters before one degree has been conferred!!! There is a certain class of persons of whom the proverb informs us that even they are taught by experience. We may venture, without disrespect, to hope that the gentlemen in Downing Street will verify the saying, and that at no distant time it will not be found necessary to issue charters to the London University annually, but that once in two or three years, or even perhaps at longer intervals, may be found sufficient.

DENTAL SURGERY.

Mr. Andrew Clark, of Brooke Street, has lately invented an ingenious and very successful method of extracting the fangs of teeth, when they are so deeply imbedded in their sockets as to be incapable of removal by any of the ordinary methods. It consists in boring a hole, to a sufficient depth, in the centre of the fang, into which a screw is introduced so as to take firm hold of the fang, and give the hand of the operator a complete command over it, so that it may be loosened and extracted with great ease, and comparatively little pain.

The preceding information has been furnished to us by an eminent physician, who himself experienced the benefits of Mr. Clark's ingenious invention.

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

The anniversary meeting of the above institution was held at Bath on Wednesday, the 18th, and was attended by nearly 300 medical men from all parts of the kingdom. Dr. Boisragon, the President for the past year, having taken the chair, he addressed some judicious valedictory observations to the meeting, and then gave place to Dr. Barlow, the President elect, who dwelt at some length upon the views and objects of the institution. We regret that our limits prevent us from giving the speeches of these gentlemen, and we must content ourselves with recording the Report of the Council, which we subjoin.

Report of the Council of the Provincial Medical and Surgical Association, for the year 1837-8.

The steady progress of the affairs of this Association from the period of its institution, six years ago, to the present time, serves most undeniably to evince that its founders made a correct estimate of the existing desire among the cultivators of medical science residing in the English provinces, to combine together, so as to make their labours more instrumental than they had formerly been in the advancement of the healing art. Each revolving year has increased the number of its members, which now amount to 1080, and the influence that has arisen from this additional strength has been directed to the accomplishment of the important objects which were at the commencement of the undertaking specified as most worthy of regard.

The review of the period that has elapsed since the last anniversary meeting, gives encouragement to the hope that each returning year may afford fresh demonstration of the advantages that may result from the continuance of an association which has in view the promotion of harmony and good feeling among its members, and the collection and diffusion of facts which may serve to render medical science more signally beneficial to mankind.

District branches. — The Council are happy to record that they believe the system which was sanctioned at the last anniversary meeting, of dividing the Association into district branches, is likely to be attended with very favourable results, by adding to the number of its members, and by facilitating the scientific and social intercourse which it is the great object of this association to promote. Since the last anniversary, two district branches have been added to those which were then in existence, viz. one at Newton, in Lancashire, called the Newton Branch, comprehending members residing at Manchester, Liverpool, Warrington, and several towns in the north of England; the other at Shrewsbury, called the Shropshire and North Wales District Branch. There are now, therefore, six branches: the Eastern, the Wells, the Southern, the

Bath, the Newton, and the Shropshire branch of the Provincial Medical Association. Each of these branches has, in compliance with the laws passed last year, submitted the regulations by which they are governed to the central council of the Association, and they have by them been confirmed. There is nothing in these regulations which calls for any comment, as they are formed on the basis of the resolutions agreed upon by the Cheltenham meeting. It may, however, be right to remark, that the associates in the Newton branch have declined receiving the allowance of one-seventh part of the amount of their subscriptions to defray the expenses of the district branch, as they prefer meeting this expenditure by an additional contribution among themselves.

Finances.—The state of the finances will ever require the vigilant attention of your Council, as much of the usefulness of the Society would be diminished if its pecuniary affairs did not flourish. It is therefore very satisfactory to discover at the end of a year during which the demands upon the funds have been greater than before, that the income has been so far increased, that a larger balance than on the last occasion of our assembling together is in the hands of the treasurers.

It may not be irrelevant here to point out to those members who have not sent in their subscriptions, that they can at any time do so, without much trouble, by paying the amount to their own bankers, and desiring that it may be remitted to Messrs. Robarts and Co., London, for Messrs. Berwick and Co., Worcester, on account of the Provincial Medical and Surgical Association. A punctual attention to this regulation by the members who have not had an opportunity through other channels of paying their subscriptions, would considerably increase the balance in our treasurer's hands.

Transactions. — The superintendence of the publication of the Transactions has anxiously engaged the attention of the Council, and the contributions of the members have enabled them to publish their sixth volume. It is gratifying to observe the progress the Association is making in laying before the public the medical topography of England. The memoirs on Exeter and Cheltenham abound in interesting medical and statistical details, which, if followed up by similar descriptions throughout the kingdom at large, cannot fail to be productive of much benefit. The Council also congratulate the members that the replies to the inquiries respecting the epidemic influenza contained considerable information from persons residing in different parts of the kingdom, and have enabled the authors of the report on that subject to publish a communication of very great value. The result of this inquiry is very satisfactory, as it shews that in all future epidemics a similar plan may with propriety be adopted; and it has encouraged the Council to issue queries of a similar nature respecting small-pox, and the protecting influence of vaccination. It has been suggested, also, that were members generally to communicate the results of their experience in the management of certain diseases upon which there exists at present considerable difference of opinion, valuable information might be expected to accrue. Cases which, taken separately, are of little value in the establishment of correct views either of pathology or practice, when arranged and compared with those already recorded, would thus possess great interest, and lead to important results. The Council are therefore disposed to urge members to forward notices or complete histories of any such cases as may occur in their practice, with the view of classifying them upon some future occasion, when they shall be in sufficient number, or of sufficient importance, to render it desirable to publish them.

Benevolent Society.—The connexion of a benevolent fund with the Association is too important to be passed by unnoticed. The Council are happy to congratulate the members on the fund having come into active operation; but, as a report will be presented descriptive of what has been done by the benevolent committee, it will not be necessary to do more in this place than strongly to recommend this object of the Association to the cordial support of the members. The committee at Cheltenham last year recommended collecting contributions in small sums. This recommendation has been acted upon by a member at Warrington, who, through the assistance of friends, has collected upwards of thirty pounds, thus proving that, by a little exertion, a considerable addition may in this manner be made to the funds.

Parochial Medical Relief.—The question of parochial medical relief, to which so much attention has been given by this Association, is still in an unsettled state; but, in compliance with the prayer of the petition sent last year from this Association to parliament, the House of Commons have entered upon this investigation of this difficult subject, and it is much to be desired that the combined wisdom of our parliamentary representatives may be enabled to devise some means by which an improvement may be effected in the present system. For the convenience of members, and for the purpose of substantiating the statements contained in the

Poor Law Committee Report, and with the hope of insuring a complete and candid investigation, the Council appointed a competent delegate to be present at the medical inquiry. Two members of the late poor law committee appointed by the Association, have also been examined before the select committee of the House of Commons, and have thus had an opportunity of detailing the evils, and pointing out the deficiencies, of the present system of parochial medical relief; and at the same time of supporting the recommendations for amendment contained in the two reports adopted by the Association, with such additions and improvements as time and further consideration have suggested. It appears, therefore, necessary, that a committee should be speedily appointed to watch the further progress of the subject through Parliament, and to suggest to the Council from time to time such measures as may appear to them necessary to meet circumstances as they arise.

Prize essay.—The committee appointed at Cheltenham to fix on the subject of the Thackeray Prize, sent in their report early in the present year, and the Council lost no time in making known their determination through the periodical press, and have announced that a prize of fifty pounds will be awarded to the author of the best essay, having for its object the investigation of the sources of the common continued fevers of Great Britain and Ireland, and the ascertaining of the circumstances which favour the diffusion of these diseases, and also those circumstances which may have a tendency to render them communicable from one person to another. The prize will be open to the competition of the members of every accredited school for medicine and surgery in the United Kingdom, and the essays are to be sent to the Secretaries of the Association, on or before the 1st of January, 1840.

Each essay is to have a motto affixed, and to be accompanied with a sealed note, containing the name and residence of the author, and the motto is to be written on the outside of the note.

The Council are of opinion that at this meeting certain members should be appointed to fix upon the essay which is best entitled to the prize.

Empiricism.—The Council are aware that one duty which should never be forgotten by the members of the Association, is that of assisting, in every practicable way, to improve the medical art, and to uphold the dignity of the profession.

The evils resulting from quackery are of so obvious a nature, and it tends so much to obstruct the steady progress of medical science, that its suppression is devoutly to be wished. Within the last two or three years there has been a growing opinion that the legislature should interfere, and endeavour to lessen these evils. It is, however, certain, that no legislative enactment could be so framed as to prevent those who are ignorant from placing reliance on quacks, and it would not be easy to interdict the swallowing of their pills and potions; but though legislative authority may not have either the right or power directly to suppress quackery, it need not give it sanction and support, as is now done.

It is hoped that ere very long the whole question of medical legislature will be brought before Parliament by Mr. Warburton, when an opportunity will occur to frame such legislative enactments as may be practically beneficial. The Association, in the meantime, may be employed in calm and philosophical inquiry into the nature of the evils, their extent and remedies, by which means they may be enabled, at the proper season, to render valuable aid in legislating upon this subject. The Council, therefore, advise that a committee of members be appointed at this anniversary, to consider the nature, extent, and evils of quackery, and to report on the same, with a view of devising such corrections as the expositions thus made may indicate.

Sections of members.—The Council consider it desirable on every recurring anniversary, not only to review the past, but also to look forward to the future, and endeavour to ascertain whether any further improvement can be effected in the existing organization of the Association; and acting on this principle, they are led to submit to the consideration of the meeting, whether some good effect may not arise from dividing the Association into sections for the prosecution of particular departments in medicine. As the question of vaccination is at this time exciting considerable interest, a section of members may be appointed, to whom the replies to the queries of the Council may be sent, and the whole question referred to them for illustration; and if, in this instance, the arrangement be found to work well, sections on other branches of medical science may at a future day be appointed.

Report on Provincial Medical Schools.—The Committee that was appointed three years ago, to draw up a Report on the state of Provincial Medical Schools, has been hitherto unavoidably prevented accomplishing the task assigned to them. Mr. Hetling's protracted illness incapacitated him from pursuing the investigation as he had intended. The lamented death of that gentleman, who was amongst the earliest and most zealous of our members, has made it necessary to assign that work to other hands; and the Council consider,

that in having prevailed upon Mr. Turner, of Manchester, to finish the work which was commenced by their deceased and respected associate, they have insured the concurrence of all in the propriety of their choice; and they have every reason to expect that the Report will be finished so as to be published in the seventh volume of the Transactions.

Conclusion.—The Council now resign the important trust consigned to them at the last anniversary, to those from whom they received it; and they venture to express a hope that during the period which has elapsed since the last anniversary, they may be found to have laboured with zeal and diligence to discharge the important duties that have devolved upon them. It is only by such means that they can expect to have their exertions crowned by the approbation of the members at large—an approbation which they are at all times anxious to deserve.

To conclude:—The Council conceive it is not irrelevant to remind each member of the implied engagement which he enters into when joining this Association, to forward the enlightened objects for which it was instituted. These objects are dignified and ennobling; and, if pursued with ardour and perseverance, must ultimately enlarge the boundaries of knowledge, elevate the character of medical men in the community, lessen the sufferings of humanity, and thus confer a blessing upon mankind.

CAUSES OF MORTALITY IN THE WEST INDIES.

A VERY elaborate and valuable set of Reports has been printed from documents furnished by the Army Medical Department, relative to the sickness and mortality of our troops in tropical climates. It is impossible to publish them at length; but the following summary will be found highly interesting:—

Though the minute train of investigation pursued in this Report has not enabled us to distinguish with certainty the essential causes of sickness and mortality among European troops and civil residents in the West Indies, and though perhaps these causes may remain for ever involved in obscurity, yet the numerical results at which we have arrived seem sufficient to warrant the belief that many of the opinions hitherto entertained, in regard to the nature and influence of these causes, must have been adopted on very inadequate evidence; at least, they are by no means in accordance with many of the facts adduced in the course of the present inquiry, and to which we shall now briefly advert.

It has been supposed by many, that the diseases which prove so fatal to Europeans in these latitudes, especially fevers, are, if not a necessary, at least a very general consequence of continued exposure to a high temperature. The sufficiency of this, however, as a uniform cause of sickness and mortality, is contradicted by the fact that these vary considerably in different stations, the mean temperature of which is nearly alike. The range of the thermometer, for instance, in Antigua and Barbadoes, is rather higher than in Dominica, Tobago, Jamaica, or the Bahamas; yet we find that the troops in the latter stations suffer nearly three times as much as those in the former. The preceding pages also afford several instances in which epidemic fever made its appearance, and raged with the utmost virulence during the winter months—a circumstance not likely to have taken place if that disease had originated in increased temperature. We may also state that the epidemic fevers which prevailed at Grenada in 1793, and at St. Christopher's in 1812, two of the most fatal which ever appeared in the West Indies, commenced, the former in March, and the latter in February, and continued with unabated violence during the whole of the cold season.

If elevated temperature was an essential cause of the mortality to which Europeans are liable in this climate, we might expect it in every year to produce similar effects; whereas, on the contrary, it appears, from the tabular statements in the preceding Report, that the mortality in one year is sometimes twenty times as high as in another, without any perceptible difference in the range of temperature. This fact has already attracted the notice of some medical authors, who, in treating of yellow fever, adduce instances of various epidemics both within and beyond the tropics, during which the temperature was not beyond the average, and was sometimes even a little below it, and inversely where the existence of a high temperature was not attended with the prevalence of fever*.

In accounting for the unhealthiness of these colonies, great influence has been ascribed to excess of moisture, and the inference derives plausibility from various facts in the history of tropical fevers, especially their great prevalence along the sea-coast, at the outlets of rivers, and in the vicinity of swampy level grounds. This hypothesis, however, seems at variance with the facts contained in the previous Report; for if the mortality of the troops depended materially on the influ-

* Craigie, Practice of Physic, pp. 224, 226, 227.

ence of moisture, we might expect it to attain its maximum in those stations where the fall of rain was the greatest, whereas the average mortality of the troops in Jamaica is at least double that which prevails among those in British Guiana, though the quantity of rain which falls in that island is little more than half as great; and in the preceding pages there are adduced many instances in which epidemic fever has broken out, and raged with great violence, at a period when no rain had fallen for several months; nay, in some stations a dry, in others a wet season, is looked on as the most unhealthy —an anomaly not likely to occur if excess of moisture was uniformly an essential cause of insalubrity.

It must also be remembered that this excess of moisture is not confined to the West Indies, but is a general characteristic of all tropical regions: and were it so productive of disease in the Western hemisphere, the same effect might be expected to ensue from it in the East; whereas, on the contrary, the Malabar Coast, which is deluged by rain for six months in the year, is generally one of the most healthy quarters in the Madras Presidency.

That neither heat nor moisture can be the primary causes which influence the health of the troops in the West Indies, is at once established by referring to the comparative view of the ratio of mortality in each year at every station, exhibited in Tables XXXIV. and LX., in which there are numerous instances of two adjacent islands, or even of two contiguous stations in the same island, being subject in an equal degree to the operation of these agencies, and yet while the one has been desolated by the ravages of fever, the other has been enjoying a degree of salubrity equal to that of Great Britain.

Though heat and moisture are not the primary causes of fever, however, it is highly probable their operation tends in some measure to increase its intensity. The tables, illustrating the influence of the seasons on the health of the troops in each station, shew that the greatest number of admissions into hospital and deaths has, on the average of a series of years (though not uniformly or equal in each year), taken place in those months when the greatest degree of heat was combined with the greatest moisture; and it may be observed as a striking exemplification of this fact, that as the sun proceeds northward in the ecliptic, carrying heat and moisture in his train, the period generally termed the unhealthy season is later in the northern colonies than in those to the south.

The unhealthy character of that period of the year in which the greatest degree of heat and moisture is combined, is not, however, confined to the West Indies, but extends also to the East, as well as over a large portion of the Northern temperate zone. In the Mediterranean stations particularly, the admissions into hospital and deaths among the troops average nearly twice as high between July and October as during any other months of the year. Even in Canada, the same peculiarity is observable, though not in so marked a degree; and conversely in stations southward of the equator, that period of the year, which on the north of the line is the most unhealthy, becomes in the south the most salubrious, in consequence of the seasons being reversed. The calculations on which these statements are founded, will be submitted in subsequent reports. At present we merely advert to them in order to guard against the error of referring to the climate of the West Indies exclusively, phenomena which are common not only to other tropical regions but also to those of the temperate zone.

A knowledge of this fact at once overturns a plausible hypothesis which attributes the unhealthy character of the West Indies during what is termed the sickly season, viz. from July to October, to the want of the free ventilation afforded by the trade-winds during the rest of the year, but which at this period either cease altogether, or become very irregular. But though these two events, the failure of the trade-winds and the increase of sickness and mortality, take place at corresponding periods, the latter can never be regarded as a necessary consequence of the former, when we find that in other quarters of the globe, beyond the range of the trade-winds, that is, in countries north of the 30th and 32d degree N.L., and in which ventilation is quite as perfect at that period as at any other, the unhealthy nature of these months is marked as strongly as in the West Indies.

This same fact strikes also at the root of another hypothesis, which attributes the sickly season in these regions to some morbific principle generated in the vast forests and savannahs of the South American continent, and wafted to these islands by the south-westerly winds which generally prevail during that period. Besides, were this hypothesis correct, we might expect that British Guiana would, from its proximity to this cause of disease, be most subject to its operation, and consequently the most unhealthy; and that the colonies further to the north, being least exposed to it, would enjoy the greatest degree of salubrity. The result of our investigations into the comparative mortality in each colony shews, however, that their relative salubrity is by no means affected by their proximity to or distance from that continent.

Some, who are conscious of the difficulty of accounting for the unhealthy character of these colonies by the operation of general causes, endeavour to trace it to the influence of local circumstances, in particular to exhalations or emanations from the soil. To illustrate, therefore, the nature and extent of the operation of this alleged cause, we have stated, as accurately as our information will admit, the physical and geological characters of the soil in each island, and in the immediate vicinity of each station; and by comparing these with the mortality there, have ascertained that at many, where the soil appears exactly the same, the rate of mortality is very different; and at others, where the soil is very different, the rate of mortality is much the same. It is also to be observed that, while the soil and its physical characters are the same in every year, the sickness and mortality are extremely variable, and only in certain seasons and years attain an extraordinary degree of intensity. It frequently happens, too, that a station which has been remarkable for its sickly character for one or two seasons, becomes, without any perceptible reason, just as remarkable for its salubrity, which could scarcely happen if the cause of that sickness and mortality existed in the soil, which was constantly there to produce it.

The agency, real or supposed, of marshes is liable to a similar objection. That the vicinity of marshes, swamps, and lagoons, is generally subject to fevers, both of the intermittent and the remittent type, is a fact sufficiently established by multiplied experience, both in tropical countries and within the temperate zones. But that remittent or yellow fever may be generated where no such cause is in operation to produce it, and that consequently it is impossible to establish a necessary connexion between this cause and the appearance of that disease, is sufficiently established by the fact that the sickness and mortality in British Guiana and Honduras, where swamps and marshes most abound, are considerably less than at Up-Park Camp, and several of the other stations in Jamaica, remote from the operation of such agencies.

The same remark may be applied to excessive or rank vegetation, to the influence of which much of the sickness and mortality at some of the stations has been ascribed. To both of these causes, indeed, the remark already made regarding the influence of the soil, is strictly applicable. The marshy lands and the rank vegetation exist at many of the stations in every year, whereas the disease, which is represented to proceed from them, is only of occasional occurrence, and the foregoing Report shows that in some years the extent of mortality has been ten times as great as at others, when the degree of heat and moisture by which the marshy soils and vegetation are most likely to have been affected, have been much the same.

The object of this Report is rather to point out effects than to speculate upon causes, especially where they are so much involved in doubt and obscurity. We have merely referred to these alleged sources of disease, to show how much they are at variance with numerical results, and because so long as the causes which affect the health of troops in the West Indies, are held to be accounted for by theories founded on error, it is not to be expected that others will be started more consistent with truth.

We are too sensible of the difficulty of the subject to venture on any theory of our own, which might on subsequent examination prove as futile as those which preceded it; but we merely wish to call the attention of such persons as may be disposed for further inquiry, to the circumstance that as yet no experiments have been made on the electrical condition of the atmosphere in the West Indies, during periods of epidemic, and as it is possible either an excess or deficiency of that powerful though unseen agent may exercise an important influence on the vital functions, the subject seems worthy of attention. Heat and moisture are well known to be intimately connected with the development of electrical phenomena, and its influence on vegetation has also recently been established by experiment; consequently if the prevalence of disease could be satisfactorily traced to that source, the reason why heat, moisture, and vegetation should have been mistaken as the causes, when acting only as auxiliaries, would be readily accounted for; and even should the results leave the cause of disease as undetermined as before, science will at least be benefited by the inquiry.

A comparison of the relative mortality at each station, with its topographical peculiarities as detailed in the preceding portion of this Report, will afford much information in regard to the influence of different localities on the health of troops. The instances of Fort St. George at Tobago, Mount Fortuné at St. Lucia, and Morne Bruce at Dominica, demonstrate that mere elevation to the height of 600 or 700 feet, instead of securing a healthy position, seems rather to have the reverse tendency. The records of the mortality at Stoney Hill, too, show that an elevation even of 1360 feet is sufficient to secure an immunity from the remittent fever of Jamaica; but the details we have supplied in regard to the troops at Maroon Town

and some of the smaller outposts, demonstrate beyond a doubt, that, at an elevation of from 2000 to 2500 feet, they are likely to be either wholly exempt from that disease, or to encounter it in so very modified a form, that the mortality from all causes will not on the average of a series of years, materially exceed that to which an equal number of European troops would be subject in the capital of their native country. The diseases of the tropics seem, like the vegetable productions of the same regions, to be restricted to certain altitudes and particular degrees of temperature. The researches of Humboldt on this subject have tended to establish, that yellow fever is never known beyond the height of 2500 feet, so that the nearer this boundary can be approached the more likely is the health of the troops to be secured.

Where sufficient elevations cannot be obtained, the localities which appear least subject to yellow fever are low sandy tongues of land or peninsulas jutting into the sea, and not shut in by any high ground immediately in rear, similar to those on which the barracks at Fort Augusta and Lucea are built, and where the troops have generally been more exempt from the epidemics of Jamaica, than at any of the other stations, except those in the mountains. It cannot be affirmed that these will prove, like Maroon Town, uniformly healthy, but there seems good reason to suppose that on the average of a long series of years the mortality there will be considerably below the average.

The superior salubrity of the windward over the leeward side of the West India islands, has been supposed to afford a ready means of obtaining better localities for the troops. But though the windward side may, from its free exposure, be cooler and more pleasant, we are by no means certain it will possess such decided advantages in salubrity. That is a point which yet requires confirmation by numerical results, and we have had sufficient experience in the course of this investigation to put us on our guard against drawing deductions as to the salubrity of stations from any other source. So far as we can learn, however, there are many instances in which epidemic fever has been found to prevail to a very great extent on the windward as well as the leeward side of the islands, but as the white population is less dense in the former, the deaths not being numerically so high have attracted less attention, and we conceive it possible this circumstance may have given to the windward side a higher character for salubrity than it really merits. In Jamaica, the only island in which we have been able to establish the rate of mortality in different directions along the coast, no such superiority in the salubrity of one side as compared with the other has been observed.

Except so far as regards elevation, therefore, it must be admitted that it is exceedingly difficult, if not impossible, to point out any practical rule to be followed in the choice of healthy localities for the troops. The example of Up-Park Camp is sufficient to show that even a position apparently remote from the noxious agencies of swamps and vegetation, with a free exposure, dry soil, and superiority of comfort and accommodation, may, without any assignable cause, prove uniformly unhealthy. It is probable the progress of our investigation into the influence of different localities on the health of troops in other quarters of the globe may open up fresh sources of information on this interesting and important subject, and enable us to appreciate with more accuracy the agencies by which so much sickness and mortality have been occasioned. We shall therefore defer any further observations till we have an opportunity of submitting the other reports now in course of preparation.

ANIMAL MAGNETISM.

To the Editor of the Medical Gazette.

Sir,

As your journal is justly celebrated for its impartiality, I venture to offer to my professional brethren, through the medium of its columns, a few plain suggestions upon the subject of animal magnetism, which I hope you will honour with a place in your next publication.

I have read the letter of Mr. Terry, of the Northampton General Infirmary, which appeared in your journal of the 21st instant; and I have also met with similar opinions expressed by other gentlemen of equal celebrity; and I have heard, in conversation with physicians and surgeons, the most sweeping condemnations and unqualified denials of all the effects and facts said to be produced by this new agent of mesmerism, or new operation of magnetizing. And though some of the effects and facts are extraordinary in the highest degree, yet those sweeping condemnations and unqualified denials appear to me to be equally extraordinary and strange, and, in my humble opinion, unphilosophical, and consequently but little worthy the consideration of intelligent men.

Now, allow me to suggest to the opposers of animal magnetism the propriety of adopting the same proceedings with the statements of the facts and effects of it, as

they would do with the statements of the facts and effects of any other new principle in natural philosophy, physiology, &c.

Whoever hears, or even reads in works of credit and celebrity, for the first time of these extraordinary facts of zoomagnetism, cannot possibly give implicit credit to them; nor does he give implicit credit to facts equally extraordinary, if new, in chemistry, galvanism, &c. &c.; but mark the difference in pronouncing a judgment. In the former case the facts are at once denied, every one feeling himself competent, without previous inquiry, to decide; in the latter case examinations and experiments are made before judgment is pronounced. But it may be said, and with truth, that the effects produced by animal magnetism, even when witnessed, are so extraordinary, that the spectator is disposed to doubt the evidence of his senses, and to put them all down as the result of charlatanry and delusion, and therefore that my comparison is inadmissible. I will grant to the opposers of this new principle the full and most unlimited force of this objection: but I now beg leave to make this other suggestion, as a final disposer of all objections, and as the means of quieting all scruples and clearing up all doubts.

Every man has in his own hands, nay, actually and literally possesses at his fingers' ends, the unerring test of the truth or falsehood of animal magnetism; and if he who doubts, disputes, and denies the magnetic phenomena, will but put them to the test of his own experience, by actually practising upon a few persons of *either* sex, subject to epilepsy and chorea, he will produce, within three months, a great many of these extraordinary effects, and convict himself of the error he commits in a premature condemnation of effects and denial of facts, that will start up before his astonished eyes. But as the higher effects of somnambulism seldom take place upon any one until after some few months' application of mesmerism, the experimenter must take a longer period than three months to put the truth of *clairvoyance* and *prévision* to the test of his own powers and practice.

Let me not be thought anxious to betray any one, or lead him into danger; for I must add, that the experimenter will find animal magnetism as powerful in itself, and as injurious in its *misapplication*, as opium or prussic acid; and that it may, like these powerful agents, be used in works both of good and evil.

If, on the one hand, my professional brethren prudently and wisely refuse to adopt every thing that is recommended to them, without previous inquiry and investigation, it is, on the other hand, equally due to the honour and reputation of others that they should not refuse, deny, and condemn that which they have not subjected to the same severe criticism and research.

Let any one, now denying the phenomena of zoomagnetism, once produce, by his own powers, the effects attributed to that agent, and he will immediately *feel* the necessity of giving credit to the statements of other magnetizers, in order that his own may be believed; at least he will no longer deny that the effects which he himself has produced, may also have been produced by others.

This letter, written hastily, may, I fear, appear too defective to gain admission in your publication; but the few minutes I can occasionally spare during the day, will not allow of a deliberate composition;—look it over, I beg of you, with an indulgent eye.—I am sir,

Your obedient servant,

JAMES BIRCH SHARPE,
M.R.C.S.

Windsor, July 24, 1838.

ALLEGED NEW MODE OF CURING CATARACT.

To the Editor of the Medical Gazette.

SIR,

OBSERVING at a book-stall a work entitled, "A Treatise on the Physiology and Diseases of the Eye; *containing a new Mode of curing Cataract without an Operation*," I was induced to purchase it by the importance of the announcement made in the last clause. As soon as I had returned home and entered my library, I opened the book, and turned with some curiosity to the chapter on cataract, which extends from page 110 to page 122. All the information which the author deigns to give the reader in this short chapter may be found in any of the elementary works on surgery; but at page 117 appears the following paragraph, which I should be much obliged by the author's explaining. Since it contains the essence of the new mode of curing cataract without an operation, I think it much too cavalier-like conduct on the part of Mr. John Harrison Curtis, oculist, who professes to be the author, to tantalize his readers with such a brief and enigmatical communication. The passage which I cannot comprehend runs as follows:—

"In all cases of incipient cataract, I should recommend occasionally a moderate abstraction of blood from behind the ears, and the application of a small blister to the nape of the neck, or behind the ears, which should be kept open some weeks with an ointment. Alteratives and ape-

rients may be given, and sedative lotions applied to the eyes; with now and then warm fomentations of poppies. After the chronic inflammation is subdued by these remedies, the cataract is to be *touched* every morning with a solution of the potassa cum calce, beginning with a weak solution, and increasing it gradually. It should be applied with a camel-hair pencil; and I beg to observe, that the dissipation of cataract depends much on the way in which the solution is applied. The eyelids must be kept open during its application, and the *centre of the cataract alone* be touched, otherwise part of the caustic will adhere to the eyelids, and occasion unnecessary pain, besides defeating the object. Great care should also be taken to use only one brush for each patient. In the incipient stage of cataract I am convinced much good may be done, and a cure effected; but when the disease is become confirmed, and the patient is old and feeble, little is to be expected, and the risk of an operation had better always be avoided."

What I wish Mr. Curtis to tell us is, *how* he contrives to touch the cataract with his potassa cum calce? In common with all his readers I feel in a dilemma. Am I to suppose that he has a secret which he keeps in the back-ground, or that, in the nineteenth century, he has had the impudence to publish a book on a disease which he has never seen?—I am, sir,

Your constant reader,

VETUS.

Albemarle Street,
July 9, 1838.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO HAVE RECEIVED DIPLOMAS.

June, 1838.

J. Nance, Portsmouth.—S. R. Robinson, Gloucester.—C. Webb, Basingstoke.—H. C. Atlay, Stamford.—G. M. Johnston, Nova Scotia.—W. H. G. Buckley, Bradford.—T. Stainthorpe, Hexham.—F. J. Lanigan, Ballymahon.—A. Romer, Judd Street, Brunswick Square.—D. R. G. Walker, R.N.—T. W. Kirby, Leamington.—J. Hawkins, Newport, Monmouth.—J. S. C. Wilkinson, South Croxton.—J. Hiley, Elland, Yorkshire.—Thomas Humble, Newcastle-upon-Tyne.—T. Mawe, Tralee, Kerry.—Gavin E. Pocock, Brighton.—Bicton Hull, Plymouth.—R. Higham, Whalley, Lancashire.—James Irons, Clones.—R. C. Anderson, Myshall, Co. Carlow.—J. Macintire, E. I.—W. Loney, Cashell.—C. Taylor, London.—J. Clarke, Penrith.—J. J. Power, Maidstone.—C. M'Nish, Demerara.—R. S. Nightingale, Liverpool.—J. S. Clarke, Islington.—J. A. Bostock, Upper Bedford Place.—M. Knight, Euston Place, Euston Square.—P. Burrowes, Demerara.—W. Thurnall, Duxford, Cambridge.—J. Arthur, Limerick.—R. T. Cappur, Nantwich.—G. S. Lidbetter, Tunbridge Wells.—J. T. Hussey, Roscommon.—E. Walker, Ardmore, Boyle.—T. C. Burton, Queen's Square, Bloomsbury.—J. Philip, Aberdeen.—S. E. Piper, Ipswich.—H. A. Rawlins, Lincoln.—H. S. Taylor, Liverpool.—M. Creagh, Ennis.—E. Barry, Kerry.—H. Smith, Plymouth.—A. Evans, Mile End.—J. E. Boyton, Cork.—S. Manning, Wicklow.—H. Saxton, Brlstol.—W. H. Duff, Islington.—J. Mantove, Nottingham.—Saint John W. Lucas, A.—S. Osborn, Bishopsgate Street.—H. O'Hara, Dublin.—E. I. Walsh, Trinidad.—T. Foster, Manchester.—E. Knowles, Soham, Cambridge.—H. Child, Hereford.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *July* 90, 1838.

Abscess	1	Fever, Typhus	1
Age and Debility	23	Heart, diseased	3
Apoplexy	5	Hooping Cough	2
Asthma	1	Inflammation	7
Cancer	1	Bowels & Stomach	5
Childbirth	1	Brain	4
Consumption	34	Lungs and Pleura	2
Convulsions	20	Insanity	3
Croup	1	Liver, diseased	1
Dentition or Teething	4	Mortification	2
Diarrhœa	1	Scrofula	1
Dropsy	3	Small-pox	21
Dropsy in the Brain	3	Thrush	2
Dropsy in the Chest	1	Unknown Causes	68
Erysipelas	2		
Fever	17	Casualties	2
Fever, Scarlet	2		

Increase of Burials, as compared with the preceding week . . . 4

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

July.	THERMOMETER.		BAROMETER.	
Thursday . 12	from 58	to 78	30·00	to Stat.
Friday . . 13	58	80	29·96	29·90
Saturday . 14	59	67	29 74	29·61
Sunday . . 15	56	70	29·66	29·80
Monday . . 16	52	71	29·94	29.98
Tuesday . . 17	48	70	30·11	29.98
Wednesday 18	58	69	29 98	30.17

Wind, S.W.

Except the 14th and morning of the 18th, generally clear; rain on the 14th and following day.

Rain fallen, ·45 of an inch.

Thursday . 19	from 51	to 75	30·16	to 30·07
Friday. . . 20	54	72	29·96	29 94
Saturday . 21	51	70	29.94	30·00
Sunday . . 22	50	63	30·60	30·03
Monday . . 23	42	68	30·08	29·98
Tuesday . 24	41	64	29·88	29·91
Wednesday 25	42	60	29·93	Stat.

Wind, N.W.

Generally clear, except the 23d and two following days, when rain fell.

Rain fallen, 0875 of an inch.

CHARLES HENRY ADAMS.

ERRATA.

In our last number but one, p. 646, col. 2, line 32, *for* "magnetized" *read* "unmagnetized;" and in p. 648, col. 1, at bottom, *for* "lilac *(corchorus kerria*, or *corchorus Japonica)*," *read* "lilac, corchorus *(kerria*, or *corchorus Japonica)*."

WILSON & SON, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 4, 1838.

THE CROONIAN LECTURES,

For 1838 (*concluded*).

By John Clendinning, M.D., F.R.C.P.

EXPERIMENTS AND OBSERVATIONS RELATING TO THE PATHOLOGY AND PATHOLOGICAL RELATIONS OF THE HEART.

Condition of the visceral enlargements in morbus cordis.—It appears probable, then, that morbus cordis is usually complicated with general visceral hypertrophy, as well from the notices of coincidence between cardiac and other visceral enlargements to be met with in pathological and practical writers, as from my own observations.

It appears further probable, as I conceive, that the heart disease is in such cases the cause of the abnormal density that is to be observed in the other viscera. Now, assuming this to be the fact, it is an important and difficult question what is the modus operandi by means of which morbus cordis is enabled so strikingly to augment the nutrition of the lungs, and of the viscera of the head, and of the abdomen. On this question some observations were hazarded in my communication to the R. M. C. S. L. already referred to. In those observations I mentioned two circumstances, to one or other of which I thought the effects under consideration might in part be referred. Whatever may be my private feelings on the subject, I freely admit that I am not *sure*, in the logical sense of that word, that I then succeeded by means of those circumstances in making out a satisfactory explanation of the facts, or an explanation, even *partially* satisfactory, in every respect. But I am not discouraged by that consideration from submitting those circumstances again, with additional illustrations, to the attention of critical hearers.

The circumstances referred to are these. In a considerable class of cases the principal change in the diseased heart is to all appearance simple increase of thickness without diminution of area in the left ventricle. This state of that most important part of the heart has been pointed out by several authors; for example, Professor Richerand in France; and Dr. Copland and Dr. Hope in this country, as highly favourable to over-excitement and hæmorrhage in the brain, and to inflammatory and febrile disease generally. Now a very obvious and but slight extension of the views of these writers would go a good way towards explaining general visceral hypertrophy in such cases. A state of the heart differing from the normal principally in greater mechanical facility for the abundant supply of nutrient fluids, would almost of itself authorize an *à priori* suspicion at least, if not an assertion, that the inconveniences attending hypertrophy of the brain, liver, kidneys, &c. might be expected to occur in cases of thickening without enlargement or without contraction or valvular defect in the left ventricle of the heart; and these inconveniences are actually found to take place under the circumstances supposed; so that if enlargement of the viscera were a concomitant, or consequence, of active aneurism of the left ventricle, to use Corvisart's name, and of that state of the diseased heart alone, there would probably be little occasion to hesitate as to the cause of the phenomenon.

These circumstances insufficient, shewn from morbid anatomy.—But when we inquire whether general visceral enlargement is limited to those cases of morbus cordis in which the condition of the systemic heart is most favourable to the general distribution of

arterial blood;—when we learn that the hypertrophy of the viscera is a regular feature of morbus cordis, with valvular disease, aortic disease, &c., we then feel the necessity of looking for some other principle besides excessive arterial supply, to explain the excessive development of the viscera. Now to meet this new difficulty, no principle, simple, and almost obvious, like that of undue arterial activity, presents itself, and the solution that has seemed most plausible to me is this:—Nutrition must be effected from blood, and if not from arterial blood, as it usually but not invariably is in the mammalia, then it must be effected from venous blood. Now bearing this in mind, and taking into account the state of the pulmonary passages, and of the return circulation, we may, I think, get a certain length with tolerably firm step towards an explanation. What is the condition of the lungs in morbus cordis, with unequivocal disease, such as considerable contraction, induration by osseous deposits, firm vegetations, &c., of the mitral or aortic valves? The conditions I have in every such case I think met with are, exclusive of the head and abdomen, the following, viz. the air-passages have always been in colour between a dark red with whitish lines, and a uniform dark red—Modena red, I think, colourists call the shade. The large tubes have otherwise appeared more nearly normal than the small ones; the latter having been changed in various ways. In the first place they were charged usually with mucous, not uncommonly puro-mucous fluids, that often seemed to fill their whole area. In the next place there was usually thickening of the parietes of those smaller ramifications, a thickening so considerable as not rarely to close nearly, or even quite close up, as it appeared, the calibre of the vessel. This state of the bronchial ramifications has, I think, been often mistaken for phthisical degeneration, owing, I imagine, to the striking resemblance between the appearance presented by the amputated air-tubes projecting on the incised surfaces of the sliced pulmonary lobes, to what may be easily imagined, and has actually been observed to be, the appearance of granular or miliary tuberculations under similar circumstances. But sometimes the thickening of the air-tubes was not what I may call concentric, but was eccentric or expansive, giving to the air-tubes a larger diameter than normally belonged to it: and air-tubes so expanded usually terminated in the vesicular dilatation that characterizes what Laennec has so infelicitously entitled Emphysema pulmonum. But whatever the form or degree of the tubular hypertrophy, there was in most cases more or less of the vesicular dilatation accompanying it.

In addition to those morbid changes, there was, further, in most cases, more or less compression of the lungs upwards and backwards by the enlarged heart or liver, or by hydrothorax and ascites, or by a tympanitic state of the stomach; and in many cases several of these embarrassing circumstances, and in a few nearly all, existed.

Now under such conditions in the air-tubes in particular, and body of the lungs in general, it would seem scarcely practicable that a proper circulation and ventilation of the blood should take place. The bronchial and vesicular mucous surfaces were covered, as it were, with a thick coating of glutinous varnish. The mucous membranes were themselves in a hypertrophous condition, and probably less apt for functional action. The air-tubes had to some extent lost their contractility or elasticity, by which they were previously enabled to accommodate themselves to the act of expiration without puckering or folding. In some parts they had lost in area, in others, on the contrary, they had expanded; but in both cases they had lost facility of adaptation to the varying condition of the lung, and had consequently suffered in functional efficiency. Besides which, the mass of permeable lung, as a whole, was diminished by external impediments to expansion on one side, and internal obstacles to contraction on the other; and the general result was, it may be supposed, an irregular obstructed pulmonic circulation, an imperfect decarbonization or oxygenation, and a mass of vascular humours falling below the average of health in purity and salubrity.

Shewn also from cardiac semeiology.—Now whether such be a true account or not of the condition of the blood in the worst sort of organic cardiac diseases, it is an account, at least, that quadrates very strikingly with numerous other phenomena indicating some such state of the humours as is above sketched. As for external appearances, every practitioner of experience is familiar with blue or purple coloration of the lips, nails, often of the cheeks, of the glans penis, of the feet, hands, and other parts of cardiac subjects, for hours, days, and even sometimes for weeks or months, before the agony; and with regard to internal evidences, reference may be made to the frequently intense coloration of the air-passages, interior of the stomach, whole body of the liver, spleen, kidneys, brain, &c. and to the extreme infarction by blood of the lower lobes or whole of the lungs. Thus experience seems to

come to the aid of hypothesis, in sustaining the supposed average inferior crasis and salubrity of the blood.

In the case, then, of a person labouring under extreme hypertrophy of the heart, with or without valvular disease, but with the usual chronic bronchitis, vesicular dilatation, and tubular hypertrophy, &c. &c. there seems to be a necessity not only for a sluggish circulation, but also for an imperfect arterialization of the blood, and for a new modification of the relations of the organs to their habitual nutriment, in order that they should be able to appropriate, without immediate injury at least, the deteriorated fluid. In a word, that there should be a resumption of the capillary susceptibilities, crasis of blood, and mode of organic nutrition, that probably ordinarily exist to some extent in the liver, but that cease to be general, as during fœtal existence, after the extrusion of the infant from the uterus.

It might, indeed, be supposed that a diminished energy of the absorbent function would account for increase of visceral weight and volume, without any change of nutrient action. But I have not observed any material diminution, in many cases, of either ingesta or egesta, nor any other fact that would render probable a diminished visceral interstitial absorption. There remains, then, only an increased supply, by abnormal stasis, of insufficiently organized blood, to which to refer the visceral abnormal conditions under consideration; and that something such as I have urged does actually occur, one fact at least, of a peculiar character, and in which the circulation must throughout life have been but imperfectly arterial, even in the aorta, offers itself to my recollection.

Illustrative case.—In the year 1835, I had an opportunity of examining the body of a female, aged 23 years, in whom the foramen ovale was open sufficiently to admit a good-sized quill or pipe-stopper. Her face, extremities, and the greater part of her surface, indeed, were livid, and her dyspnœa was extreme. In her the heart weighed 15½ oz. and was universally hypertrophous; the encephalon was 44 oz., or nearly 2 oz. above our average for her age and sex; her liver weighed 55 ozs. and her kidneys 10½ ozs.; both glands considerably more than the normal averages for life for the female. All her viscera were much loaded with blood; her lungs were in a state of extreme emphysema. (I have unfortunately not taken down the weight or volume of the other viscera.) Now this patient had laboured under dyspnœa almost all her life, or since she could recollect any thing; and in her the viscera and whole person, which was fleshy, must have been nourished by an arterial fluid of a semi-venous character.

But organic and functional defect in the lung is apparently not the only influential element in the causation of the deteriorated distribution and crasis of blood that appears to attend advanced stages of that disease. There is, in the heart itself, a defect of vital power and contractile energy, to which I suspect much of the hæmostasis and habitual visceral engorgement must be attributed. There is, I have long thought, in the distinction of the excellent Corvisart, of active from passive aneurism of the heart, an *apparent* error at least of considerable importance.

Hypertrophy often united with adynamia.—Excess of nutrition does not necessarily include augmented vital or functional power: so far from that, that I imagine there can be no doubt that in hypertrophous organs the functional energy is rather in an inverse than a direct ratio to the increase of nutrition. The hypertrophous brain is more subject to the disturbances and disorders that cause headache, convulsions, coma, delirium, palsy, &c., than the normal brain. The hypertrophous liver is peculiarly susceptible of pain, peculiarly liable to icterus: witness the frequency in advanced heart cases of those conditions. The hypertrophous stomach digests not more vigorously, but much less so on the whole, than the normal, being more liable to the impediments arising from flatus, acidities, nausea, the extremes of bulimia and anorexia, &c. Hypertrophy in the lungs produces nearly proportional pulmonary incapacity. The same condition in the kidneys is attended by a diuresis, irregular, and often in quantity very defective; and when local morbid predisposition exists, not rarely albuminous or otherwise vitiated in composition. In like manner hypertrophy of the heart appears to produce diminished functional aptitude, and this in several ways. Moderate sthenic hypertrophy without valvular defect occurring in a well-nourished subject, may be conceived to produce misdistribution of blood, and consequently abnormal nutrition, either simply, by habitual excess of supply, as already pointed out above, or in a less direct but not less injurious manner; namely, by its frequent irregular action from the right ventricle often overloading the lungs, and disturbing the vital epuratory process of which those are the seat; and by, in like manner, from the left ventricle frequently overcharging the visceral vessels generally, and producing abnormal though transient stagnations and venosities of the nutrient fluids. In cases of moderate irregularity of the cardiac functions, as in young subjects of incipient simple sthenic

hypertrophy of the heart, it may fairly be presumed that the morbid effects of the cardiac disease, as evidenced by visceral enlargement, &c. would be moderate, and in a considerable number of observations belonging to this category in both sexes, I certainly have found it so: *cæteris paribus*, I have always found the visceral enlargements moderate when the heart was still in a condition of moderate simple enlargement; and on the contrary, I have observed almost constantly the irregularities of visceral development in subjects, not exceeding 60, or perhaps 65 years of age, to exceed most the normal average when the volume of the heart itself was most excessive.

Summary inferences.—Increase of power, therefore, with thickening of the heart's walls, and without valvular defect, might be conceived, *à priori*, to be capable of modifying in excess the nutrition of the viscera; and would appear further, *à posteriori*, or experimentally, to have actually that effect. But active aneurism is not a distinct species of cardiac disease. It is only a first stage, and surely, if life be prolonged, passes into passive or asthenic aneurism, or hypertrophy and dilatation, with loss of power. When this transition takes place cannot be stated more nearly or precisely than this; that when the heart has very considerably exceeded its normal dimensions; when it may be supposed to have acquired an excess above the normal average of at least 30 or 40 per cent. in weight or volume, then, if not before, it will generally be found to have lost in energy while it has gained in bulk, and to have begun to be in functional efficiency sensibly much feebler than before its dilatation. The debility of the organ, Corvisart and others seem to have attributed to the thinning of the heart's parietes, but, at least in a practical sense, erroneously; for thinning of the ventricular parietes is rare in morbus cordis, and sluggishness and asthenia interrupted perhaps by occasional spasmodic action of the organ, are very common, and in advanced stages nearly constant, more especially in the left ventricle. The supposed diminution of muscular substance attending passive or asthenic aneurism, is then, I imagine, as regards the left ventricle, either wholly fanciful, or so rare, as to be practically insignificant; interesting, probably, for a museum, but unimportant because singular, or very rare, in clinical practice; and the passive aneurism of the right ventricle is in most cases, I think, apparent only, and is properly an effect of that distension and dilatation to which, as the *ultimum moriens*, the right ventricle is liable during the agony.

Direct evidences of cardiac asthma.—The evidences of this debility of the hypertrophous heart are numerous and clear. Of course, the first that present themselves are those that are afforded by direct observation of the heart's action; and according to my experience I should say—I speak from recollection, and without counting, and assuming the patient to be in a state of repose—that a soft compressible pulse, of a frequency little different from that of health, is that which is most frequently met with, taking all species and all stages of morbus cordis together; and that in the more advanced stage, after hepatic enlargement has become obvious, and pulmonary embarrassment habitual, and after effusion has begun in the lower extremities and abdomen, and the desire for solid food has begun to flag, or has quite failed, and whilst the urine has become scanty, and the susceptibility of temporary relief from diffusible stimuli has become less, while the desire for their use has rather increased;—in such a state of things, which I have known to last many days very often, and for several weeks in not a few instances, the pulse is usually smaller and feebler, as well as quicker than normal; and this in cases in which the parietes of the left ventricle and the area exceed by much the dimensions of health, and in which there has been no abnormal murmur observed during life, nor any valvular defect ascertained after death. And this comparative feebleness and smallness of pulse I have repeatedly observed in cases of certain and excessive hypertrophia cordis, where the organ was ascertained afterwards to exceed the standard average by a quarter of a pound or the equivalent excess in volume; by half a pound, or by three-quarters of a pound; and even where the heart has been twice and a half as large again as the normal dimensions. Now since in all these cases nearly, the left ventricle participated fully in the morbid nutrition, and since in not a few it was the principal seat of hypertrophy, it is abundantly plain that any feebleness, &c. perceptible in the pulse at the wrist, or throb of the heart, must be owing to cardiac debility coinciding with and arising out of excessive nutrition of the heart; and the force of the argument is perhaps increased by the fact, that not a few of the cases on which the statement is founded exhibited no inconsiderable vigour of digestion and assimilation, while they at the same time were under no peculiarly heavy drain by evacuants of any exhausting kind; that, in short, there was good reason to believe that their vascular sys-

tems were, in point of fulness and tension, little different from health.

Of the proofs of the adynamic character of morbus cordis, in many cases from an early period, and in all cases nearly in advanced stages, what I have already stated might perhaps be considered sufficient, at least by those who do not dispute my facts. But I have long held that element of the disease to be one which, in its practical bearings, is of the first importance; and I feel myself, on that account, warranted in dilating and accumulating evidence upon the subject, without, however, thinking it necessary or convenient in this place to enter into any minute detail.

Other indirect proofs.—One source of error connected with morbus cordis is, that where no mechanical impediment to the transmission of the ventricular impulse along the arterial trunks exists, such as rigidity of the aortic valves; aneurism, or loss of elasticity in the aorta; defect, whether functional only or structural. also in the mitrals or aortics, &c. &c;—in that case sudden effort or emotion, or a strong medicinal stimulus, may, and generally will, readily produce vigorous throbbing in the heart, even in cases far advanced towards an unfavourable termination; and such increase of action will, of course, be sensible at the wrist, and may lead, and I imagine often has led, to erroneous and even mischievous practice. Of the truth of this observation I have had repeated experience, and have often felt the importance, so ably stated by Senac*, of being able to make with certainty even an approximatively correct diagnosis in cases howsoever named, that involve hypertrophy of the heart. I am convinced that I am within bounds when I state that I have known many dozens of cases of inflammatory disease, especially pneumonia, and very many cases of fever, and several of mania, which, though of no extraordinary severity in themselves, have proved fatal notwithstanding all the usual means, and wholly owing, as I think, to their being complicated with morbus cordis. That fatal complication told doubly in opposition to remedial means. It had previously, in all probability, occasioned general visceral hypertrophy; so that the condition of the patient was not that of a mere pulmonic, or maniac, or typhoid subject, but was, on the contrary, that of an individual labouring under a complication of diseases involving in one degree or another nearly every vital organ; and, secondly, the excitement communicated to the heart hastened the downward impetus, by augmenting the embarrassments of the general circulation, arising out of the cardiac functional lesion, and by rapidly exhausting the deficient vital powers of the heart itself. Of this I have had, within a few months, a striking example in a case of ague.

Illustrative case.—An athletic man of 38 years of age, and, by his own acknowledgment, given to strong liquor, was attacked with a quotidian ague while engaged as a labourer on the Great Western Railway, a short way from town, and was in a short time after, being obliged to give up work, admitted into the St. Marylebone Infirmary. There was in his symptoms nothing very peculiar for some time after his admission, but pains of the lower extremities of a doubtful nature, like those of sciatica in some points, but in others resembling more closely neuralgia of the great femoral nerves and their branches. Quinine by itself in considerable doses proving insufficient, the liquor arsenicalis was added, and by this combination the aguish attacks were stopped, but the femoral and crural pains continued, and delirium set in at night, and some pyrexia was generally observable during the day. He became at length anasarcous and leuco-phlegmatic, lost all inclination for solid food, and was supported for the last fortnight almost exclusively by beef-tea and brandy, of which latter he took, for many days before death, 12 ounces daily. The pulse at no time exhibited any peculiarity, nor was there any abnormal impulse or sound detected in the cardiac region. About three weeks before his death the pulse began to flag very decidedly, and, in addition to some wine he had previously been taking, a few ounces of brandy were thought necessary. After the increase there was a decided improvement in his general appearance, and in his pulse, which, however, proved transitory; for in a few more days the pulse again flagged, and general adynamia became urgent. Again and again the stimulant was increased, and again and again temporary improvement followed, and at length the quantity of stimulant reached the uncommon amount above mentioned. He sank at last apparently exhausted. There was general visceral hypertrophy, the cerebellum being the only exception; even the stomach, which varies less than any other organ, was heavier by one-third than normal. The heart weighed 14¾ oz., the encephalon 3¼ lbs. or 52½ oz., nearly half a pound, therefore above the normal average. There were soft recent vegetations or shreds of lymph attached to the aortic and tricuspid valves. The kidneys were degenerated in the manner described by Dr. Bright, the pyramidal bodies being nearly undistinguishable. There were slight bloody exudations in various

* Lib. vi. sect. 5.

places, as, under the arachnoid, under the cuticle, in the pericardium, &c.; serum was present in the abdomen, extremities, and cerebral ventricles, in some quantities.

This man never once complained of his chest, even when questioned and examined by the mechanical method. The throb of his heart, as well as of his pulse, was common-place in character, and in point of intelligence he seemed to me always below the average for his sex and age. The deposits on the valves were apparently recent, and had the ear been employed after their formation must, if the circulation were sufficiently vigorous, have produced some modification of sound. But the man survived a fortnight after I had in my own mind lost all hope of his recovery, and I had consequently ceased to make inquiry as minutely as I should otherwise have done.

In this case there can be no doubt, I conceive, that the main cause of death was the functional incapacity of the heart. The visceral hypertrophy in other respects, though well-marked, might have been borne for years. The structural degeneration of the kidneys no doubt contributed more than a simple renal hypertrophy to the unfortunate result; yet I can hardly attribute to it more than a very subordinate share in the event. Whether the urine was albuminous or not was not inquired. Similar to that case a good number have fallen in my way variously complicated, and not rarely masked by other perilous or fatal diseases.

There are three or four classes of cases which have particularly impressed me deeply with the importance of hypertrophy in the heart as a complication in clinical practice:—

1. Pulmonics, including phthisis, pneumonia, and bronchitis.
2. Cerebral affections; some acute, as apoplexy and delirium tremens; and others chronic, as mania, &c.
3. Puerperal peritonitis, and other puerperal inflammations.
4. Fever; typhus, synochus, &c.

CASE OF DISLOCATION OF THE FEMUR ON THE OBTURATOR FORAMEN.

By F. C. Skey, F.R.S.

Assistant-Surgeon to St. Bartholomew's Hospital, &c.

I was requested some weeks since to accompany a medical friend, residing in Herts, to give an opinion on, and superintend the reduction of, a supposed case of dislocation of the hip. After a careful examination of the case, and a protracted and vigorous effort at reduction, I returned to town without having accomplished the object of my journey.

The case appears to me an instructive one, and I make no apology for laying it before your readers, persuaded, through many inquiries I have subsequently made, that such cases are by no means so rare as may be supposed.

Although I am not ambitious of a reputation for candour at the expense of professional competency, yet I willingly contribute the particulars of a case of no very infrequent occurrence, the recognised treatment of which, although sanctioned by general professional authority, has proved inoperative.

A young gentleman, aged 11 years, of a strumous habit, having small eruptions about the angles of his mouth, and marks of recent sores on his hands and feet from chilblains, fell while in active play at school. He was raised from the ground and taken into the house. Four days elapsed before he was removed a distance of a few miles to his father's house. His limb was examined by two highly competent members of our profession, who concurred in opinion that the head of the right femur was dislocated on the obturator foramen; they failed, however, after a well-directed effort, in reducing the dislocation, and the limb remained as before.

I saw the case in the afternoon of the fifth day following the accident, and the symptoms were as follow:—

Flattening over the right trochanter, which was driven inwards and downwards. Right anterior superior spine of the ilium lower than the left by about an inch, as was the whole right extremity generally. The latter was a little everted, and drawn forwards on the pelvis. Slight rotation feasible, though painful. Greater fulness perceptible to the hand on pressure below Poupart's ligament on the right than on the left side; and a marked difference in the beat of the two femoral arteries. On turning him round on his face, the flattening over and behind the trochanter was rendered more obvious. Spine curved to the left side. Unless the abdomen were supported with pillows he inclined to the sound side; by their aid he lay without pain or difficulty. Slight fulness detected by pressure of the fingers along the ramus of the ischium

towards the obturator foramen on the right side. When placed upright on the floor, his right extremity was drawn forwards, his knee bent, and he touched the ground with the distal part of the foot only; the upper part of the right patella nearly corresponding with the lower edge of the opposite bone, the two limbs being not very readily brought in contact.

The nature of the accident could not be questioned for a moment. I had no doubt but that the opinion of my medical friends was correct, and that it was a dislocation of the femur on the obturator foramen.

Considering the age and constitution of the boy, I anticipated no difficulty in its reduction by the hand, without the aid of pulleys, but I found I was mistaken. After extending the limb for five minutes, and endeavouring to raise and evert the head in the direction of the acetabulum, I was compelled to relinquish the attempt, and to resort to the pulleys.

I applied the pulleys to the dislocated limb immediately above the knee, the opposite end being fixed to a staple in the wall, and confined the pelvis by means of a round towel twisted, which crossed over the spine of the ilium, and was fastened to the post of a large four-post bedstead. The boy lay on his back inclining towards his left side, along the end of the bed.

When the extension, which was made in a direction a little outwards from the mesial line of the body, had been continued for about five minutes, and during which it was obvious that the trochanter descended, I endeavoured to raise the thigh close to the upper end. I drew the pelvis towards me for the purpose of crossing the legs. I then pushed the pelvis in the opposite direction, carrying the line of extension more outwards; this failing, I crossed the legs more considerably, and everted the limb, but with no better success. I then placed a towel round the thigh, which I carried round my neck, and raised till I elevated the whole body off the bed.

The extension was sufficiently great to move a large four-post bedstead, with the boy on it, from its position; and it was fixed to the grate by means of a cord. Our efforts, which occasioned great outcry from our patient, had now been continued for upwards of half an hour, and I proposed to cut the rope and evert the limb, suddenly drawing the thigh across the opposite one; but before making this last effort I repeated the former endeavours in a variety of ways. At this period the tackle slipt over the knee, and the joint remained as at first. I then brought the boy to the bed-post, padded with a small pillow, and Mr. —— and I extended with all the force we could command, drawing outwards from the opposite limb. But every effort failed, and the boy returned to bed.

I then proposed that the effort should be renewed on the following morning, when we might have recourse to additional means, more especially as the boy did not appear to have sustained the slightest injury from his temporary suffering. He was lying quietly in bed, nearly free from pain.

On explaining our views to the parents of the boy, they strenuously objected to the repetition of the effort, and declared they would willingly see him go a cripple to his grave, but that no entreaty should obtain their consent to a second trial; they were perfectly assured that all that art could accomplish had been effected—that if it could have been accomplished by medical skill, we should have succeeded in reducing it. I stated that we had done the boy no evil; that he was at that moment in no pain; and that it not unfrequently happened that we were compelled to repeat our efforts a second time, when the attempt was made at a remote period from the accident; and that I had no doubt we should succeed if we could obtain their permission to renew it, pledging my reputation as a professional man that we would do the boy no injury whatever. My medical friends each "followed on the same side," and employed every argument and every persuasion; all of which were met by an absolute denial.

I then entreated permission to bring the boy with his mother to town to Sir Astley Cooper: but it was all fruitless; and the boy, through the affectionate benevolence of his exemplary parents, is condemned to be a cripple to the last hour of his life.

I have given the detail of the treatment adopted, lest it be supposed that I had trifled with the case by the employment of such means as might reasona-

bly be deemed insufficient to the end in view. The extending power I employed was very great, and more especially so when we consider the age and the constitution of the boy. It was persisted in during a period of not less than three-quarters of an hour, and was finally remitted in consequence of the indisposition of the parents to its renewal, if necessity required it.

It is true that the bone had been dislocated for five days; but to what extent is the knowledge of that fact important, as regards the means required for its reduction? I consider very little; neither the bone nor the parts contiguous to its unnatural position could undergo any change prejudicial to its replacement, in so short a period as five days. It was no uncommon practice in the old French school of surgery, after an unsuccessful attempt at reduction, to carry the patient to bed, and devote many days to preparation by low diet, purgatives, and emollient cataplasm to the part affected.

Boyer says, "It often happens that the first efforts to reduce a dislocated thigh are unsuccessful, and we are compelled to repeat them many times before we have obtained the end we have in view."

With respect to the collateral aid of depletion by extensive bleeding, I confess I am not a convert to its indiscriminate application. It appears to me, that the only cases of dislocation in which we are warranted in the resort to it, are those in which the power of muscular contraction is greater than that of judiciously applied artificial extension; and that it is inadmissible as an agent of reduction, in old dislocations, which have contracted new adhesions, in young subjects, and in unhealthy persons of every age and sex.

The quantity of blood required to be taken in all cases will of course depend on the object of the surgeon, the mode of operating, and the condition of the patient under the operation.

If syncope be the object, it may readily be obtained by the abstraction of from 16 to 20 ounces of blood, preceded by tartar emetic or tobacco, provided the mode of performing the operation, and the condition of the patient, be not unfavourable to the change. And this may be accomplished without difficulty by placing him in a nearly upright position, and removing from him every exciting cause unfavourable to the production of syncope. On the other hand, double or treble that quantity will often fail to produce syncope when employed during the act of extension, *which itself is a preventive of it.*

If I employed bleeding, therefore, it would be with the former object in view, and it should be employed during rest, and in conjunction with antimony or other nauseating medicines; convinced as I am, both from general observation and experiment, that, in order to reduce the force of muscular contraction, without producing syncope, *a very large proportion of blood in the body* must be removed from it.

Inasmuch, therefore, as the pulleys effect a slow although powerful action, it appears to me that their application is rarely compatible with the abstraction of blood.

In the above-mentioned unsuccessful case, I did not resort to bleeding, because I expected to have accomplished my end without it, and I did not contemplate the interference of friends. But most assuredly, in the case of a boy 11 years of age, and not of the most healthy aspect, I should be most unwilling to begin by taking away blood; although I am not prepared to say but that, had I foreseen the remarkable decision of the parental authority, I would have left no resource untried. I foresaw no difficulty, for the probability of failure scarcely crossed my mind; and here lay my error:—

1st. The case had been already attempted by two intelligent surgeons, who had failed; and 2dly, I might have recollected that great difficulty had existed in the effort to reduce nearly every similar case I had witnessed, both in hospital and private practice. But I had never seen a case in so young a patient, and I had never known a case of dislocation on the obturator foramen, in which the issue was finally unsuccessful: and yet such cases, as I have before stated, are not unknown. My communication with other medical authorities has obtained for me the acknowledgment of two, if not three, similar results: of these, one was attended by two eminent hospital surgeons. The bone had been dislocated ten or eleven days, and every effort at reduction that experience could suggest was made

without success. A second case occurred to a provincial surgeon in very extensive practice, in whose hands likewise this form of dislocation was attended with similar consequences.

Petit (Sur les Maladies des Os) considers the reduction of this form of dislocation generally *as most difficult;* though he does not afford any satisfactory solution to the difficulty. He refers it to the elongated condition of the muscles, the extension of which requires great power. But, in reality, little extension is required, and this was readily effected in the above case, for I distinctly felt the trochanter major descending sufficiently far to detach the head from its false cavity; into which, however, it was forcibly dragged on the slightest remission of the extending power. The difficulty does not appear to consist in removing the head from the obturator foramen, but in controlling the tendency of the pectoralis and adductor brevis, to draw the limb inwards when the extension is remitted. Hence the advantage of directing the limb across the opposite one, by which these muscles are relaxed.

On reflecting over this, to me, mortifying case, I have the consolation of feeling, that for the result we are not responsible; and that but for the mistaken affection of the boy's parents, his limb would in all probability have regained its natural situation. It is also but a meagre satisfaction to know that his gait through life will be but slightly impaired, when it might have been totally recovered by the exercise of a few hours' forbearance and resolution.

One thing I resolve from this case of dislocation on the obturator foramen, should a similar case present itself to my notice, and that is, that I will make the extension neither outwards nor inwards merely, but by fixing the patient in the sitting posture, *directly forwards;* flexing the leg and thigh almost at right angles with each other, and with the pelvis. By this position I shall extend no muscles, for the gluteus maximus will at all times freely admit that extent of motion forwards; while the psoas and iliacus, the pectoralis and adductors, will be paralyzed by their mutual relaxation.

Charterhouse Square,
July 24, 1838.

ON THE PULSE.

To the Editor of the Medical Gazette.

Sir,

The subject of the action of the heart and arteries, in conjunction with the capillary circulation, has engaged the attention of several of your correspondents, and some very interesting papers have appeared in the Medical Gazette lately. If you think the following extract from a paper on the Pulse, that I read at the King's College Medical Society, in 1836, worthy of insertion in a corner of your valuable periodical, you will oblige, sir,

Your obedient servant,
J. C. Small, M.R.C.S.L.

25, Thavies' Inn, July 25, 1838.

Extract from a Paper on the Pulse, read before the King's College Medical Society, 1836.

The physiology or causes of the pulse, and its different varieties. — Every writer on this subject since the time of Harvey admits that the pulse mainly depends upon the action of the heart, which, by the contraction of the ventricle, expels a portion of blood from this cavity into the arteries; the pulsation of the arteries corresponding with this contraction or systole, as it is termed, of the heart, and the quiescent state of the artery, in like manner, with the diastole of the heart. This point I shall hereafter endeavour to establish. But, bearing upon it, there are several minor considerations which are worthy of attentive investigation, and have given rise to disputes, long and warm, among philosophers, from the time of Harvey to the present age, and even now are causing no little stir, from time to time, in the medical world. Indeed, when we reflect on the interest which is attached to all parts of the circulation, whether from the minute and elegantly arranged fibres which form the heart and propel the blood through the arteries, or the minute ramifications of the capillaries, a subject is offered to the most powerful mind to work upon, at once beautiful, yet surrounded by difficulty.

My first inquiry will be to ascertain if the arteries possess any contractile power, so as to enable them to co-operate with the heart. It has been proved,

as I have just mentioned, that the pulsation of an artery is perfectly synchronous in the heart; in fact, they occur at the same moment. Mark the jet of blood which proceeds from the extremity of a wounded artery, and it will be found to correspond with the action of the heart. From this fact we may draw the following conclusion: that if the heart is the only circulating agent, the pulse should also be synchronous over the whole body; and if we attentively observe this action we shall never find an artery beat slower in one limb than in another, which would happen if the arteries assisted in the circulation. However, with respect to the contractile power of arteries, Dr. Bostock*, after giving the experiments of both parties of physiologists on this subject, says, "We may assume it as an established point, that the blood is propelled along the vessels by the joint power of the heart and arteries." But one of the principal arguments he advances in favour of the opinion is, that after the heart has been removed from the body, the blood was found to be propelled along the vessels. But surely this may happen from the pressure of the surrounding muscles, from the action of the capillaries, in which all arteries terminate, or from a mere tonicity (which power is certainly established) in these vessels.

Again, in those animals and insects without heart, an analogy has been attempted to be drawn between them and man. Now, in order to show how fallacious this is, it may be as well to give a description of this vessel in insects. An artery from the head to the anus constitutes the only blood-vessel in insects. That this is a true heart is proved by its regular contraction and dilatation, and this is easily perceived exteriorly in the transparent thin-skinned larvæ. The passage of oxidized blood into the heart or dorsal vessel is occasioned by its expansion and contraction, which take place synchronally with the respiratory motion of the whole body, and particularly of the abdomen; and these individual motions of the heart are partially produced by its muscular tunic, and partially by the muscles of the wings which bind it to the dorsal plates. So that by comparing the anatomy of these parts and its relation to respiration, with those of the higher order of animals, the hypothesis of analogy at once falls to the ground. Again, their constitution and economy are so totally at variance, that of what exists in one not a rudiment is to be traced in the other; and some important parts of insects are but slightly developed in the higher animals. From this short sketch of the heart or central dorsal vessel of this class, it follows that it is contracted and dilated by its own powers; and this anatomy proves. Jurine, Chabrier, Carus, and Müller, confirm this by their dissections. Let us now consider the supposition that man possessed this power of contraction and dilatation in the vessels, as Dr. Bostock affirms. What would be the consequence of such an action? Simply this:—A mass of blood is thrown into the aorta by the contraction of the ventricle. Now if the aorta contracts at the same moment as the heart, the vessel takes off just so much of the heart's power in effecting the circulation, as the vessel's own power amounts to; and on the other hand, if the artery dilates at the time the ventricle contracts, the spirting of blood from a wounded artery would not be synchronous with the action of the ventricle, which, however, is always the case. The only material difficulty that occurs is, that experiments have been tried by the most eminent physiologists to prove the alternate contraction and dilatation of arteries by the application of stimulants or sedatives to the circulation of the frog's foot; and they say these states may be produced or suspended by this action, and that it is so independent of the heart as to continue when that organ is removed. I conceive the proof, however, very defective in this case: it appears much more probable to suppose it a creation of the imagination of the experimenters, as many others who have looked for these contractions have denied them. Wilson Philip and Hastings attest these supposed facts; but although such great authorities are opposed to my theory, so doubtful an aspect have their experiments, that the most unprejudiced mind, on reflection, will consider them as the mere natural fallacies to which all observations dependent on an extremely minute exercise of the senses are liable.

But on the authority of Hunter I place my reliance to explain the pheno-

* See Cyclopædia of British Medicine.

mena of arterial action; for his experiments, I need not say, were founded on numerous observations, and are supported by many repetitions, and these facts support and prove only the elasticity and tonicity of the arteries; and this action is absolutely competent to explain the circulation. It is a known fact that if you tie the main artery of a limb, the vessel immediately contracts so much as almost to obliterate any trace of its cavity; while, on the other hand, the vessels that rise above the ligature begin to enlarge immediately, and continue their expansion until all parts of the limb are as well supplied by blood as before. The spontaneous dilatation of the arteries is shewn in blushing. The mind in the last process is concerned; the vessels of the face are dilated, and become tinged with blood. We cannot refer this action to the heart, as Bichat does; if so, the heart, the belly, and arms, would be equally affected. Then to what power must we refer it? To the connexion that exists between the arterial and nervous systems, and to a power in certain arteries to expand or diminish under such influence. Numerous and well-authenticated experiments tend to confirm this view of the subject. Mr. Swan, with whom I have had some communications on this point, informs me that he has traced filaments of nerves in various parts of the body to the coats of the arteries. It is generally supposed, he says, that the blood-vessels are furnished with nerves almost entirely by the grand sympathetic; but this is not the fact, for although the aorta be supplied by it, many of the arteries, on the contrary, receive contributions from other sources, and frequently from the nearest branches, and thus their actions become more readily associated with the actions of the part the branches themselves are distributed to. This account of nervous distribution materially assists me in the explanation I have attempted of the tonic contraction of arteries, and illustrates the admirable designs of nature in carrying on the separate functions and actions of our frame.

Although it would be difficult to account for the phenomena of the pulse in hypertrophy and other morbid states of the heart, by the theory which assumes a propulsive power of the arteries, still we can account for such phenomena according to the views I have laid down.

Our next question is, does the heart, when it throws blood into an artery, expand it; and is the expansion followed by a diminution of the vessel? Experimentalists say, no; our sensations say, yes. The circumstance of the artery being denuded of its coverings and nervous connexions, diminishes the decision of the experimentalists materially.

Dr. Bostock says, "the pulse is felt when the motion of the blood is impeded by the compression of the artery, and it is the resistance to this obstruction which produces the pulse." Now I think I need only appeal to observation and experience to refute this decision; for if it were by the compression of the artery that the pulse was caused, it would be a most ineffectual criterion of its action. Again, the arteries may be seen distinctly to pulsate synchronously with the systole of the ventricle; and the jet of a divided artery confirms this fact. It follows from this, that if the heart is increased in action, the arteries are increased in the same ratio; and if the heart is diminished, the same ratio of diminution occurs. The objection raised against this theory is, that a local cause produces a local effect, and that this local effect is the result of co-operation on the part of the arteries to assist in the propelling power of the heart. True it is that if a blister is applied, or if we bruise, scald, or rub the leg, or any other part, we have redness produced in various degrees. Now it appears that these phenomena can only be explained on the supposition of a power existing in the arteries which enables them under peculiar circumstances to enlarge or diminish their calibre. These actions I believe are generally admitted now to be under the guidance of the nervous system; and from the completeness with which it explains the phenomena, I shall assume it.

I need not dilate upon the necessary regularity of the respiratory apparatus; the important part it plays in chemically changing the blood: upon its regularity depends the force of the heart's contractility, and any interruption to the natural changes impedes, though it does not arrest, the action of the heart, and affects the whole circulation.

Our position then is this: we take the arteries as tubes capable of no other power than that of gradual enlarge-

ment or diminution of calibre, (which is called tonicity, and is analogous not to the sudden contraction of muscles, but to what is familiarly known as the tonic contraction of these parts) and the phenomena of the pulse may be accounted for thus:—If the ventricle acts with greater or less celerity, the pulse is quick or slow; as it acts in uniform time or not, the pulse is irregular or intermittent; if there be a greater quantity of blood in the system, and the heart be so excited that the ventricle enlarges itself to the highest degree, and contracts itself to the lowest, it will produce the incompressible pulse; the opposite state of partial dilatation, and of partial contraction, will produce the compressible pulse; the sudden pulse is produced by the velocity with which each separate contraction of the ventricle is effected; lastly, according as the artery itself is dilated or contracted we shall have the small or large pulse.

ON SECALE CORNUTUM.

To the Editor of the Medical Gazette.

Sir,

Should the subjoined remarks on the abuse and use of secale cornutum, appear to you worthy of public notice, perhaps you would oblige me by giving them a place in your respectable journal.—I am, sir,

Your obedient servant,

John Armstrong, M.R.C.S. &c.

Gravesend, July 1838.

It is evident that many conflicting opinions exist among professional men, concerning the effects of this drug, some considering it useful, whilst others regard it as either inert or injurious. For several years I have been endeavouring to ascertain its true properties as an obstetric agent; and the results of these observations, which have been not a few, I now beg leave to submit to the profession. I would just add, that every statement made in this paper has been deduced from repeated observations, and as much scrutiny and discrimination as I could bring to the task.

The abuse of this drug may form the first part of our remarks; the proper and suitable cases for its administration, and the best mode of administering it, the last part.

The opinions of many highly respectable practitioners are before the profession concerning this medicine, and as some of the opinions contained in this paper may be found at variance with theirs, it is intended not to advert to names, but simply to state the result of my own observations.

The first class of cases in which I think it is abused, is abortions: in these cases, at all times, under all circumstances, it has been tried, and I venture to affirm very few persons can have felt pleased with its effects. If given in an early stage, before the os uteri has sufficiently opened, it will be found to produce an increase of the pains, to diminish the discharge, and to intercept the expulsive process: these effects are not of long duration: for after an hour, sometimes more, sometimes less, if the ergot be not repeated, the pains subside, to be renewed again naturally in a few hours, with the discharge, &c. as previous to its exhibition. It seems almost a contradiction to say that the pains are increased while the uterine action is interrupted. The following case will perhaps place the matter in a clear point of view:—

Mrs. H. has had two children; is now about the fourth month of utero-gestation; has had slight uterine hæmorrhage, attended with occasional pains: supposing these signs of no importance, she made no application for advice, until two days after, when the discharge had become considerable, with pains, as she said, "as bad as labour pains." Assistance was now sought; and on examination the os uteri was found dilated so as to admit the point of the finger. Conceiving that the ergot here would soon settle the business, a dose was administered: in about twenty minutes the pains were increased, and hæmorrhage partly subsided; but the os uteri, instead of dilating, had become rigid, and the contents of the uterus no more forward than previous to its administration. Another dose was administered with similar effects, and another with no better success; the os uteri was rigid, the ovum "*in statu quo*," and the *abdominal muscles contracting* with great expulsive power. After a little time, finding there was no prospect of getting the ovum away, a strong opiate was administered, which quieted this irregular, painful, and unavailing action. Next evening I was summoned again; and still relying upon the statements of

a high authority in these matters, I provided myself with a fresh stock of the ergot. I found the uterus, hæmorrhage, and pains, very much as they had been when I first saw the patient. The ergot was again administered with precisely similar results—viz. rigidity of the os tincæ, diminution of discharge, with violent expulsive efforts of the abdominal muscles. An opiate was again given, followed by an aperient, and when the natural efforts returned they were attended to in the ordinary way: the contents of the uterus were expelled, and the patient recovered favourably. I could mention several instances of this kind, but I hope that this case will be sufficient to shew that the early use of ergot in cases of abortion is calculated to protract and increase the patient's sufferings, to disappoint the expectations of the attendant, and may place him in a most perplexing situation; for the friends and patient, momentarily expecting, from the violence of the pains, the termination of the affair, begin to fear that something must be wrong; and it will require no small confidence in the attendant to be assured that their fears are groundless.

The next order of cases in which it may be improperly administered is the first stages of natural labour. A medical man is sent for to attend a patient at some distance: when he arrives, he finds the pains, like angels' visits, "few and far between;" the os uteri may be dilated to the size of the disc of a shilling; the presentation is natural, but it is likely to detain him, if left to natural efforts, for several hours: he has cases of great importance elsewhere. The ergot presents itself as a likely auxiliary: a dose is administered; the woman complains of pains going all round her—there is no cessation of them; she feels as if they did no good, but her pains are intolerable; she wishes with tears she never had taken the medicine; she was doing well before. The accoucheur examines: the os uteri is rigid, no more dilated than before its administration; he is perplexed, annoyed, and, after some time of further ineffectual suffering, he is glad to give her an opiate; or perhaps patient and friends become dissatisfied, and he is dismissed. This is not an imaginary picture. A few years ago I was requested to see a patient under precisely similar circumstances; the practitioner was an intelligent, respectable man. I found the woman in great distress, both of body and mind; the pains were incessant, and there was little or no effect produced on the uterus; they seemed entirely the efforts of the abdominal muscles: her pulse was quick, the abdomen tender, and there was considerable heat of "the parts." I bled her immediately, and when about six ounces were taken she fainted: 40 drops of Tinct. opii were given, which were repeated in an hour; in another hour she was asleep. An aperient was administered next morning, after which she seemed comfortable. A few days after, before I could get to the house, a fine healthy fœtus was expelled, and the woman recovered favourably.

These are not the invariable consequences of the too early exhibition of the secale. If the membranes be ruptured, after its administration, the labour will be accelerated; and should the liquor amnii be freely evacuated, the child, in all probability, will perish, particularly if a first child. This is easily understood. The first effect of the ergot is, I conceive, to produce firm contractions of the abdominal muscles, which, by acting on the uterus, excite the increased efforts, and the head of the fœtus, or the funis, or both, are so compressed as to destroy the life of the child. At other times, the head of the fœtus, which was presenting naturally, will be forced into a different position, so as to render the delivery extremely difficult, and perhaps to require the aid of instruments, and also very much to increase the probabilities of a rupture of the uterus. There are very few instances, indeed, in which it may be exhibited early with safety; and especially in first labours its use is rarely indicated, and requires sound judgment, with a good deal of practical knowledge.

A gentleman was engaged to attend a lady in her first confinement; the pains came on regularly, the presentation was natural, and every thing seemed to promise a favourable issue; after several hours of suffering, the anxieties of patient and friends became so urgent, that he thought right to give a dose of the ergot. Very soon the pains became increased; and after a couple of hours, finding the labour was not terminated, the friends and patient became alarmed, and another practitioner was called. Need I add the

sequel? The head of the fœtus was forced into an unnatural position, the patient was in a very exhausted state, the forceps were used, and a fine fœtus extracted, which had obviously been dead for some time. The woman recovered slowly, and with so much aversion to her former attendant, that he has lost the attendance on the whole family.

I was informed of another case from unquestionable sources:—A gentleman, while attending lectures in London, had a case of midwifery assigned to him, and when he was called, not finding it proceed with sufficient rapidity (?) he gave a dose of ergot; the pains became alarmingly strong, and after some time suddenly ceased; the woman became faint and sick. It is needless to add, the uterus was ruptured. All was done which could be done. The patient died the second day after. The indiscriminate, injudicious use of this drug has been productive of more fatal effects than many persons are aware of.

It is scarcely necessary to say that, in cases of deformity of the pelvis, of disproportion between the head of the child and the pelvis—as in cases of unusually large head—although the pelvis may be of the proper dimensions in presentations of the feet, or shoulder, or hand, that it would be great rashness to use this drug. In my next I shall endeavour to point out those cases in which it may be used with propriety and safety, and also the best mode of administering it.

ON THE MANUFACTURE OF MURIATE OF AMMONIA.

To the Editor of the Medical Gazette.

Sir,

I shall feel obliged by the insertion of the following communication in your valuable periodical,—on two of the products in the manufacture of muriate of ammonia. It will render the subject more intelligible and complete, if a concise outline of the manufacture of sal ammoniac be first given, just as I have witnessed it at the manufactory of Messrs. Bush and Co., Bow Common, Essex, and which, I believe, is the process at present very generally adopted.

Gas liquor is the source from whence this salt is procured; to it chloride of calcium is added, when a copious precipitation of carbonate of lime takes place, muriate of ammonia being left in solution. The whole of this is put into a tub, having holes in the bottom to allow the solution to drain through, leaving the solid particles behind. This solution is evaporated at a gentle temperature in iron tanks, when it yields impure crystals of muriate of ammonia, of a brownish colour. The salt is then dried, and the water of crystallization driven off in a long iron vessel, very similar to a sand-bath. It is now placed in an iron subliming pot (previously coated to the extent of from one to five inches in thickness, with a composition of common clay, sand, and charcoal), capable of holding about 5 cwts. This is covered by a dome of lead, with an aperture at the top, in which a stopper is placed, by the removal and appearance of which the manufacturer judges of the progress of the sublimation. A gentle fire is kept up under the subliming pot for seven or eight days, when the dome having cooled down, and the sal ammoniac somewhat contracted, so as to loosen from the sides, the dome is thrown off from the iron pot, and about 2 or 3 cwts. of white, semitransparent, muriate of ammonia are knocked off in cakes.

Some manufacturers employ sulphate of ammonia (procured by adding sulphuric acid to gas liquor, evaporating, crystallizing, &c.) and common salt, mixed together, and placed in the subliming pot; muriate of ammonia being sublimed, and sulphate of soda remaining behind.

Whichever process is followed, it has been observed that the cakes of muriate of ammonia sometimes are marked by brownish streaks or bands: these are attributed by the manufacturer to the carelessness of the workmen falling asleep in the night, allowing the fire to go down considerably, and then suddenly raising the heat.

These coloured bands have been generally considered as dependent upon the presence of chloride of iron; but (Mr. Pereira was the first, I believe, who observed that) on adding a solution of ferro-cyanuret of potassium to a solution in distilled water of the coloured muriate of ammonia, no Prussian blue is found, unless a few drops of nitric acid be added, when a copious precipitate takes place immediately.

I took a portion of the coloured salt, and crystallized it two or three times; having by this means divided it into two portions—crystals and the mother liquor. The former were first examined, and found to consist solely of muriate of ammonia, without any iron; the mother liquor, on the contrary, though it yielded no precipitate on the addition of ferro-cyanuret of potassium, gave an abundant one of Prussian blue, when two or three drops of nitric acid were added. A solution of pure potassa did not precipitate oxide of iron until a slight quantity of nitric acid was added. Muriatic acid and ammonia were both detected by their appropriate tests in this solution; so that the fair inference appears to me to be, though it cannot be considered as proved, that this coloured band is a double chloride of iron and ammonium.

On the outside of the cakes of muriate of ammonia a grey-looking crystalline substance is frequently found between the cake and the leaden dome. This substance is scraped off, as impurity, previous to the sale of the salt. If a portion of it be dissolved in distilled water, it will yield with iodide of potassium a yellow precipitate of iodide of lead.

A portion of this grey salt was dissolved in distilled water, crystallized, and re-dissolved and crystallized three times: the crystals were needle-shaped.

A solution of these crystals yielded no iodide of lead on the addition of iodide of potassium; but on passing a current of sulphuretted hydrogen gas through, a precipitate of sulphuret of lead ensued, which, on being dissolved in nitric acid, evaporated to dryness, dissolved in distilled water, and tested with iodide of potassium, afforded a yellow precipitate of the iodide of lead. Potassa, carbonate of ammonia, and sulphuric acid, produce no precipitation in a solution of these crystals.

The presence of ammonia and muriatic acid is readily ascertained in this salt.

The mother liquor of these crystals yielded an abundant precipitate with iodide of potassium, also with a solution of potassa. Muriatic acid and ammonia were detectable.

Chloride of lead is most probably contained in mechanical mixture with muriate of ammonia in the mother liquor, but the crystallized salt is very likely to be a double chloride of lead and ammonium.

The iron in the first salt most likely is contained as impurity in the rough muriate of ammonia previous to sublimation, and the lead in the latter compound is obviously derived from the dome, which is made of lead.

I remain, yours truly,
GEORGE HENRY JACKSON.

30, Church Street, Spitalfields,
July 30th, 1838.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Elements of Clinical Medicine and General Pathology; with a Sketch of the Origin, Progress, and Prospects of Clinical Medicine and Surgery, and an Exposition of the Creeds of Materialism and Vitalism, &c. By J. STEWART THORBURN, M.D.

THIS work is remarkable in several respects. It shews extensive information, and is written in a quaint and original style; but, with all, it is lamentably defective in method and perspicuity. It is full of strange conceits, rendered yet more strange by the typography, which, though really beautiful as a mere specimen of printing, is yet such as to give a motley and patched appearance to the whole: thus we are told that surgery has been elevated from its "state of *barber*-ism" (see p. 82); that young practitioners must be trained to their various duties "when fairly set down at the BACK*GAMMON*-BOARD of private practice." Hundreds of similar specimens might be adduced. Neither are the eccentricities confined to the mere mechanical parts of the composition; but they extend throughout to the terms employed and general construction of the language. Thus Section XI. is entitled—

"Of 'Symptoms,' or Fallings-with, in general; and of Coincidence-discourse, or, 'Symptomatology,' technically so called."

With all these drawbacks, there is much originality of thought, and there are many sagacious observations connected with clinical medicine, scattered through the work. There are also some very pertinent observations on

operative surgery, with one of which—which also serves to illustrate the author's peculiar style—we close our brief notice of the work:—

"Whatever is done well enough, is done quick enough. Operative surgery constitutes no exception to the Roman adage, *sat sito, si sit bene.* Most erroneous ideas are entertained by others than merely the youngest students, that the more rapidly tumors are excavated, condemned legs and arms severed, diseased mammæ dug out, and the affected glands torn away, the better for the patient, and the cleverer the surgeon! No mistaken opinion can probably be more banefully injurious in its influences upon the constitutions of unhappy sufferers. Assuredly no criterion of the abilities of a surgeon is more fallacious and less complimentary than the precipitate celerity with which the various steps of a chirurgical dismemberment are begun and ended by him. What may be termed *foudroyantes* operations, are worthier of the charlatan and of the juggler than of the philosophical and humane surgeon.

"Opinion would seem, among other things, to dictate the exercise of the utmost possible degree of rapidity, in performing operations in the surgical amphitheatre. It would be as idle to characterize such a state of things, as it would be difficult to say how far the rapidity of the manipulations of knivesmen are influenced by the conduct, and consequent opinion, of lookers-on, who take out their watches as regularly as the operator takes up his knife. Cutting, literally, against time, and thereby endangering the lives of the subjects, inside and out, by an emulous Jehu, sparing neither wind nor limb in seeking to outdrive his rival, retained upon an opposition stage-coach establishment, may be so far excusable, from the probable benefits resulting from the temporary excitement, and the improvement of the breed of horses, in this agricultural and macadamizing country. But if the knivesmen of two rival hospital schools allow themselves to be goaded by such feelings, and cut as literally against time, to the hazard of the recovery, and to the certain injury, whether appreciable or not, of the healths and constitutions of *their* subjects—such conduct, by all who take the ground of common sense, humanity, orphilosophy, will be unhesitatingly characterized as worse than *barber*ism, or, at least, that is not surgery. The experience of the unprejudiced portion of the profession may be confidently appealed to, in support of the plain statement of a melancholy truth, that the possession of surgical talent has been tested as racily as the speed of an anxious quadruped between the distance and winning-posts, by—the progress of the moment-hand of a time-piece."

STRANGERS' HOSPITAL AT HAVRE.

It appears from a Report now lying before us*, that a small hospital for sick seamen was founded at Havre in 1837, by Dr. Douglass. He observes that the seamen from the north of Europe, such as Swedes, Danes, Russians, and Dutch, generally speak English, while not one in fifty understands French; so that it is extremely desirable that when ill, they should be attended by persons speaking the former, rather than the latter language. In point of economy, too, the saving is considerable; for at the sailors' boarding-houses the charge, when they are ill, is about six francs and a half a day, while at the Strangers' Hospital it is only three francs; in addition to which "the surveillance, so essential for the interests of shipowners and captains, is insured by the constant attendance of a surgeon in the house; and by a very simple regulation, so soon as a patient is admitted, if the nature of his disease does not prohibit it, he is placed in a warm bath, his clothes are taken away, a list of which is given to him, and he is provided with an hospital dress, which, however good, is not of that kind a man would wish to be seen abroad in, could he even leave the hospital without permission." Dr. Douglass took the lease of a small house and garden, near the gate of Ingouville, at the rent of 600 francs a year. It was originally supposed that eight beds would be sufficient, whereas twenty were afterwards required, so that it became necessary to sling hammocks in the loft. The number of pa-

* Report of the establishing and progress of the "Strangers' Hospital," Havre, in the year 1837. Addressed to the donors and subscribers, by H. G. Douglass, M.D., Physician to the Hospital. Paris. Printed for private distribution. 1837.

tients admitted from March 14th, 1837, to March 14th, 1838, was 134; of whom two only died; one of dysentery, and one of phlebitis. The chief heads in the list of diseases are:

Catarrh	16
Intermittent fever	14
Fever	12
Syphilis	10
Lues Venerea	8
Gonorrhœa	8
Contusion	8
Dyspepsia	6
Ulcer of inferior extremities	6

The donations amounted to 5,590 francs, and the annual subscriptions to 2,100 francs, while the disbursements were 10,664½ francs, leaving a deficiency of 2,974½ francs; but then no account is given of the sum received from the sailors, or those who sent them in, which would, of course, more than pay for their board, all other expenses being included in the 10,664½ francs above. In a port like Havre, where upwards of six thousand foreign seamen arrive in one year, such an institution must be eminently useful, and we wish it every possible success.

MEDICAL GAZETTE.

Saturday, August 4, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

LAY MEDICINE.

One of the most singular and painful contradictions between theory and practice, is to be found in the difference existing between the medical attendance really bestowed on the sick poor, and that which is nominally their right. Suppose a panegyrist of things as they are—a jolly well-fed optimist, who looked at every thing through the distorting medium of Acts of Parliament and Poor-law Reports—to describe the medical relief attainable by the country poor: would he not assert (especially after dinner) that, let the sick man be as indigent as he may—let him live in the most remote hamlet of Durham or Cornwall, ten miles from a market-town, if this be possible—he still has a right to claim medical attendance, with all the requisite drugs and surgical apparatus?—so that the relief he gets, though less elegantly administered, is not less potent to cure than the dear-bought medicine of the rich; he obtains, in fact, "the power of art without the show."

Such is the theory: the common practice is, that the country surgeon receives a tantalizing nothing*, of some twenty or thirty pounds a year, for his services, hardly sufficient to pay for an adequate quantity of genuine drugs, and gives a nominal attendance in return for his nominal salary. Dr. Gooch, whose head was as clear as his heart was benevolent, was so well convinced of this, that he proposed the establishment of an order of Protestant Sisters of Charity, or female physicians, to give that real assistance which the law merely affects to bestow: another very sensible physician proposes, in a recent work†, to make parish clergymen perform the office.

Dr. Gooch's scheme, which appeared in the first volume of our journal, and was afterwards reprinted in Southey's Colloquies, has never, we believe, been put into execution; whether from its clashing too much with the instinctive delicacy and reserve of Englishwomen, or from its not having been proposed to the classes most likely to adopt it, by those who have immediate influence over them. That such a proposition should have been broached by such a man as Gooch, shows clearly how bad was the state of things which it

* "Rien veut dire rien, ou peu de chose."—*Molière.*

† The Village Pastor's Surgical and Medical Guide; in Letters from an old Physician to a young Clergyman, his son, on his entering upon the duties of a Parish Priest. By Fenwick Skrimshire, M.D. Physician to the Peterborough Public Dispensary and Infirmary, &c. London, 1838.

was intended to remedy; for had matters been only tolerably ill carried on, he could not have dreamed of it for a moment. There must be some sad defect either in the efficiency or the numbers of the regular force, when experienced generals wish the volunteers to be called out. One of the most obvious objections to Dr. Gooch's plan is, its repugnancy to modesty. How could women treat the numerous maladies of the urinary and genital organs, or even go through the examinations requisite in other diseases?

The preliminary training would be another, and perhaps an insuperable difficulty. We freely confess that many branches of medical education might be lopped off without much damage. The knowledge of Chinese and heraldry, recommended by Rothe, and the courses on comparative anatomy and the theory of medicine insisted upon by certain Universities, might be dispensed with; nay, in the case of female physicians, we might give up botany and chemistry, and reduce several other courses of lectures within very narrow limits. But the practice of physic itself, to be taught well, must be taught elaborately. Gooch says of these Sisters of Charity,

"I would place them in some hospital under an experienced, clear-headed, practical physician, who should explain to them, in untechnical language, as they went from bed to bed, the signs by which he is guided in the choice of his remedies. Why, in one case, the prominent symptom of which is a cough, he gives opium; and in another, in which the prominent symptom is still a cough, he draws blood. Why, in one case, in which the prominent symptom is pain, he employs fomentations and opiates; and in another, in which the prominent symptom is still pain, he draws blood, gives purgatives and low diet. * * * * * This system of instruction should continue until my Sisters of Charity have acquired a readiness in detecting all ordinary diseases, in selecting the guiding symptom or symptoms, and in the use of that short list of remedies which even medical men find sufficient in pauper practice *."

This is unquestionably the true method of teaching the practice of physic; but to do it effectually requires time, a couple of years at the very least. For it is vain to hope that medicine, or any other art, can be reduced to a kind of essence to be instilled into the minds of pupils without dilution, and without repetition. Thus, a physician who possessed the power of condensation might comprise the chief points in the pathology and treatment of fever in half an hour's lecture; or he might write them down upon a sheet of paper, and give his pupils this Iliad in a nut-shell: no student, however, could learn the art of treating this multiform disease from these laconic hints; this he must attain by observation of the teacher's practice, by attempting to copy it, by repeated lectures, and, we fear, by his own mistakes. The difficulty, too, would be increased in Gooch's plan, by the pupils being chiefly middle-aged women, probably from 35 to 45 years of age, and from the nature of their education and habits ill suited to see their way amidst the maze of diagnosis. Still these difficulties to the zealous would not be impossibilities; and, though the experiment has not yet been tried, it might be.

Dr. Skrimshire's letters are addressed, in the first place, to his son, a curate in a populous village in the Isle of Ely, where there is no medical practitioner within several miles; secondly, to country clergymen in general; and thirdly, as we imagine, to ladies, for he says that it would have been improper to have introduced some diseases, because the letters are designed for general readers. Hence, with one or two exceptions, he has omitted all mention of diseases of the urinary organs; those

* Medical Gazette, vol. i. p. 153.

of the genital organs are entirely left out, excepting that he just notices injury of the testis received in riding. In medical books generally, authors follow the example of Aristophanes, who boasts that he calls a spade a spade, and Cleon a knave; but it is one of the disadvantages of lay medicine that this is thought unadvisable; and in the passage just referred to, instead of testis, we find "a more delicate and peculiarly sensible part."

Dr. Gooch required, with great justice, that his female physicians should walk the hospitals. Dr. Skrimshire does not suppose his readers to have had this advantage, and we fear that, in consequence, some parts of the book will be almost unintelligible to them. In any elementary medical work, whether intended to teach physic to laymen or to young practitioners, the distinction so lucidly pointed out by Gooch, in our quotation above, between diseases of the same name, should be taught by precept and examples. To bleed or not to bleed, that is the question. Shall a given cough or palpitation be treated by the antiphlogistic or tonic method? What are the symptoms which shew clearly that the tumultuous action of the heart requires the lancet—what are those which indicate wine and the mist. ferri comp.—and what are those mingled and perplexing ones which make even the good physician doubt, and force him, in spite of himself, to a sort of inaction? This method of instruction is not pursued in Dr. Skrimshire's letters, which, however, have great merit; not so much, indeed, as a guide to the layman, as to the young practitioner, to whom they will be a useful compendium of the diseases which are discussed in them.

Another indispensable requisite for a layman's medical manual is, that the practice should always lean to the side of safety; the bold might call it timid, but the timid should never be able to call it bold; *saltem non nocere* should be visible in every page—should, in fact, be the running title of the book. Not so our author; he tells his son, in a case of apoplexy, while waiting for the arrival of the medical practitioner, "if, after the abstraction of a pound or a pound and a half of blood, you perceive no signs of returning motion and consciousness, you may, in a stout robust habit, take away double that quantity." Strong purgatives are to be administered repeatedly; and in two or three hours, under certain circumstances, the curate is not to hesitate to draw away another basinful of from a pound and a half to two pounds of blood. This is pretty bold, it strikes us, for an amateur practitioner.

The doses of medicines, too, are often beyond those usual in medical practice, and therefore far, far too large to be recommended to laymen. Thus, in the table at the end of the work, the reader is taught that the dose of opium for an adult is two grains; if only one dose was to be mentioned, we should have said one grain, which is commoner than two, in the proportion of twenty times to one. In bilious fever, if the pains or the cramp are very intense, the author gives two, three, or four grains of opium—doses which ought not to be mentioned within a layman's hearing. If amateurs are to try experiments in this style, give us back Lady Bountiful with her bottles of pennyroyal water, and gargles of sage and vinegar. In another place (p. 170) the lay practitioner is told to give laudanum to infants, without any warning as to the extreme danger of this remedy in young patients. He is to give two, three, or four drops of laudanum by the mouth, or from five to ten in a clyster. If the patient is an infant a couple of months old, the smallest of these quantities will be too large, except in some extreme

cases, of which the amateur cannot be a judge.

Digitalis, that frightful drug, should not enter into the layman's medicine chest. He is instructed, however, (p. 409) to administer the tincture or infusion in dropsy, until it operates as a diuretic, or until it produces nausea, giddiness, and faintness. The dose is here directed to be two drachms of the infusion every eight hours, gradually increased; but in the table at the end of the book it is stated to be half an ounce. The infusion is ordered to be made with a drachm of foxglove to half a pint of water, being more than double the strength of the formula of the London Pharmacopœia. In anasarca full doses of cream of tartar and jalap are recommended, as half an ounce of the former, and half a drachm of the latter, two or three times a day*; and an eighth of a grain of tartar emetic may be given to an infant every four hours in catarrhal fever†, but must be omitted as soon as a pallid countenance, cold clammy extremities, and yawning, indicate faintness and exhaustion. These are startling recommendations; but what shall we say to the doses of strychnia which our author advises his son not to administer except under professional direction, but asserts to be a quarter of a grain three times a day, gradually increasing the dose to a grain‡!! In the table the dose is stated to be one-eighth of a grain.

These little specks and blemishes, however they may detract from the value of the work, if considered as one to be consulted by persons who possess no other medical compendium, are comparatively of little importance if it is studied by the young practitioner, who will profit by the lucid summaries of disease which it contains, without always adopting the boldness of its practice.

If lay medicine must be allowed, and even encouraged, we would have it in the least nocent form; the lady-doctors should keep to rhubarb, magnesia, salts, and peppermint-water; and we would banish strychnia, veratria, and digitalis, from the medicine-chests of amateur practitioners of the bolder sex. But if it be thought necessary that the clergy should return to the medical profession, and be again the physicians of England, as they were four centuries ago, let them have a suitable education. We do not insist on their knowing the class and order to which nux vomica belongs, nor on their demonstrating the nervous system of the lobster, but let them have that sound and clinical knowledge which Dr. Gooch claimed for the Protestant Sisters of Charity. For our own parts, we feel quite convinced that if it were well known to the philanthropists of England that there is really no effectual medical aid for the country poor, but that nominal salaries procure a nominal attendance, the evil would be remedied almost as soon as it was set forth. It would be strange if no method could be found of transplanting part of the superfluous medical population of London and Liverpool, York and Manchester, to populous villages where there is no resident practitioner. When an architect is required in Australia, he is sent out from England, which is assuredly much better than to attempt to teach the farmers of those settlements to build houses at their leisure hours. Or, to take examples which touch us more nearly, if surgeons can be persuaded to die at Sierra Leone or Vittoria, why not to live in the Isle of Ely? Diffuse a knowledge of the present state of things, and paid practitioners will be sent

* Skrimshire, p. 414.
† Ibid. p. 227.
‡ Ibid, p. 196.

to live in remote villages. The proverb warns us not to let children play with edged tools; but what is this to suffering amateurs to sport with strychnia, veratria, and digitalis?

STUDY OF AUSCULTATION.

To the Editor of the Medical Gazette.

Sir,

Great numbers are scared from the study of auscultation by apprehension of extraordinary difficulties; and there is not, perhaps, any generally considered greater, than that of detecting and discriminating between the several valvular diseases of the heart. The following experiment, made by four students of St. George's Hospital, affords demonstrative proof that the diagnosis in question, usually supposed to require years of experience, may be efficiently taught in the brief space of ten minutes: and I communicate it to you in the hope that, through the medium of your valuable journal, it may, by encouraging the diffident, prove subservient to the progress of medical science.

Dr. Hope took four students, all novices in auscultation; and as several of them did not know the sound of a valvular murmur, he introduced a single patient to afford them the opportunity of hearing it. He then ascertained, by examination, that they were acquainted with the anatomy of the heart, and with its situation and relation to the exterior. This being done, he occupied ten minutes in giving an explanation, elucidated by a chalk diagram, of the mode of discriminating between the various valvular diseases, and in catechising to ascertain that it was understood.

Six patients, presenting five distinct varieties of valvular disease, some complicated and obscure, were now introduced, and each pupil examined as many of them as the leisure of the patients would permit, writing his notes and diagnosis on the slips of paper which I forward to you. Out of sixteen diagnoses which were made, one alone was partially defective.

Should the present communication prove acceptable, I shall have pleasure in sending you the explanation of Dr. Hope for a future number.

I remain, sir,
Your obedient and humble servant,
T. W. Pocock.

10, Trevor Terrace, Knightsbridge,
July 23d, 1838.

I. *Regurgitation through the Aortic Valves.*

Jacob Clark. There is murmur heard from the sigmoid valves, which can be traced upwards in the course of the aorta, but is scarcely to be heard at the apex of the heart; from which I suppose that the mitral valve is sound, but the sigmoid valves of the aorta diseased. It is not heard in the course of the pulmonary artery, but alternates with the pulse—that is, regurgitation takes place during the diastole. Pulse jerking.

Correct.—E. H. Hills.

Semilunar of aorta chiefly. Murmur heard distinctly over the sternum, and in the course of the vessel. Sound dull and hollow at apex, shewing that the disease is not connected with mitral. Sound alternating with the pulse. The murmur may be heard in the pulmonary artery, but indistinctly. Pulse sharp and jerking. Regurgitation during diastole of ventricle through the similunar valves of aorta.

Correct.—C. J. Sandford.

II. *Regurgitation through the Aortic Valves, and Roughness of the Arch of the Aorta.*

William Sexton. A slight murmur over the aortic valves, continued down into the ventricle, and following the *second* sound, which is sharp and short. A more blowing sound is heard along the course of the aorta, which corresponds with the *first* sound. The murmur following the second sound is produced by regurgitation through the aortic valves. The sound in the arch of the aorta arises from a roughened state of its lining membrane.

Correct.—T. W. Pocock.

Slight murmur at the apex of the heart, becoming louder as you ascend in the course of the aorta; alternating with the pulse. The disease, I presume, is situated in the aortic valves, with regurgitation through them. Mitral valve is apparently unaffected.

The sound high up in the aorta is systolic, and produced by the flow of blood over the contracted valve.

Correct, except that "contracted valve" should have been "rough aorta."

Fred. Malton.

The same as Jacob Clark (*i. e.* aortic regurgitation), with the exception of the murmur being heard *nearer* in the ventricle and upper part of the aorta than at the sigmoid valves themselves.

Defective, in not stating that the murmur high up the aorta was systolic, and produced by aortic roughness—E. H. Hills.

III. *Contraction of Aortic Valves, with Regurgitation.*

Michael Tighay. A double murmur

over the aortic valves, not heard at the apex of the heart, very obscurely along the pulmonary artery, but *very near* along the course of the arch of aorta. Pulse weak and jerking, arising from contraction, and regurgitation through the aortic valves; the systolic murmur being from contraction, the diastolic from regurgitation.

Correct.—T. W. Pocock.

There is a double murmur at the sigmoid valves, one corresponding to the diastole and the other to the systole of the heart. The first is produced by regurgitation, the other by the blood flowing through the valves.

Correct.—E. H. Hills.

Systolic murmur synchronous with the pulse; semilunar valves of aorta contracted; sound produced by blood rushing into the artery; mitral valves healthy.

Murmur not synchronous with the pulse, produced by blood regurgitating into the ventricle through the semilunar valves during the diastole of the ventricle. The sound heard is feeble at the apex.

Correct.—C. J. Sandford.

IV. *Mitral Regurgitation.*

Hannah Isaac. A filing murmur heard at the apex of the heart, very distinct, and synchronous with the pulse; extremely faint at the base of the heart, from contraction and regurgitation through the mitral valves.

Correct.—T. W. Pocock.

Frances Haines. Murmur heard at the apex of the heart, synchronous with the pulse, which is weak and jerking; murmur very feeble and indistinct at the base of the heart; contraction and regurgitation through the mitral valve.

Correct.—T. W. Pocock.

Frances Haines. A murmur is heard most distinctly at the apex of the heart, and but slightly over the sigmoid valves: it is synchronous with the pulse. Pulse weak. Regurgitation through the mitral valve.

Correct.—E. H. Hills.

The murmur is heard more distinctly at the apex of the heart synchronously with the pulse. No murmur in the course of the aorta, and obscure over the pulmonary artery. The disease, I presume, is situated in the mitral valves, the sigmoid valves being unaffected. Regurgitation on the systole of the ventricle through the mitral valves.

Correct.—Fred. Malton.

V. *Obstruction in the Pulmonic Orifice.*

Charlotte Manning. A blowing murmur heard at the centre of the sternum, slightly along the arch of the aorta, and at the apex of the heart, but very near and distinct along the pulmonary artery, synchronous with the pulse From disease of the pulmonary valves.

Correct.—T. W. Pocock.

Murmur heard distinctly over the situation of the sigmoid valves, but especially in the course of the pulmonary artery. The murmur is synchronously with the pulse. The pulmonary valves are diseased; the tricuspid and mitral are healthy. No regurgitation.

Correct.—Fred. Malton.

A murmur is heard at the sigmoid valves, which can be traced over the course of the pulmonary artery; it is synchronous with the pulse, and not heard in any other part. The sigmoid valves of the pulmonary artery are affected, and the murmur is heard whilst the blood is flowing through them.

Correct.—E. H. Hills.

There is a murmur audible over the region of the semilunar valves, which I can trace in the course of the pulmonary artery. It is audible very feebly in the aorta, and at the apex of the heart. The sound is synchronous with the pulse, or with the systole of the ventricle, shewing that the sound is produced, not by regurgitation, but by the blood issuing from the ventricle. I cannot detect any disease of the mitral. Sound produced by contraction of the semilunar valves of the pulmonary artery.

Correct.—C. J. Sandford.

NOTE IN REFERENCE TO THE PRECEDING.

To the Editor of the Medical Gazette.

Sir,

I shall be obliged by your inserting my disavowal of the following statement, erroneously ascribed to me by Dr. James Johnson, in his verbal remarks on a paper of mine on the pulse, lately read to the Medico-Chirurgical Society:—

"Dr. Hope states that in three or four hours he could teach a pupil to understand and appreciate the various states of the pulse as indications of disease."

My statement was, that "an intelligent student, familiar with the anatomy of the heart and with its situation in reference to the surface, will learn the diagnosis of the several valvular diseases in two hours, if they be demonstrated to him on six or eight well-marked cases, presenting all the varieties of valvular disease." Had

I been apprized when the paper was to have been read, I should have pointed out at the time that nearly the whole of the remarks of this gentleman and another were founded on misapprehension of the nature and object of the communication.

The valvular diagnosis in question was accomplished with complete success last week, by four pupils, at St. George's Hospital, the only difference being, that, instead of two hours, *but ten minutes* were occupied by myself, as a diagram and a verbal explanation proved sufficient for the purpose. The students will furnish you with the particulars, if you deem them worthy of your attention.

I am, sir,
Your most obedient servant,
J. Hope.

13, Lower Seymour Street.
July 21, 1838.

HÔPITAL DE L'ÉCOLE.

CASE OF HYPOCHONDRIASIS IN A PHYSICIAN.

Dr. Charles D. living at Paris, of middle height, nervous constitution, pale complexion, and slender limbs, entered the clinical hospital in order to be treated for a neurosis.

On the 5th of March, 1838, at the evening visit, he gave the history of his case, and the symptoms which he experiences, as follows:—

"My affection began at the time when the cholera was in Paris, in 1832; the extreme fatigue to which I was then exposed obliged me to drink spirituous liquors to keep up my strength, and perform my duties. I took coffee and alcoholic liquors in very large quantities; and whether from fatigue, or from being unaccustomed to spirits, one day I suddenly fell down. I was carried home, and soon recovered; and, when the epidemic was over, my extreme fatigue ceased also. I was able to confine myself to seeing a few patients, but I studied too hard. I undertook a work on medical biography, which required immense literary labour; and in the month of December, 1833, after fourteen hours' study, I felt of a sudden as if something was going up rapidly to my head. Was this a cerebral congestion? M. Cazenave, whom I called in, thought so, and wanted to bleed me; but as I knew that I was extremely nervous, I preferred the application of leeches to the anus, and the injection of several stimulating clysters. Under the use of these remedies the symptoms disappeared. I had zealously resumed my pursuits, when on the 18th of April, 1834, I again suffered from congestion. It is from this period in particular that the strange symptoms of my disease began to appear. This affection, consisting chiefly of internal sensations, cannot be fully estimated excepting by myself, and none but sound physicians can comprehend it, according to the greater or less clearness of the idea which I am able to give of it.

"You see in me," continued Dr. D., addressing himself to the pupils around his bed, "a type of what is commonly called a *malade imaginaire*; but the expression is a wrong one, and conveys a false idea. It was well said by Pascal, there are no *malades imaginaires*. Above all, a man who is acknowledged to have acquirements, and who possesses a certain station in society, is not like a child who says, 'I have got a head-ache, or belly-ache, or leg-ache; I can't go to school.' A man of spirit will never say, 'I am ill; I can't walk; I can't work any more,' unless some organ suffers, unless there is some material disorder in his system. This is my case; whether from extreme fatigue, or from chagrin, or from misfortune, in short, whatever may be the cause, it is clear to me, that my organs are no longer in connexion with my faculties; that my will is no longer completely obeyed. Does this come from having used my will too much? I know not; but I have arrived at the certainty through my own case, that a man may sometimes lose the faculty of willing."

It is evident that here the patient does not express his thought correctly. In him, as he has often said, the will continues, but the organs charged to perform the acts of the will fail in so doing. This is what makes him say that he wills without willing, and that he is no longer able to will. Hence we may understand how much courage he must have had, and how much energetic resistance must have been expended, for his will to remain thus powerless before the inertia of the organic mechanism; and hence has necessarily resulted a state of mental uneasiness, and intellectual irritation, which have rendered his position more and more painful. Can any thing be conceived more hopeless than to be conscious of one's worth, both as regards what one has already done and what one feels capable of doing, and then to find oneself suddenly reduced to nothingness? Add, too, that most painful reflection, that those who are unable to understand your state, your friends, perhaps, both think and tell you that you lack energy and fortitude, and that your sufferings exist only in your imagination. It is then assuredly that a man would prefer having some very obvious and palpable disease,

however hideous and disgusting; he would, at least, be pitied, and misfortune, when shared, is always more easily supported. The opinion of Dr. D. seems to us quite correct; when a reasonable man says he is suffering, it must be so; and though his complaints are not supported by any thing within the grasp of the senses, let us not conclude that he has no disorder; for by reflection we may arrive at a more just and correct conclusion.

We do not, however, pretend to deny that the constant propensity of hypochondriacs to analyze all that is going on within them, and the pleasure which they experience in painting all their sufferings with more or less exaggeration, contribute to sharpen their imagination. But this is merely accessory to our first position, which remains unassailed — there is an organic lesion. Dr. D. gives the following account of the sensations which he has experienced :—

"It is particularly since 1834 that I have been afflicted with the symptoms of a most singular cerebral neurosis. I felt as if a circle compressed my brain and the roots of the nerves. I felt suddenly stopped in walking, and this sudden check could be overcome only by the most painful efforts. If only a short distance remained to go over, I was able to finish it by running, when walking had become impossible or automatic. Sometimes I thought I felt my brain coagulating or solidifying in a spiral form; and it was the anterior part in particular which was the seat of these sensations. Every kind of work had become impossible; I could neither read nor write, and even conversation tired me, if at all prolonged. After a mere chat, where I had had no occasion to strain my mind, I have had a complete paralysis of the tongue, lasting ten minutes. If, in despair at such a state, I endeavoured to force my attention, I suddenly felt a great heat in my head and a very painful sensation of cold in my extremities. I suffered from humming and tinnitus in my ears, and an icy rigor; and then I had no other resource but putting myself in a horizontal position. I then by degrees felt my head become more free, formication took place in my limbs from the return of blood, and the equilibrium of caloric being once re-established, I felt relieved.

"When the congestion towards my head was less strong, I felt relieved by crossing my arms upon my head, as if the brain obtained ease by the thoracic muscles thus rendering the capacity of the chest greater, and more blood flowing to the heart.

"I often had palpitations; and the pulse was frequent and irregular.

"In the midst of this state, being struck with the idea that I had some serious, and, perhaps, incurable disease, I was continually besieged by terrors and chimerical fears, even while awake, and my sleep was disturbed by frightful dreams and nightmares.

"I must remark that I had a violent and almost continual desire of eating; and was obliged to take many meals, consisting, too, of very nourishing food. When the stomach was full I was much more comfortable."

Here M. D. explained the difference between the desire of eating and hunger, the former being merely the want of refreshment, the latter consisting especially in the pleasure of the taste, and in the desire of eating food of some particular kind.

After having spoken in this way for a considerable time, Dr. D. stopped, and assured us that the emission of thought became impossible to him. The efforts he had made to give coherence so far to his narration had fatigued him too much to allow him to go on, in addition to which it was painful to carry back his memory to such recollections.

To cure so grievous a malady, he made use of every remedy, both those which occurred to himself, and those which were suggested by his medical brethren. He employed without advantage leeches, blisters on different parts of the body, the actual cautery, purgatives of every kind, tepid and cold baths, *douches* on his head, carbonate of iron pills, and cinchona wine. He had resolved to put a seton in the nape of his neck, when Dr. Paparel persuaded him to try electricity.

On the 29th of December, 1834, the patient went into M. Le Molt's establishment. Here he was put into connexion with the common reservoir, and was somewhat relieved by the use of the brushes; he tried the electric bath once only, and was not able to bear it. After having been benefited by the application of electricity for six weeks, M. D. continued to suffer more or less till August 1836.

At this time he went to bathe in the sea at Dieppe, and returned without any great alteration.

On the 2nd of October, 1836, he obtained from the ministry, at the recommendation of M. Rostan, the place of physician to the post-office steam-boats. He set out in the hope that this occupation would contribute to the re-establishment of his health by withdrawing his mind from its accustomed train of ideas. He remained a whole year in this office, in spite of the sea-sickness to which he was constantly exposed. His fortitude at last gave way, and he was obliged to resign his situation. It would have been imprudent in him to

allow such an agent as sea-sickness to continue to act with all its intensity on the brain, that organ being already diseased.

Travelling into different parts of France has already improved his health considerably. His attacks are now far from being so violent or so frequent as those which he has narrated; and it is in order to complete his cure that he has resolved to enter the clinical ward, and put himself under the care of M. Rostan.

Present state. — March 6th, morning.— The patient has passed a bad night, which he attributes to the change of his habits— to the impression which he could not prevent himself from experiencing from being in the hospital—and to the thought that the resolution he has taken, combined with his absence, must have mortified his family, and particularly his wife. He is usually better in the morning than in the evening, but then he must have slept and had a quiet night. The more the diseased organ has reposed, the more fit it is to resume its functions; he requires a good deal of sleep. He can then read in the morning, and is able to write for an hour or two; but if he wishes to go on working when he feels that he can no longer keep up his attention, his will no longer suffices; his organs refuse their task; his pupils become dilated; he is no longer able to see; he has a humming in his ears; his head grows hot; and he is obliged to stop. So it is every time that he is forced to concentrate his attention upon a single point, as, for instance, to cut his nails, or to shave. In such cases he is obliged to change his posture frequently, to lean on one side, and to alter the direction of the visual ray, making it more or less oblique, in order to recover his sight. The same thing occurred when, in seeing patients, he had to prescribe, and was obliged to conceal his state. He used then to lean more or less to the right or left, and was thus able to write a coherent and regular prescription. It is remarkable that when Dr. D. was at the worst he always preserved a wonderful correctness and rapidity of diagnosis, which he modestly attributes to a pathological faculty. He is powerfully affected by atmospheric influence; he is pleased with clear weather and sunshine, and yet, formerly, exposure to the sun was hurtful to him, and he avoided it in the streets with a sort of horror. He cannot bear solitude, and he tries to find some one to speak to, preferring those who listen to him, and seem to be interested in his condition. He seems to say to them, "Pity me; stay near me while I repose; if not, I shall not eat any more. I shall go with you if you go." He is obliged to change his place frequently, and has need of much to divert his attention. He likes moving on better than standing or sitting.

All the senses, as well as the memory of the patient, are sensibly weakened; but these functions of the brain excepted, all the rest are executed with perfect regularity. Digestion and the secretions in general, respiration, circulation, and calorification, are uninjured. His locomotion still frequently feels the morbid influence of the brain, which is shewn by sudden weakness in the muscular system, and irregular movements in progression; so that now, as well as formerly, the patient can finish by running what he could no longer get over by walking. There is nothing particular in the generative functions, excepting occasional nocturnal emissions, which recur at no very distant intervals, and are caused by a lively sensation, whether agreeable or the reverse.

8th. He has passed a disturbed night, because not far from him there was a labourer suffering from pneumonia in an advanced stage, who was delirious.

Dr. D. gives the following account of the sensations which this caused in him:— "At first this man's moans affected me in a disagreeable and merely physical manner; I then insensibly experienced a mental uneasiness, and such an irritation that when day broke, it required some fortitude to dress myself and not to go away, just taking my clothes in my hands. Pity was first the predominating sentiment, and then impatience at the painful manner in which I had been affected. I should not like to see a hysterical attack, as I should be afraid of having one immediately."

Apropos to this extraordinary and inexplicable influence of the moral over the physical feelings, M. D. told us of a fact that had occurred to himself. He was labouring under ophthalmia, when a letter came which interested him extremely. Suspecting what it contained, he gave it to his wife to read. The moral impression which he experienced from it was very forcible, and his ophthalmia immediately disappeared. Was the blood that was carried to the brain under the strong moral influence partly abstracted from the inflamed organ, and was a natural derivation thus caused? This is theory, but the fact remains.

M. D. wishing M. Rostan to make his case public for the instruction of the pupils, and knowing that he intended to mention it in his next clinical lecture, sent him the following note:—

Tuesday, March 18th, five o'clock in the morning. *Nota:*—"The origin of this affection lies in too great an activity of the brain; whence has resulted a super-

abundance of life in this organ, an accumulation of fluids, which has been carried to a high pitch under the influence of the efforts which I have often made to accomplish certain functions which could not be executed; hence has arisen a forced dilatation in the vascular system of the brain, which explains why with the smallest impression (under the influence of accumulated caloric), or with the slightest fatigue, the head becomes engorged, in proportion to the varicose disposition of the veins and arteries of the encephalon. The whole therapeutic secret would consist in diminishing the area of these vessels, and in carrying back the excess of vitality from the head to the general economy, in order to re-establish the broken equilibrium. It must not be forgotten that during sleep this equilibrium is completely restored.

"CHARLES D."

M. Rostan remarked how fond patients affected with this kind of neurosis are of talking over the phenomena which they experience, and how eagerly they seize an opportunity of speaking of their sufferings. The ancients placed the seat of hypochondriasis in the intestines, the spleen, or the face; and these fancies may be explained by the slender knowledge they possessed of general human anatomy; but it is astonishing that in our days this affection should have been looked upon as a gastro-enteritis. The frequent examinations of hypochondriacs at the Salpétrière, who have died of every other disease, have given no support to this opinion. M. Rostan related that on one occasion the transverse colon having been found vertical in the body of a hypochondriac, it was soon published that this was the seat of hypochondriasis. MM. Rostan and Georget think it much more rational to place the seat of this malady in the nervous centres, and to consider it as the result of a modification of that part of the brain which is subservient to the understanding.

The prognosis of hypochondriasis, says M. Rostan, is not unfavourable; indeed, this affection almost leads to longevity.

The treatment is very difficult, from the variety of remedies which are necessary. The patients being always eager for something new, soon lose their taste for what they had welcomed at first, and what had originally benefited them; the more they reason upon the treatment employed for them, the more difficult they are to treat. But it is then that the physician must redouble his energy, and not be disheartened by the obstacles which he meets with every moment; when the means furnished by his art are insufficient, he must not forget that it is not with the physical powers alone that he has to do, and that a very powerful resource, namely moral influence, still remains.

Hence a physician must not be an ordinary man, nor be skilled solely in the source of diseases and the remedies which cure them; but he must also have that knowledge of men which will enable him to penetrate the soul of his patient, and ascertain the good and ill, the feebleness and energy, the faults and the virtues, that co-exist in the same man.—*Gazette des Hôpitaux*, April 10th, 1838.

MEDICAL ATTENDANCE ON THE POOR.

To the Editor of the Medical Gazette.

SIR,

THE poor and the profession are indebted to you for calling the attention of the latter to, and keeping it fixed upon, the present defective arrangements for guaranteeing the sick poor of England efficient medical relief; and for ensuring to professional men adequate rewards for their parochial labours. The following statement will, perhaps, interest your readers. As it is drawn from authentic documents, you may rely upon its accuracy. And, to use a sailor's phrase, it is "as plain as a pike-staff"; the least learned of your readers will be able to furnish his own commentary upon it.

In 1837, the parishes of Chelsea, Kensington, Hammersmith, Fulham, and Paddington, were formed into one union. Each parish was again subdivided into so many districts, and a number of medical officers elected to occupy them. At Lady-day last the medical officers were all re-elected, the best assurance which the Board of Guardians could give them that they were considered to have done their duty. On the 17th of August, a new election of medical officers is to take place —the third in one year! a new arrangement having been made as to the medical division of the parishes, and the remuneration of the medical men.

The gross estimate of the united population of these five parishes states it at 85,500 inhabitants; and for the medical relief of the sick poor, the Poor-law Commissioners judge 700*l.* per annum a sufficient allowance, and the Board of Guardians agree that that sum shall be allotted for the purpose specified.

But how to divide the spoil the Board has found it somewhat difficult; with the help of Dr. Kay and a *medical* guardian (Mr. Morrah), however, they have managed to cut it up and determine upon its division, somewhat after the following manner:—

The medical relief is to be provided for on a new principle, which we may be suffered to call, for distinction's sake, the Kensington Union Rule of Three. This consists of population, workhouses, and acreage. Very well. Five hundred pounds is to be divided according to the population. One hundred pounds is to be distributed among the workhouses, and the remaining hundred is to be scattered with liberal hand, in due proportions of course, over the entire acreage—"all thinly scattered to make up a show."

The following table will furnish you with a bird's-eye view of the distribution of our Poor-law Commissioners' munificent donation to the doctors:—

		For Population.		For Workhouses.	For Acreage.
Chelsea.....	are to receive between them £700 in the annexed proportions	£188	the respective populations being	32,300 .. £40	£ 9
Kensington ..		122		20,900 .. 30	29
Paddington ..		85		14,500 .. —	19
Hammersmith		60		10,300 .. 15	23
Fulham		45		7,500 .. 15	20
		£500	Inhabitants,	85,500 £100	£100

So that for the entire medical relief of the sick poor, having divided the workhouses equally among the inhabitants and the acreage, the population pays at the rate of 1½d. and less than the sixth of a farthing each person. The land is taxed for many square miles to produce an additional hundred and fifty pounds, and contributes not quite a halfpenny to each person. Two-pence each from every inhabitant more than covers the seven hundred pounds.

Whether the medical officers are too well paid for the professional custody of such a vast population, let those say who know the value of human life and health. Whether the inhabitants are too heavily taxed, at less than two-pence a head, for the medical relief of the sick poor for a whole twelvemonth, let the inhabitants themselves determine. Whether the pauper proportions in each parish be sufficiently provided with efficient medical skill, attendance, and all remedial appliances to boot, for less than a fourth part the amount of what it formerly cost the parishes of the Kensington Union to make such provision for their poor, let all England judge. The Poor-law Commissioners have already determined that five parishes—including four workhouses, and numbering, at a *minimum* estimate, *eighty-five thousand five hundred inhabitants*—can provide competent medical attendance, and adequate medical treatment, inclusive of medicines, instruments, leeches, and every thing, except trusses, for less than half what parliament considered enough for the maintenance of an Assistant Poor-law Commissioner.

Why, sir, I would undertake to collect from this parish alone, in contributions from house to house, and from man to man, double the amount for the above purpose, heavily taxed as we have always been, increasingly so as we have been since we came within the operation of the new poor law, and even with the prospect before us—no very agreeable one—of having our poor-rates doubled in less than another year as a sort of first-fruit of our unnatural marriage to four other parishes, every one of which has an immensely smaller population.

I hope you will give this letter an early place in the London Medical Gazette, as it may not yet be too late to bring a blush into the faces of the Poor-law Commissioners—not yet too late to arouse our very respectable Board of Guardians to a sense of their own necessary weight and influence, when opposing justice and mercy to any board of commissioners whatever—nor yet too late to cause another arrangement to be devised, with greater assurance for the poor of efficient medical relief in times of sickness, and with better prospects to the profession of reward for the active and zealous discharge of their benevolent, toilsome, and even dangerous duties, to the sick paupers committed to their charge.

I am, sir,

Your humble servant,

Justitia et Misericordia.

Chelsea, July 29, 1838.

[The above letter has been authenticated.—Ed. Gaz.]

BIRMINGHAM EYE INFIRMARY.

Report of Cases, by R. Middlemore, Esq.

Fistulous Opening communicating with the anterior chamber.

Mary Elwell received a blow from a cork, which was forcibly projected against the eye. In a few days after the accident she called upon me: there was a small,

nearly transparent, tumor just without the margin of the cornea, which contained a small quantity of aqueous fluid. On its removal, by a minute opening made with the point of a fine needle, it soon reappeared, and the iris appeared somewhat narrower on the side of this little vesicular enlargement. No astringents I could use had the effect of causing its contraction, and, on opening it with a small needle, it soon filled again: had I used a large one, I might have produced prolapse of the iris, or have established a fistulous opening, through which the aqueous fluid would have constantly flowed. Subsequently I applied the nitrate of silver to the part. The small swelling gradually diminished, and has not since reappeared.

This description of tumor sometimes occurs after a small but penetrating wound at the corneo-sclerotic junction, and may either be produced by the protrusion of the membrane of the aqueous humour, or, as in this instance, by the passage of a minute portion of the aqueous humour beneath the conjunctiva. The application of the nitrate of silver to the part, after the evacuation of its contents, is usually adequate to its cure.

Dislocation of the Lens through a Rent in the Sclerotica, beneath the Conjunctiva.

Wm. Manton, æt. 21, a labourer, received an injury of the right eye from a cow's horn. The left eye was lost in infancy, from small-pox. Soon after the occurrence of the accident he was attended by Mr. Cook, and subsequently by Dr. Arrowsmith of Coventry*. When he first came under my care four months had elapsed since the date of the injury. The eye was in the following condition: — The cornea was slightly nebulous, and somewhat conical; the anterior chamber enormously large (almost amounting to dropsy of that part); the iris was wanting at its superior part, so that the pupil resembled that of an eye in which the upper section has been made for the extraction of cataract, the surgeon having shaved off a portion of the iris in making the incision; and there was a blue mark just behind the corneo-sclerotic junction, shewing that the sclerotica, perhaps also the conjunctiva, but I am not sure of this, had been ruptured at that part.

This man can see moderately well without spectacles, but his sight is not much improved by the use of double convex glasses, which, I presume, is chiefly owing to the increased convexity of the cornea and amplitude of the anterior chamber, by which the tendency to far-sightedness, occasioned by the loss of the lens, is in a great measure corrected.

The form of dislocation of the lens from which Manton suffered, is by no means one of frequent occurrence. It has fallen to my lot to see two instances in which the lens has passed through a rent in the sclerotica, and lodged near the corneal margin and beneath the conjunctiva. These cases came under my observation when only a very small tumor was visible, which I supposed, in both instances, to be the remains of the displaced crystalline. Although the patients suffered extreme pain for some time after the accident took place, they eventually recovered a very useful degree of vision, notwithstanding the omission, on the part of their surgeon, to remove the lens by a division of the conjunctiva. Sometimes the lens will not only pass through the sclerotica, but through the conjunctiva also, by the laceration of both membranes at the same time, and yet the patient's vision will be moderately good when assisted by the ordinary cataract glasses; but this, of course, is neither a common accident, nor the customary termination of it when it does take place.

To conclude my remarks on this interesting case:—Whenever a convex tumor of considerable size, and covered by the unbroken conjunctiva, forms near the corneo-sclerotic junction soon after an injury to the eye-ball, it is almost sure to consist of either effused serum or blood, or to be the displaced lens. The means of determining to which of these causes the enlargement is owing, are, for the most part, abundant and manifest, and if the swelling be ascertained to arise from the presence of the lens, the rule for the guidance of our practice is absolute; the displaced crystalline must be at once removed by a division of the conjunctiva.

Wound of the Cornea, and Dislocation of the Lens.

James Simcox, æt. 12, on the 5th of November was engaged in firing off a cannon, when some portion of its contents struck his left eye. He came to me on the following day. The eye was then a good deal inflamed, the lens was partly pushed through and partly fixed within the pupil: fragments of it were also noticed at the bottom of the anterior chamber. The cornea was very slightly wounded. He suffered a good deal of pain, although the eye was not acutely inflamed.

Treatment.—Mercury to be administered so as to affect the mouth slightly. Bella-

* Dr. Arrowsmith has been so good as to furnish me with an account of the appearance of Manton's eye when he first examined it, and my impression is, that our opinions differ in some degree respecting the nature of the case; but I am sorry to state that I have lost the letter he sent me when I applied to him for an account of the symptoms existing three weeks after the injury.

donna to be applied daily over the eye-brow. Flannels soaked in strong narcotic lotions to be applied to the eye-lids, and six leeches to be placed behind the ear.

Result.—In about a month, the lens being absorbed, the appearance of the eye was so perfect that it would be difficult to determine that it had ever been the seat of an accident. The sight recovered as after an operation for extraction of cataract.

I relate this case (one of a series I am engaged in arranging) to illustrate the propriety of omitting the performance of a surgical operation, in many instances where the lens, not covered by its capsule, is dislocated by accident. I have tried both plans pretty extensively, and am assured that the removal of the lens may be generally dispensed with, with great advantage as regards the result, and with that important advantage as relates to the patient, which no humane surgeon should ever lose sight of—that of avoiding the infliction of unnecessary pain. The removal, through an incision of the cornea, of a lens displaced by a severe accident, when the eye has become, as it soon will do, inflamed and intolerant of light, *always* occasions severe pain, and, *in some instances*, a degree of agony which the fortitude of a martyr can barely sustain.

Chronic Inflammation of the Orbital Cellular Membrane.

Joseph Hope, æt. 22, complained of pain in the head, throbbing in the orbit, and a feeling as though the eye ball was being extruded from its socket. The eye, on careful examination, was found to be slightly protruded; but there was no sense of pulsation conveyed to the finger, nor any appearance of infiltration of the orbital cellular membrane. The young man says he has received no blow or other injury to the eye. He considers it to be occasioned by being out late on a cold damp evening.

The complaint continued to increase; the eye became more and more protruded; the whole orbital contents became vastly increased; he suffered from dreadful hemicrania, from feverishness, and from general derangement of the health. At this time he applied at the Town Infirmary, at which institution he was attended by Mr. Gem; and when he called upon me after he had left the Infirmary, his eye was nearly as usual, except that the lids were slightly puffy and œdematous.

The treatment employed by me during the time the patient was under my care was founded on the supposition that he was suffering from chronic inflammation of the orbital cellular membrane, producing serous infiltration of that tissue. The illness for which he was attended by Mr. Gem did not appear to be connected with the state of the orbit. At one period of his illness, the severity of the pain, the increasing protrusion of the globe, the sense of tension of the eye-ball, and of stretching of the parts behind it, made it a matter of serious reflection how far the patient was suffering from the development of a tumor within the orbit. Of course the symptoms were manifestly different from those proceeding from inflammation of the periosteal lining of the orbit.

Employment of various Remedies for the Relief of Amaurosis.

Veratrine and aconitine ointment.—These unguents have been so much talked of, that I have thought it proper to give them a trial. I will briefly explain the results of their employment. In only one example of amaurosis, out of eight subjected to the treatment, have I derived the slightest benefit, namely, in the case of a soldier suffering from dimness of vision, which, however, was not so great as to prevent him from walking with tolerable ease about the streets of this town, and managing the sale of vegetables at home. The pupil of his eye was rather large and sluggish, and there was just that sort of muddy (occasionally approaching to resplendent when viewed in a particular light) green appearance within the eye, which is noticed where chronic inflammation of the septa of the hyaloid membrane has induced a slightly turbid condition of the vitreous fluid. The pupil (and this is a very common effect of these applications) became smaller and more active, and he thinks his sight considerably improved. After discontinuing the remedy, his sight was very little better than it was before he used it. In some cases of neuralgia of the eye-ball I have prescribed these ointments with unequivocal advantage.

The ointment is prepared by mixing four grains of aconitine or veratrine with half an ounce of lard.

Mode of using the ointment.—A quantity, the size of a *small* nut, to be rubbed above the eye-brow (by means of a bit of sponge attached to a convenient handle), until the skin begins to smart and feel very hot. The *rubbing* to be practised daily.

Use of the nitrate of silver.—The application of a finely-pointed piece of nitrate of silver to the margin of the cornea in certain forms of amaurosis, has been advised by various surgeons. Some years ago I tried this plan of treatment, but not finding it serviceable to the extent I expected, discontinued to employ it; but, finding it lately recommended by Lisfranc, and that recommendation supported by the relatio-

of a number of cases * in which it had proved serviceable, I felt that it was due to so distinguished an authority to carry his suggestion into effect. The following is one of the cases in which this method of management has been tried.

Case.—William Earp, æt. 12, residing at Atherstone, was brought to me by my friend Mr. Ward. The little patient had suffered from an attack of fever, nearly two years ago, since which time he has been blind, although in every other respect he is perfectly recovered.

State of the eyes.—Pupil large, clear, and immovcable; there is no inflammation present, and his eyes are not painful. With the right eye he can just discern the degrees of light, with the other he has not the slightest perception of light. This eye was selected for our treatment, and Mr. Ward was good enough to use the remedy at the boy's residence. After the nitrate of silver had been applied four times, at suitable intervals, Mr. Ward brought the child to me again, when the vision of the eye (formerly entirely dark) was sufficiently restored to enable him to distinguish colours, and to make out large and conspicuous objects. The pupil is smaller than it was, and the iris is more active. The use of the remedy and the improvement of vision are now progressing, though slowly. In one or two instances in which I have employed the nitrate of silver, a troublesome form of ophthalmia has occurred afterwards, but, inasmuch as most of the cases in which the nitrate of silver are admissible and advisable, are those in which there is an anæmic and atonic condition of the ocular tunics, and of the vascular system of the eye generally, this occurrence is very rare, and has always been, under my own observation, quite manageable.

Mode of using the nitrate of silver.—Having a portion of nitrate of silver worked to a delicate point, I touch the cornea near its junction with the sclerotica so slightly as merely to produce a small eschar, on the detachment of which a minute, superficial, and perfectly healthy ulcer remains, which very readily heals and becomes imperceptible, and this I do at about four points, which are comprised within the half of the cornea.

Use of strychnia.—I must not omit to mention that in two instances of amaurosis occurring in my private practice, I have, after the failure of other methods, placed a blister in front of the car, and applied to the raw surface left after its removal a small quantity of strychnia, and that its use has been followed by the most satisfactory results; satisfactory, indeed, when it is considered that this method of management was tried after all the measures previously employed had failed to relieve.—*Trans. Prov. Med and Surg. Assoc.*

* The Continental and British Medical Review, No. 3 (for May, 1837.) Edited by A. M. Bureau Riofrey, M.D.

CURES BY COMPOSITION;

OR, THE FACULTY OUTDONE.

To the Editor of the Medical Gazette.

SIR,

As the enclosed *affiche*, which is now in circulation, may not have fallen in your way, nor the profession generally, I have taken the liberty of sending you a copy for insertion in your valuable journal, if you think it worthy of preserving, and can spare it a niche in your columns. This curious document might never have reached you but for the excellent communication, "Puffing Quack Medicines," which appeared last week in the Gazette, and to which it may serve as an addendum.

VIATOR.

London, August 1, 1838.

"Here's the cat!"—*verbatim, literatim et punctatim:*—

"TOOTH-ACHE
Cured by Smoke,
Charge:—Poor people, 3s.; Trades people, 5s.

The HOOPING-COUGH, from 2 to 4s.

JAUNDICE, 7s.

A most excellent Lotion for weak Watery Eyes, 1s. per bottle.

If the Tooth or Teeth should cease to ache before or on my arrival, the charge will be, to the poor, 1s.; to the trades person, 1s. 6d. for attendance.—apply personally or by letter, post paid, from 12 to 6 o'clock, to

G. F. A. BRIDGMAN,
At Mr. Shickell's, 2, Compton Street, Clerkenwell.

References can be given to persons I have cured.

R. Feeney, Printer, 26, St. John Street, Clerkenwell."

DIPLOMA CONFERRED UPON A TURK.

THE University of Wurtzburg conferred a short time since the degree of doctor of medicine on Anastasius Demetriades, a native of Philippopolis, in Turkey. On this occasion the thesis of the candidate was delivered in modern Greek—a circumstance the second of its kind in the German universities, the first having occurred at the University of Vienna.—*Times.*

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns, July 24, 1838.)

	Price.						Duty.			Duty Paid.	
										In 1838 to last week	Same time last year.
	£	s.	d.		£	s.	d.		s. d.		
Aloes, Barbadoes, D.P. c	12	0	0	to	30	0	0				
Hepatic (dry) BD..... c	5	0	0		14	0	0	B P. lb	0 2	58,922	42,862
Cape, BD. c	1	16	0		2	1	0	F. lb	0 8		
Anise, Oil of, German, D.P.... lb	0	9	6		0	9	6	F. lb	1 4	133	251
E. I. ..... lb	0	6	0		0	6	0	E. I.	1 4	525	780
Asafœtida, B.D. c	2	10	0		5	0	0	c	6 0	35	12
Balsam, Canada, D.P. ..... lb	0	1	3		0	1	4	lb	0 1	5,092	1,350
Copaiba, BD. ..... lb	0	5	6		0	5	6	c	4 0	68	138
Peru, BD. ..... lb	0	4	3			—		lb	1 0	1,798	346
Benzoin (best) BD..... c	25	0	0		50	0	0	c	4 0	62	70
Camphor, unrefined, BD..... c	10	0	0			—		c	1 0	321	281
Cantharides, D.P. ..... lb	0	4	6			—		lb	1 0	9,179	15,780
Carraway, Oil of, D.P. ..... lb	0	8	0			8	6	lb	4 0	521	1,135
Cascarilla or Eleutheria Bark, D.P.C.	3	10	0			—		lb	0 1	4,031	2,096
Cassia, Oil of, BD..... lb	0	6	6		0	6	6	lb	1 4	2,391	2,026
Castor Oil, East India, BD ..... lb	0	0	6		0	0	10	c	1 3	3,385	4,374
West I. (bottle) D.P. 1½ lb	0	2	3			—			—		
Castoreum, American ..... lb	0	17	0		0	18	0	lb	0 6	782	147
D.P. Hudson's Bay ..... lb	0	18	0		1	0	0				
Russian..... lb	none										
Catechu, BD. Pale c	1	2	0			—		c	1 0	186,66	17,653
Dark	3	0	0			—					
Cinchona Bark, Pale (Crown).... lb	0	2	0		0	3	6				
BD. Red ..... lb	0	2	0		0	4	0	lb	0 1	74,909	97,171
Yellow ..... lb	0	2	6			—					
Colocynth, Turkey ..... lb	0	2	6		0	4	0	lb	0 2	5,773	6,089
D.P. Mogadore ..... lb	0	3	0			—					
Calumba Root, BD. c	0	12	0		1	15	0	lb	0 2	17,939	8,812
Cubebs, BD. c	3	10	0		3	15	0	lb	0 6	14,841	19,800
Gamboge, BD. c	5	0	0		15	0	0	c	4 0	65	53
Gentian, D.P. c	1	4	0			—		c	4 0	286	353
Guaiacum, D.P..... lb	0	1	0		0	1	8	c	6 0	15	52
Gum Arabic, Turkey, fine, D.P... c	8	0	0		9	0	0				
Do. seconds, D.P. .. c	5	0	0		7	0	0	c	6 0	3,355	2,042
Barbary, brown, BD. c	2	6	0		2	8	0				
Do. white, D.P..... c	4	15	0			—					
E. I. fine yellow, BD. c	2	14	0		3	0	0	c	6 0	3,025	1,137
Do. dark brown, B.D. c	1	15	0		2	5	0				
—— Senegal garblings, D.P. c	3	15	0		4	0	0	c	6 0	1,0580	523
—— Tragacanth, D.P. c	8	0	0		12	0	0	c	6 0	262	228
Iceland Moss (Lichen), D.P. .... lb	0	0	2½		0	0	3	lb	0 1	5,179	55,194
Ipecacuanha Root, B.D. ..... lb	0	1	9		0	2	0	lb	1 0	9,923	6,820
Jalap, BD. ..... lb	0	1	7		0	1	10	lb	0 6	22,468	36,991
Manna, flaky, BD. ..... lb	0	4	0		0	5	6	lb	0 3	4,356	14,793
Sicilian, BD. ..... lb	0	1	7			—					
Musk, China, BD. ..... oz	1	0	0		1	8	0	oz	6 0	1,015	1,254
Myrrh, East India, BD. c	5	0	0		14	0	0	c	6 0	106	92
Turkey, BD..... c	2	0	0		11	10	0				
Nux Vomica, BD..... lb	0	8	0		0	9	0	lb	2 6	740	1,120
Opium, Turkey, BD. ..... lb	0	15	0			—		lb	1 0	15,991	17,931
Peppermint, Oil of, F. BD..... lb	1	1	0			—		lb	4 0	528	594
Quicksilver, BD. ..... lb	0	3	6			—		lb	0 1	188,265	122,909
Rhubarb, East India, BD..... lb	0	2	6		0	3	3	lb	1 0	19,925	23,334
Dutch, trimmed, D.P. lb	0	3	6		0	4	0	F. lb	1 0	3,704	3,805
Russian, BD. ..... lb	0	8	3			—					
Saffron, French, BD. ..... lb	0	18	0			—		lb	1 0	3,051	3,255
Spanish ..... lb	0	17	6		0	18	6				
Sarsaparilla, Honduras, BD..... lb	0	1	0		0	1	9	lb	0 6	68,300	65,909
Lisbon, BD. ..... lb	0	2	0			—					
Scammony, Smyrna, D.P..... lb		—				—		lb	2 6	4,948	5,025
Aleppo ..... lb	0	12	0		0	15	0				
Senna, East India, BD. ..... lb	0	0	3		0	0	4	E.I. lb	0 6	44,363	57,762
Alexandria, D.P..... lb	0	1	9		0	1	10				
Smyrna, D.P. ..... lb	0	1	0		0	1	3	Other sorts	0 6	39,075	36,679
Tripoli, D.P. ..... lb	0	1	0		0	1	3				

‡§‡ BD. In Bond.—c. Cwt.—B. P. British Possessions.—F. Foreign.—D. P. Duty paid.

ANIMAL MAGNETISM.

To the Editor of the Medical Gazette.

Sir,

I was much pleased by reading, in your journal of last Saturday, a letter from Mr. Terry, of Northampton, boldly stating his opinion upon animal magnetism, which subject is daily getting more absurd and insulting to common sense, though, with the mass of thinking people, I believe it is fast on the decline. My only wish is, that more hospital surgeons and physicians, and other men of influence in the profession, would come forward in the same manner, and give their opinion against it; but I am sorry to say, there appears to be a great lack of moral courage in this respect, and that men seem to forget that it is their duty to suppress humbug quite as much as to advance truth. A few names from the influential part of our profession, boldly disavowing their belief, would soon quiet the public mind, and save *some* of our brethren from the odium that must come upon them when it is found that they have been made the dupes (the most charitable name to give them) of two or three cunning hysterical girls.—I remain,

Your obedient servant,

Juvenis.

July 23, 1838.

To the Editor of the Medical Gazette.

Sir,

The observations elicited from time to time from the Medical Gazette on the all-fallacious science of mesmerism, appear to have excited disgust in the mind of Mr. Terry, at least so his letter in a late number would seem to imply; not as to the observations themselves, but the system to which they are applied. But Mr. T.'s assumption that a like feeling pervades the breasts of all your other readers is quite gratuitous; for in my own case, I have been highly gratified at the masterly style in which, step by step, the follies and deceptions of the system have been disclosed.

When the first rumour of "Animal Magnetism" appeared in conjunction with the name of Dr. Elliotson, and corroborated by public experiments performed at the North London Hospital, I felt an impression that more truth existed in its doctrines than was generally attributed to it, and looked anxiously to the Gazette for a confirmation of my views. My disappointment was only exceeded by my surprise at the palpable fallacies on which I grounded my fancied realities; and thus, instead of "disgust," I felt grateful to your magazine for the information, which had enabled me to rectify my ideas with regard to mesmerism.

The insertion of the above will, if you think it worthy of such, much oblige

Yours, &c.

Scriptor.

London, July 31, 1838.

[The principle we have always acted upon is that of letting both sides of every question be heard. By such means every thing soon finds its level, and so no doubt will Mesmerism among the rest.—Ed. Gaz.]

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, July 19.

Robert Bancroft Kinsey.—James Johnston Luce, Weymouth.—Henry Reader Gleadowe, Liverpool.—Robert Penlington Collison, Liverpool.—John Morgan Puddicombe, Dartmouth, Devon.—James Wilson Davis.—George Wayler, Devizes.—Edward Michael Prynn, Cornwall.

July 26.

Robert Williams, of Bristol.—Robert Jones, of Denbigh.—James V. Solomon, of Birmingham.—William B. Keeling, of Sunderland.

WEEKLY ACCOUNT of BURIALS.

From Bills of Mortality, *July* 31, 1838.

Abscess	1	Heart, diseased	1
Age and Debility	19	Hooping Cough	6
Asthma	1	Inflammation	11
Cancer	1	Bowels & Stomach	2
Cholera	1	Brain	4
Consumption	35	Insanity	2
Convulsions	22	Measles	1
Dentition or Teething	5	Paralysis	1
Dropsy	8	Rheumatism	1
Dropsy in the Brain	2	Small-pox	12
Dropsy in the Chest	1	Thrush	1
Erysipelas	1	Tumor	1
Fever	9	Unknown Causes	33
Fever, Scarlet	3		
Fever, Typhus	2	Casualties	3

Decrease of Burials, as compared with the preceding week . . . 54

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

July.	Thermometer.		Barometer.	
Thursday . 26	from 47	to 67	29·92	to 29·78
Friday . . 27	50	66	29·69	29·75
Saturday . 28	51	67	29·75	29·68
Sunday . . 29	50	65	29·63	29·56
Monday . . 30	46	65	29·60	29·71
Tuesday . . 31	47	70	29·76	29·86
August.				
Wednesday 1	44	70	29·94	29·91

Prevailing wind, S.W.

Generally clear, except the 26th, 29th, and afternoons of the 30th ult. and 1st inst., when rain fell; distant thunder, accompanied with rain and hail, on the afternoon of the 30th.

Rain fallen, ·3125 of an inch.

Charles Henry Adams.

Wilson & Son, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 11, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXV.

Physical Examination of the Heart (continued). Analysis of the Causes of the Sounds.—Varieties of the First Sound; from the contracting Walls; from the Quantity of the Blood; from the Action of the Valves; from the Impulse against the Ribs, &c.—Varieties of the Second Sound.—Distinction of the two Sounds.—Abnormal Sounds or Murmurs—Acoustic Explanations of their Production, with Illustrations.—Pathological Causes of Murmurs.—Causes of their Varieties.—On the Arterial Pulse as a Sign of the Action of the Heart.—Signs from a simultaneous Examination of the Heart and Pulse.—Varieties of Pulse from Diseases of the Heart.

Considering it, then, established that the first sound of the heart is produced essentially by the tightening of the valves and walls of the ventricles by muscular contraction, and that the second sound is caused by the sudden tension of the arterial valves by the recoil of the arterial column of blood upon them, we proceed to examine how far these sounds may represent the condition of the parts which respectively produce them; how they may be modified by changes in these parts, and by the addition of new elements of sound arising from such changes.

The elements of the first sound are the contraction of the walls of the ventricles, and the resistance given to their contraction by the blood within them. Each of these elements may vary in character, and give corresponding varieties in the nature of the sound. You can see at once that force and briskness in the contraction must together increase the sound, and that briskness gives it loudness, but not duration, which is rather caused by strength and continuity of action. Hence a heart that is contracting abruptly may generate a loud sound, even when its contractions are not strong; and a heart acting strongly, but slowly, will produce a duller sound, and of longer continuance. These different modes of contraction are frequently dependent on the thickness of the walls of the ventricles; but not constantly. You can readily understand why the contraction of a thin muscle should be abrupt and short, from its simplicity and want of mass; and why a thick muscle should be slower, although stronger in motion. Observe the different sounds produced on tightening this thin silk and this thick baize. The thinness of the silk gives a unity and briefness to the impulse which it receives, and the sound is short and loud. In the baize the impulse is divided and prolonged in the complexity of the fibres, and the sound is dull and less brief. So a thin ventricle will give a louder sharper sound than a thick one, under similar circumstances. But when under the influence of increased irritability, a thick ventricle contracts with more abruptness than strength, it may give as short and as loud a sound as a thin one which is acting slowly and with more tone. I have frequently heard a hypertrophied heart, when excited by nervous irritation, produce sounds louder than those of a dilated heart that was acting quietly. In this case the stronger impulse from the thicker heart would still

distinguish it; but there are some conditions in which even this test may fail: for instance, during or after syncope, the action of the heart is abrupt and feeble; and even when the walls are thick, the sound is generally short or flapping, and the impulse weak. It would seem that the systolic action is incomplete or partial, confined perhaps to the outer fibres and the tension of the valves; being abrupt enough to be sonorous, but insufficient to cause an impulse. These variations, which are exceptions to the rules given by Laennec and other writers, are explained by the view which we take of the production of the sound, and ought to be known, or they may lead to error. Although the ventricles are two, yet their fibres are in great part continuous, and their action and sound is one. But the different thickness of the two causes the sound to be shorter and clearer at the sternum which is over the right ventricle, and duller and longer where the apex of the left ventricle beats between the fifth and sixth ribs; and these differences are more remarkable where, in consequence of disease, the difference in thickness is augmented.

The other element contributing to the production of the first sound is the resistance opposed to the contracting ventricles by the mass of blood within them. Now the amount of this resistance will depend on the quantity of the blood, and the opposition that it meets with in its passage from the ventricles. When the quantity of blood is great, it will resist the ventricular contraction longer, and therefore maintain the sonorous tension longer than when it is small, but the resistance will not be so sudden, and the sound not so loud: for if the heart be distended with blood before the systole begins, its fibres cannot be so loose, and the great cause of sound, the transition from slack to tight, will be less complete. On the other hand, when there is little blood in the ventricles, they pass from great flaccidity to tension so suddenly that the sound may be unusually loud, but it can have no continuance. As far as the blood is concerned, therefore, a dull prolonged sound is a sign of a large quantity, and a short loud one of a small quantity, expelled from the ventricle. In accordance with this we find that when the pulse is slow, other things being equal, the sound is much longer than when it is quick; and such a length of sound in a pulse that is not slow, and the heart not diseased, becomes a sign of sthenic plethora. The longest first sound that I ever heard was in a gentleman whose pulse beat only twenty-eight in a minute; it occupied nearly a second of time. But this is not the case with all slow pulses, for where the pulse is slow from weakness, as in syncope, its contractions may be brief and incomplete, and the sounds short and weak. A short sound with a slow pulse is essentially a sign of weakness, and this may occur in asthenic plethora or cardiac congestion. When there is little blood in the system, we find the sound remarkably short and flapping, and it may be very loud; but then the pulsations are commonly frequent, and the radial pulse small.

But besides the quantity of blood, the resistance to its expulsion from the ventricle may modify the character of the first sound. If the vent for the blood be free, the resistance will be brief, and the sound short. Now the chief resistance opposed to the blood is that of the semilunar valves tightened by the pressure of the arterial column, and this will be great in proportion to the fulness of this column; here is then another reason why an abundance of blood tends to lengthen the systolic sound; there is more blood to be expelled, and more resistance to its expulsion. On the other hand, when there is little blood in the arteries, the systole takes place suddenly, with little resistance, with a short sound, and throws the blood into them in a loose sudden jerk, which is often felt in the pulse of those who have lost much blood. A permanent narrowing of the arterial orifice by disease must also tend to lengthen the first sound by prolonging the systole; but as such a change developes a new sound or murmur at the constricted point, it will be better to notice it afterwards. There is, however, one more element in the resistance given to the systolic contraction which must be noticed as a chief cause of the commencement of the sound—that is, the closure of the auriculo-ventricular valves. Until this takes place, the resistance scarcely begins, for the passage back into the auricle is too free; but no sooner have these valves closed, than the ventricles tighten on the ball of blood, and the sound is produced. You can see then how the free perfect play of these valves contributes to the first sound, not only by their own tightening, but also by their completing the strain of the other walls of the ventricles. The closure takes place suddenly, almost at the beginning of the systole, and thus causes the loud and flapping commencement of the sound. Where from disease their closure is not sudden or complete, this flapping commencement is less perceptible. There may besides be a new sound or murmur produced by the current of blood through the unclosed valve, but this we shall consider by and by.

You see that in the production of the first sound many elements are concerned;

and although the process is simple enough in itself, you must be fundamentally acquainted with its parts before you can fully understand the varieties of sound, and safely interpret them as signs. I suspect that very few will attempt to do this; the majority will content themselves with the easier but far less exact rules given by Laennec and others. I consider myself bound to describe the phenomena as I have found them, and to explain them as far as they will bear it; but not to sacrifice accuracy to simplicity. I have not done yet with the varieties of the first sound; for besides those of duration and loudness, there are other differences in its character.

Sometimes the systolic sound, without being longer than usual, seems broken into two by something like a flap in the middle; or to enunciate it, instead of being *lubb-dŭp* as usual, the first and second sounds are *bullub-dŭp*. I am doubtful as to the cause of this variety, but I think it probable that it may depend on some irregularity in the action of the auricular valves, especially the mitral, by which their tightening with its attending flap is either delayed or takes place in two successive jerks. This kind of sound occurs most commonly in cases of diseased heart; but I have heard it sometimes where there was no reason to suspect permanent lesion, but where the action of the heart was languid. In these cases the impulse likewise is sometimes double, and from the same cause. A double impulse is also now and then produced by the action of the valves following with a jerk the first stroke of the apex against the ribs; this, too, may make the first sound double.

The systolic sound has sometimes a remarkable drum-like character, without any murmur or other irregularity, the note of the heart being then *lumb-dŭp*, or *lung-dŭp*. Dr. Chambers, who has also heard this sound, described it as *gong-like*. As far as I can remember, this sound has been observed only in cases where there were signs of liquid in the pericardium or left pleura; and if this be the case, we may explain its more musical character by the contractions of the ventricles taking place in a medium which leaves their vibrations more free than usual, not muffled by a closely-wrapped membrane and the spongy lung. In these cases the impulse is diminished, and this separates them from those with the metallic clink which frequently accompanies the sounds of a strongly-acting heart, and which Laennec referred to air in the pericardium, whilst recent French writers have been imagining other causes equally fanciful. This metallic clink originates in the ear of the observer, and has nothing to do with the heart further than that it is excited by the blow which the heart communicates to the ear. You may hear the very same note if you lay your ear flat upon the table, and tap the under side of the table just opposite to it. I believe this to be the proper note of the tympanum. You may often hear it in auscultation, but when once aware of it, you can easily recognise it as belonging to yourself, and not to the patient. It is especially produced when the heart strikes against the ribs, and the external knocking sound is added to the intrinsic systolic sound. The addition of the knocking to the first sound is very perceptible when the heart beats strongly, or in the circumstances of posture, state of the respiration, and shape of the chest, before mentioned as most favourable to increase the impulsion of the heart against the ribs; but it is so much incorporated with the intrinsic sound that it seems to form a part of it. This is not surprising when we consider that the sounds produced by at the same time striking and straining a body, must partake of the same character, and become incorporated, seeing that its tension and mode of vibration are for the moment uniform. The sound is no doubt partly generated also in the walls of the chest; and when the impulse is very abrupt, it gives the impression of a knocking sound.

Besides the intrinsic sound and that of impulse which is thus occasionally added to it, the heart's motions sometimes produce sounds in the adjoining parts. The friction sound of the pericardium I shall notice hereafter; but beyond this, a strong impulse of the heart on a portion of lung may forcibly press the air from it; and if there happen to be any partial obstruction or mucus in its tubes, a short sibilant or mucous rhonchus may accompany each beat. The character of these additions, and the circumstance that they accompany the breathing also, and are more or less diminished by holding the breath, or by posture altering the manner in which the heart beats on the lung, may serve to distinguish them from the true cardiac sounds. Again, I have repeatedly heard the tinkling echo of pneumo-thorax—of a large cavern—of dilated bronchi — nay, even of an inflated stomach, accompany the pulsations of the heart when excited; these circumstances may be easily detected by their other signs, as displayed by auscultation and percussion.

The *second sound* of the heart is more simple in its character and causes, and less liable than the first sound to be modified by circumstances. Caused solely by the sudden reaction of the arterial columns of blood on the semilunar valves, its loudness

will depend on the mobile and perfect state of these valves, and the extent and abruptness with which they are stretched by the recoil of the blood at the moment of the ventricular diastole. It will, therefore, be most perfect when the heart acts regularly and slowly, giving time for a full gush of blood to carry the valves loose into their slight recesses in the walls of the artery, and for the as perfect reaction of the contents of the distended artery on their concave surfaces. On the other hand, if, from the quickness or weakness of the ventricular contractions, the jets of blood thrown into the arteries be small, the play of the valves will be less complete, and the sound less distinct. Also, if the permanent distension of the arteries be great in proportion to the propulsive power of the heart, as it sometimes happens during fits of palpitation, the valves may not in many pulsations be opened enough to flap distinctly. Or if the arterial tension be greatly diminished, as under the immediate influence of profuse hæmorrhage, it may not close them with sufficient force to give a sound. The second sound is generally heard most plainly at about the middle of the sternum, or between the third and fourth ribs close to it, which is opposite to the situation of the arterial valves; but it is generally audible over most of the upper parts and front of the chest; in the normal state perhaps more extensively than the first sound. Although there are two sets of valves, aortic and pulmonary, yet being brought into play at the moment of the diastole, which is simultaneous in both ventricles, they are generally tightened at the same instant, and produce but one sound. But if from the less mobile state of one set of valves, or from the greater pressure of the blood on them in one artery, the tightening of the two sets do not coincide, the second sound may be double, consisting of two flaps in quick succession, so that the note of the heart is then *lubb-dŭpŭp*, or *lubb-dŭrrŭp*. This sound sometimes closely resembles in rhythm the footsteps of a cantering horse. It rarely occurs (never, I believe, for a continuance) in cases where the heart is free from disease. When the action of the mitral valve is also retarded in the first sound, both sounds may have this double character.

I must not conceal from you, that it requires some practice in auscultation to enable you to distinguish some of these varieties of sound; and if the action of the heart is irregular or quick, it is not always easy to distinguish between the first and second sounds. When the impulse against the ribs is pretty distinct, this being felt either by the ear, or by the finger applied at the same time as the ear, will sufficiently declare its companion the first sound. When the impulse is obscure, the finger may be applied to the pulse in the carotids, which is not perceptibly after it. The radial pulse is so much later that it may not unfrequently even accompany the second sound. The different characters of the sounds will often serve to distinguish them; and when these are little marked in the region of the heart, they are often quite obvious at the top of the sternum, or in the carotid arteries, where the flap of the second sound can seldom be mistaken. The sounds in these arteries are, under ordinary circumstances, merely those propagated from the heart; but, under the influence of violent action, I have no doubt that a sound is produced by the shock of the blood against the sides of the great arteries, and is heard over them as a loud short sound, as intense as the simultaneous first sound of the heart, but more abrupt. So much for the natural sounds and the variations from them, all of which are seated in the solids of the heart and its appendages.

We have argued by reasoning, and have shewn by many experiments (see the investigations before quoted), that although sounds accompany the motions of the solids, yet the motion of the blood through the heart and vessels is habitually accomplished with the smallest possible resistance, and therefore wants one element essential to generate sound. But under various circumstances of disease this element, resistance, may be produced in the obstructions to the current of the blood, or in the novel channels into which it is thrown; and thus are developed new sounds, which, as distinct from the natural sounds, we may, with Dr. Forbes, class under the general term, *murmurs*. An example or two will set before us something of the character and causes of these murmurs.

At the moment when the blood is expelled from the ventricle, if, instead of a smooth, equally tense channel, it meets with a rigid constriction, or an abrupt orifice, its passage through it will be attended with a whizzing or blowing noise, which may be heard in the region of the heart. Or to take a simple case: if you apply the stethoscope in the course of an artery far from the heart, you will hear nothing as long as the current of blood flows smoothly and unmodified through it; but if by pressure you diminish the caliber of the tube at any point, you then complete the elements of sound; you give resistance to the moving blood, and at each pulse you hear a blowing or a whizzing

sound, which will vary in tone and loudness according to the force of the current and the degree of resistance which it meets with. This is purely a physical phenomenon; you may produce it in any tube through which a current of water runs. Thus, if you take an India-rubber tube, and adapt it to a reservoir of water, so that the water shall flow freely through the tube, you may, by pressure on the tube, produce murmurs, varying according to the force of the current and the resistance which the pressure opposes to it. They are sometimes like blowing; sometimes like rasping or sawing a piece of wood; and now and then they may be heard in quite a musical tone, which implies that the vibrations are then regular and sustained. The blowing and musical murmurs are generally caused by greater regularity but less force of current than that which produces the sawing or whizzing sounds*.

In fact, all these murmurs are produced by the passage of *liquids* through solid tubes or apertures in the same manner as analogous sounds are produced by the passage of *air* through pipes or holes of different kinds. They are the music of *water-instruments*, as the latter are the music of wind-instruments. There are only these differences between them, that liquids being more sluggish than air, are less susceptible of the sudden motions which constitute sonorous vibration, and not differing so much in density from the solids in which they move, liquids will have little of those reflected or echoed vibrations which increase and modify the sounds produced in air filled tubes. Holding in mind these qualifications, we may explain the murmurs heard in the heart and arteries by referring to parallel instances of the tones of wind-instruments; nay, we may find the parallel phenomena in the rhonchi, respiratory, and vocal sounds of that most complete and diversified wind-instrument, the wind-pipe and its branches. Like in these, there are varieties of sound, in generating which the solids and the current have different shares: thus in the grating, sawing, and stronger droning murmurs, the vibrating resistance of the solid is chiefly concerned; and its vibrations are transmitted to the adjoining parts as well as to the current, so as to produce in them a thrill which may sometimes be felt by the hand. Being in the rhythm of the heart's motion, this thrill resembles that felt on the back of a purring cat, whence Laennec called it the "frémissement cataire." These have their parallels in the sonorous rhonchus, in reed instruments, and in those imitations of these murmurs which we can produce by forcibly breathing through the nearly closed teeth, tongue, and lips, which in like manner communicate a sensible vibration to the solids, as in ringing the letter *R*, in a whisper. Again, in the blowing, hissing, whistling, and cooing murmurs, the vibrations are more those of the current reflected by the solid, in the manner of cavernous breathing, the sibilant rhonchus, blowing, or whistling with the mouth, or of the flute class of musical instruments. Here there are no perceptible vibrations in the solids; they are less actively concerned in the production of these sounds, which are rather transmitted in the direction of the current.

Although I adhere to this view (which I first proposed in 1828) as the only one which gives a complete physical explanation of all cardiac and vascular murmurs, I must not omit to tell you that many other explanations have been proposed. Most of these are too vaguely stated to be really explanatory, being mere references to the indefinite influence of friction or obstruction. There is one, however, which is more precise, and none is more plausible or ably supported; it is that of Dr. Corrigan, of Dublin, to whom we owe many interesting observations on the pathology and diagnosis of diseases of the heart and arteries.

Dr. Corrigan considers that murmurs are produced not in the constricted or resisted portion of the current, but in the flaccid walls beyond, which are thrown into vibration by the rippling motion which the current assumes there. This view, he thinks, explains the occurrence of murmurs in the uterine arteries and in varicose aneurism, where, instead of constriction, there is an unusually free passage for the current; but from the changed calibre of the vessels, it is of a rippling kind. Dr. C. has illustrated this view by a number of ingenious experiments, for an account of which I must refer you to his paper in the Dublin Medical Journal. I think, however, that this view of Dr. Corrigan is partial, and mistakes an effect for a cause. The flaccid state of the tube beyond a constricted point is an effect of the impediment which the constriction opposes to the current, and the rippling motion of the current equally originates in the constriction, although being once produced it continues beyond it in the looser part of

* These statements have been since amply confirmed by numerous experiments performed by Dr. Todd and myself, in committee, for the meeting of the British Association for the Advancement of Science, held at Liverpool last year. The report is published in the MEDICAL GAZETTE for December 2, 1837. In these experiments every variety of cardiac and vascular murmur was imitated, by variously modifying currents of water through caoutchouc tubes of different sizes and shapes, and the sources of variety were in great measure determined.

the tube. To prove this, cut off this flaccid part, and let the tube terminate with the constricted point, you will still have the murmur produced in it. You can try this experiment after lecture, with a current of water through this elastic tube, or with this India-rubber bottle, from which, by sudden compression, you can produce a current like that from the heart. You will find, that if you oppose a resistance to the current, either by pressing on the tube, or mouth of the bottle, or by holding something in it, the murmur is produced as well at the terminal orifice as at any other point. In these cases the constricted portion is like the mouth-piece of a wind-instrument, it alone produces the sound; but when so produced, the tube beyond may receive and modify it.

How then, you will ask, are we to explain the murmur in the uterine arteries and in varicose aneurism, in which there seems to be no constriction? I would refer you to the parallel case of a wind-instrument, the flute, in which also there is no constriction; for after the wind has left the lips, there is free vent enough for it, and yet the sound may be of the loudest kind. There may be a vibrating resistance to the current without constriction: for instance, when the current enters a new and enlarged channel, at a considerable angle, and strikes against its sides, it communicates to them an impulse which they resist, and the series of impulses and resistances which thus ensue, if forcible and rapid enough, constitutes sonorous vibrations. The dilated and tortuous state of the arteries of the gravid uterus, and the unusual direction of the arterial current into a lateral or a varicose aneurism, present conditions well calculated to produce sounds in this manner.

I will now briefly state the chief pathological conditions causing different kinds of murmurs, which may prove signs of those conditions:—

1. Constriction or projection in the arterial orifices; causing a murmur with the first sound, generally rasping or whizzing: very common on the left side of the heart.

2. Constriction or impediment in the auriculo-ventricular orifices, causing a murmur with the second sound: very rare.

3. Imperfect closure of the arterial valves, causing a murmur from regurgitation, with or instead of the second sound, generally whiffing or grating, sometimes musical: very common in the aortic valves.

4. Imperfect closure of the auriculo-ventricular valves, causing a murmur from regurgitation, with, and often impairing, the first sound: generally blowing, sometimes whistling or cooing: very common on the left side.

5. Considerable dilatation or relaxation of the aorta or pulmonary artery, just above its orifice, which, although of normal dimensions, is narrow, compared to the tube beyond it; causing a murmur with the first sound, generally grating: rather rare.

6. Partial obstruction or projection in a blood-vessel; causing a murmur with the pulse; blowing, musical, whizzing, or grating; prolonged in proportion to the degree of obstruction: common.

7. Abrupt aneurismal dilatation of an artery; causing a murmur with the pulse; generally grating: rather common.

8. Unnatural opening from a ventricle, or from an artery into another cavity, sac, or vessel, causing a murmur with the pulse, generally whizzing or rasping; if from the ventricle it must terminate with the second sound; if from the artery it may continue beyond it: rare.

It may be generally said of all these murmurs that they are increased by augmented force of the heart's action, and they may sometimes be changed by this influence; the blowing becoming whizzing or rasping, the musical murmur raised in pitch, and the like. So also the constriction of an orifice or artery, the defect of a valve, or any of the other causes may be present in so low a degree as to produce sound only under the temporary influence of an excited heart. On the other hand, I have met with instances, but they are very rare, in which increased action has diminished or even removed a murmur. The quantity of blood in either extreme may also modify these sounds: when excessive, it increases and prolongs them; when very defective, and accompanied by an exeited action of the heart, it may make them loud and short. Even the quality of the blood may influence the sounds; for a thin watery fluid is more readily thrown into sonorous vibrations than one of a richer, more viscid character; and this is one reason why murmurs are so easily produced in chlorotic and anemic subjects. In these the slightest pressure of the stethoscope on the carotid arteries in the neck is enough to cause a loud continuous, or remittent whirring, to which the French have given the name of "bruit de diable," from its resemblance to the noise of a toy called a "diable*."

* By experiments performed by Dr. Todd and myself since these lectures were delivered, I have been convinced that this and other continuous murmurs, like the buzzing of a fly, heard in the neck, are more commonly produced in the jugular veins. Dr. Ogier Ward had previously discovered these to be their seat. They may be arrested or diminished by pressing on the vein above, by hanging down the head, or by efforts to expire

It is in similar conditions that a murmur strongly generated at the orifice, or in the course of an artery, may be propagated through a great extent of the tube beyond, being carried with the current, which retains the vibrations, and may even communicate them to the finger by a sensible thrill. Sometimes the thrill is perceptible without the murmur; this takes place when the vibrations are too slow to produce sound. It gives the feeling of something rough passing rapidly under the finger, and is quite distinct from the hard throbbing pulse of arteries leading to an inflamed part*.

I do not believe that either increased or diminished action is capable of producing a murmur in healthily constituted hearts and arteries. There must be something to modify, as well as to hasten the current, before it can produce sound. Neither do I see it possible that an altered shape of the ventricle can cause a murmur in the arterial orifice, without either constriction or projection in that orifice; or, what is equivalent to it, dilatation or relaxation of the artery beyond it. In fevers, inflammations, and nervous irritations, we have every degree of increased action; but no murmur, without other disease; and I have met with hearts enlarged and dilated in various ways, but producing no murmur, without some change in the orifices or arteries. This statement is not in accordance with the opinions of other writers; but it is given as the result of my experience and study, and it increases the value of murmurs as diagnostic signs, if by their characters or positions they can be traced to their mechanical causes. That in many cases they can be so traced, I hope to shew in the following lectures.

The *arterial pulse* is another sign by which we judge of the action of the heart. I need scarcely commend it to your attention; for it has been too long and too universally consulted to be in danger of neglect. It is seldom so correct an index of the action of the heart as the sounds and impulse of the organ itself; yet some of those impractieable sages, who find physical signs in general too troublesome to be studied or valued, have exhausted their ingenuity with refined distinctions in this one physical sign, thereby only giving proof that there is more sense in their fingers than in their heads. I have formerly had occasion to explain to you the elements and the chief varieties of the pulse. (See Lecture XI.) Depending as it does on the size and condition of the arterial tube, and on the quantity and quality of the blood, as well as on the motions of the heart, it may be varied by many circumstances that do not affect that organ, and the motions of that organ may be variously intercepted or modified before they reach the artery, which is commonly felt by our fingers. But although these circumstances impair the accuracy of the positive indications of the pulse with regard to the heart, if we only take the trouble to analyze them, they may give us useful informa-

with the glottis closed; and they return with increased loudness on interrupting these acts.—(See Report of Committee, &c. Medical Gaz., Dec. 2, 1837.) These sounds are not essentially morbid; for they may be produced by the pressure of the stethoscope in the healthiest individuals; but in accordance with what I have stated in my lectures, they may probably be more readily produced when the blood is thin and deficient in quantity. The production of a sound by a partially obstructed venous current gives us a new physical sign, and its continuous character makes it in great measure distinctive of its seat. I have lately met with several cases in which its presence assisted me in the diagnosis of tumors within the chest. It is commonly a continuous humming or droning sound, heard near the clavicle or top of the sternum, or between the scapulæ, diminished by efforts at expiration, and increased immediately after them, or by contrary efforts.

* Professor Graves, in one of his interesting communications on inflammation, (Medical Gazette, July 7, p. 606) has objected to my explanation of the throbbing hard pulse of arteries leading to an inflamed part. I attribute this phenomenon chiefly to their dilatation, and to the obstruction of the vessels in the inflamed parts, to which they lead. That they are dilated has been proved by the experiments of Drs. Alison, Thomson, and others.—(See Fourth Report of the British Association for the Advancement of Science, p. 674). That the passage of blood through an inflamed part is more or less obstructed when the inflammation is established, seems to be proved by the observations of Hunter, Thomson, Philip, Hastings, Gendrin, and others, and is in accordance with the known effects of inflammation. In ascribing the throbbing of the vessels to these causes, I merely connect it with known conditions, which seem to me to explain it. But Dr. Graves thinks that they do not explain it. He observes, "the dilatation of the vessels, *however caused*, can on no principle account for their becoming the seat of throbbing and a hard pulse: their being more open than others to the pulse-wave from the heart could at the utmost only place them in the situation of other arteries naturally of the size they have now attained to; but we do not find that such arteries throb, or have a hard pulse." Dr. Graves surely cannot mean that a small artery dilated, and with its terminations more or less obstructed, is in the same circumstance as a large artery undilated, and with its terminations free. The coats of the latter have that share of elastic strength that equalizes in every artery the natural pulse. The coats of the former being already made tense and thin by dilatation, receive untempered each pulse from the heart, which here consequently becomes hard and throbbing. "Arteries do not throb, or become the seat of a hard pulse in proportion to their natural size:" but arteries do throb when their size is increased at the expense of their elasticity, and when opened to an impulse from the heart, which is unnaturally great. I had no time nor inclination to discuss in my lectures the theory of inflammation, which I leave to the more acute powers of Professor Graves; and in explaining the principles of the leading varieties of pulse, my aim was to exhibit the relation of well-ascertained phenomena, and to render them rationally instructive in diagnosis and practice.

tion, positive or negative, with regard to the condition of the circulation.

The radial pulse in general represents truly the number of the heart's contractions; it never can exceed them; but when the heart acts very feebly, its pulsations may not reach the wrist; and when they are irregular in force, some may be propagated to it and others not, in which case the pulse intermits. On listening to the heart, the ineffective pulsations are heard, and they are often loud enough, but their character, together with the fact that they do not reach the wrist, may inform us as to the morbid condition of the organ on which they depend; and we may learn from the same examination, that however violent the heart may be seeming to act, it is defective in its power to propel the blood through the system. This inconsistency between its apparent and its effective power is often manifest on comparing the pulsations of the heart itself during a fit of violent palpitation with the comparatively weak pulse that reaches the wrist. These discrepancies do not, however, occur in every case; for in some individuals the radial pulse pretty precisely represents the number, force, and even the time of the contractions of the ventricles, however varying they may be. This it does in those individuals in whom the coats of the arteries are rigid and unyielding, and transmit unmodified each pulse-wave from the heart. Where, on the other hand, the arteries are thin, elastic, and imperfectly distended, they may soften the hardness of the heart's pulse, in its course through them, reduce its strength, retard its period, and if it be small, annul it altogether. Thus, with your ear at the heart, and your finger at the same time on the radial pulse, you may take a far more accurate survey of the condition of the circulation than you can by examining these parts separately; and the utility of this method is by no means confined to affections of the heart. Thus, in inflammatory fevers, the pulses at the wrist owe their hardness and strength in some measure to arterial tone or tension, for they are more instantaneously propagated from the heart, and *accompany* rather than follow the ventricular systole. In debilitated atonic states of the system, on the other hand, the radial pulse *follows* the first sound of the heart by a distinct interval, which is occupied by the transmission of the wave along the course of the comparatively lax and yielding artery; and it is in passing through such a tube that the pulses lose also their strength and hardness, and those that are weak may be altogether suppressed. Thus, independently of disease of the heart, a pulse that reaches the wrist tardily and weakly, is a sign of an atonic or asthenic state of the vascular system; whilst one that is transferred with celerity and force, equally indicates that fulness and tension of the vessels which is the chief character of sthenic or inflammatory conditions. I recommend this comparative examination to you as well worthy of your attention, for I have often found it, as well as the character of the sounds of the heart, give positive practical indications in various diseases, when the pulse alone and other symptoms wore a doubtful aspect.

The character of the pulse may be very much modified by diseases permanently affecting the vital properties or mechanism of the heart and arteries. These varieties will be noticed as we treat of these diseases. I shall here only exemplify some of the different elements which are concerned in their production. A large strong heart acting with a prolonged first sound will cause a strong full pulse, which will be hard in proportion to the tension of the arterial coats. A large weak heart with a short first sound will produce a pulse which may be sharp or abrupt, but not strong, and it generally occupies an appreciable interval of time in passing to the extreme arteries. It may be small or large, according to the size of the artery and the general fulness of the vessels. Defects in the valvular apparatus of the left ventricle tend also to modify the pulse by altering the manner in which the blood is propelled from it. When the mitral valve does not close perfectly during the systole, part of the strength of this action must be lost backwards into the auricle, and the pulse will be weakened in proportion. This regurgitation commonly produces a murmur, and where it is considerable the pulse is also generally irregular. When the aortic valves are imperfect, the tension of the arteries is not maintained beyond the period of the ventricular systole; for when this ceases, the blood returns into the ventricle with a freedom proportioned to the insufficiency of the valves. This occasions a very peculiar character in the pulse. Owing to an increased thickness of the walls of the ventricle which commonly accompanies this lesion, the pulse is strong at the moment of the systole, but recedes instantly after, from the absence of the arterial tension, so that it has a jarring or jerking character which is very remarkable, and which often makes its motion visible, the artery starting into a slightly tortuous line at each pulse. This visible pulsation of the arteries has been described by Dr. Corrigan as a sign of incapacity of the aortic valves. Constriction of the

aortie orifice diminishes the strength of the pulse, but, unless extreme, not its hardness and sharpness. Constriction of the auricular orifice is generally accompanied with great irregularity of the pulse.

ON THE

TREATMENT OF FRACTURES

BY THE APPAREIL IMMOBILE,

As modified by Messrs. King and Christophers, with Cases; being the substance of a Lecture delivered at the Blenheim Street Dispensary, Aug. 1st,

By T. King, M.D.,
Surgeon to the Dispensary.

In submitting to the judgment of the profession a modification of the *Appareil Immobile* for the treatment of fractures, I deem it may be advantageous to inquire what are the principles on which the treatment of ruptured bones ought to be founded; in other words, what are the indications which require to be fulfilled in that treatment. For the purpose of ascertaining what they are, it will be necessary to revert to the process which nature adopts in the cicatrization of bone—to the phenomena observed in the formation of the callus. This kind of inquiry ought to be made, whenever new plans are proposed for the treatment of any disease, in order that a just estimate may be formed of their value. When we know what the natural process of cure is, we generally know also pretty well what measures are best suited to protect, facilitate, and ensure it. We must afterwards appeal to experience to have them ratified or annulled, according to the results obtained. With regard to fractures, then, we shall first endeavour to discover the principles of treatment, *à priori*, and examine how far the modification of the appareil immobile accords with them; and then state the results of the experience we have had in using the modified apparatus.

It appears that, when a bone is broken, the ruptured vessels of the osseous tissue, medullary membrane, and surrounding parts, give issue to more or less blood, which spreads itself around the fragments, and which, after some hours, coagulates. In a short time, all the parts involved swell and throw out lymph. The blood or its red part is very soon absorbed, and a tenacious, viscid matter is found between the fragments, opposite the walls of their canal. This matter is to form the ultimate cicatrix, and, although joined to, and continuous with the general mass of lymph existing on the outside of the fragments and within the medullary canal, is distinct from it. At the end of eight or ten days, this mass becomes firm, pale, and semi-cartilaginous, while the lymph opposite the walls of the canal remains gelatinous. Its consistency continues to increase; it becomes more circumscribed, and finally ossifies. It is this mass, forming a kind of soldering on the outside and on the inside of the walls of the medullary canal, which Dupuytren (to whom I believe we are indebted for a knowledge of most of the details concerning the cicatrization of bone) has termed the temporary callus. Its ossification is complete about the fortieth or fiftieth day. It is only after this time, that the substance between the walls of the canal, which is to be the permanent callus, ossify and knit the ends of the bones firmly together. When this has become ossified, the mass outside and inside the canal, that is, the temporary callus, is absorbed.

When the fragments are not kept in apposition, the phenomena are different: the temporary callus remains, and the medullary canal is not re-established. When the fragments are exposed to the air, as in cases of compound fracture, they unite, like the soft parts, by granulation. It is a remarkable fact, that the details furnished by Dupuytren are in accordance with the doctrines of our Hunter on the union of soft parts, and that the antients were not far from the truth when they supposed the fragments of a broken bone became united by a plastic matter exuded between and around them.

We perceive that the process, by which the fragments of a broken bone unite, is a long one—one in which a great and long-sustained effort is made, producing, through divers changes, a great result. It appears evident that it can be well carried on, only when the fragments are kept still. The first principle, therefore, to be observed in the treatment of fractures, is to keep the broken parts in a perfect state of repose; and this involves another—that any apparatus applied for the purpose should press equally on every part of the limb. We want, if possible, to clasp

the bone around, as if it were naked; and the more exactly the limb is embraced, the more efficiently will it be supported.

As the callus undergoes divers changes, not only with regard to consistency but also as to volume, the soft parts must also be subject to changes of position, which cause the size of the limb to vary. But the size of the limb will vary most, on account of the inflammation resulting from the injury the soft parts usually receive from the cause producing the fracture. From this variation of volume we derive the principle, that the apparatus applied round the limb should expand and contract accordingly.

As the progress of cure is a long one, as it takes from forty to fifty days to give solidity to the callus, and that complete privation of exercise for this time is detrimental to the whole body, more especially so to the joints in the neighbourhood of the fracture, which thereby become rigid, and not unfrequently the seat of incipient anchylosis, it must be a principle of great importance, that the apparatus applied should admit of exercise of the body, and of the joints near the fracture, to the fullest possible extent compatible with the stillness of the fragments themselves.

If we inquire how far the apparatus usually employed answer the indications required, we shall find them in many respects defective. The splints, which constitute the chief part of such apparatus, and on which we rely to keep the limb steady and motionless, cannot be made to press equally; and, when the limb is surrounded by them, they are usually fastened so that, if any variation occur in its volume, the box they form is in nowise susceptible of enlarging or diminishing accordingly. Whatever padding is employed to fill up the hollows between the splint and the limb, the pressure can never be perfectly uniform; and it has the inconvenience of keeping the parts in a state of heat. Pasteboard splints can be made to fit more exactly; but even these cannot be made to wrap round the limb with great nicety. Impressed with these defects, Seutin conceived that a bandage, rolled round the limb with perfect exactness, might be converted into one entire hollow splint, case or mould sufficiently strong to prevent motion. He effected this object by applying successive layers of bandage, with a thick coating of paste to hold them together, between each layer; and when the paste is dry, the limb is thus encased in a box or mould, which exactly fits it. It is impossible to deny that this apparatus accords with the principles we have deduced, and fulfils the indications much better than those usually employed. It presses equally on every part, and possesses sufficient resistance to supply for a time the place of the bone. It is, however, open to the objection, that it does not expand and contract as the limb may swell or diminish. In some cases, I believe, it has been necessary to remove it; and the proposal to cut it down in several places, so as to convert it into several splints, first led to the modification we have just now introduced. This proposal was made by a medical gentleman whom I was attending for a fracture of the fore-arm. Very shortly after this, I was called to treat a fracture of the humerus. The limb was in a state of tumefaction, which made me fear the absolute confinement of it in Seutin's apparatus; and I employed straight, separate splints, such as the French use. When the callus had become sufficiently strong to admit of some little motion, as the patient complained of the weight of the apparatus and the cramped position of his arm, I applied the apparatus of Seutin; but, as the limb was still tender, and swelled a good deal towards evening, I considered it would be advantageous to slit the apparatus open along the inside of the arm, so that it might yield and return upon itself according to the variation in the volume of the limb. It answered the purpose tolerably well, but was not sufficiently elastic to follow the limb in its changes of volume. In talking the matter over with Mr. Christophers, who was attending the case with me, it occurred to him, that if it could be made so, it would be a considerable improvement; and the means he suggested, which are as simple as ingenious, I immediately adopted. He proposed applying around the apparatus slit open, a certain number of elastic straps, made of India rubber, with buckles which admit of their being drawn to the requisite tightness. They are rather more than an inch wide, and rather longer than is necessary to encompass the limb. Four of these were applied, and converted the apparatus

into a case sufficiently elastic to follow the changes in the volume of the limb, and yet of sufficient strength to afford the requisite support. It appears to me that Seutin's apparatus, thus modified, fulfils, as nearly as possible, and much better than any other, all the indications required; and it must be evident that it will be even a greater boon to the patient affected with a compound fracture than to one whose fracture is simple.

In case the limb undergo a considerable diminution of volume, it will only be necessary to remove a longitudinal strip of the apparatus, instead of opening it by a longitudinal incision; and the strip should, of course, be removed or the slit made along that side of the limb on which the nerves and vessels exist and which can least bear pressure. We deem it not improbable that the apparatus, thus modified, will be found useful in the treatment of many diseases, where it is essential to keep the parts motionless, without exercising an unyielding resistance, or a pressure in the least degree unequal. Mr. Christophers proposes to employ it for that troublesome disease—a varicose state of the veins. I cannot help thinking that, if made light and applied with care, it will answer the purpose admirably. The results of our experience of its employment in cases of fracture will appear in the following cases:—

Case I.—Mr. Hemming, of No. 6, Piccadilly, fell down, about two years ago, and severely lacerated his right arm, in the neighbourhood of the condyles, which, since the accident, has been subject to swelling and pain. About two months ago he was thrown from a gig and fractured the humerus of the same side, at the upper part of the inferior third. There was considerable contusion and the whole limb became much swollen. I applied the ordinary apparatus for fractures of the humerus, which required great attention, on account of the inflammation of the soft parts. At the end of three weeks I substituted Seutin's bandage for that before employed, to the great comfort and satisfaction of the patient. I took, however, the precaution to slit it open, after it had become dry, along the course of the nerves, on the inner region of the arm. A great amelioration followed this change of apparatus. As, however, Mr. Christophers suggested, that it might be made more perfect by the employment of elastic bands, and as he had the kindness to prepare them without delay, I applied them, and found them fully as advantageous as I had anticipated. The case has gone on well, the patient having been able to move the limb a good deal and to take general exercise ever since the last apparatus was applied.

Case II. (as drawn up by Mr. Christophers.)—Elizabeth Dixon, aged eleven months, residing No. 1, Phœnix Street, became a patient of the Blenheim Street Dispensary, June the 19th, 1838. She is a strong, healthy child, not yet weaned. Her mother gave the following account: My eldest daughter, aged 10 years, was carrying Elizabeth, on one arm, and drawing a child's chaise with the other, when the child suddenly sprang backwards and fell on the pavement. She cried but little at the time; but, when put to bed, exhibited symptoms of great pain, and would only lie with her shoulders and pelvis raised. In this position I found her the next morning; she was exceedingly irritable, and apparently in much pain. There was a bruise, with considerable swelling on both knees, and a cut on the upper lip. Not feeling satisfied as to the extent and nature of the injuries, I called in Mr. King, who detected a fracture rather below the middle of the left femur. We applied Seutin's apparatus, which appeared to do well for two or three days, when the child seemed uneasy. The uneasiness was diminished by a longitudinal division of the apparatus, along the inside of the thigh. As soon as I had seen the advantage of the elastic bands, I added these to the apparatus; and ever since, the child appeared to move the body with greater security, and to be quite free from pain, although left to play on the floor with her brothers and sisters, without that care which the case appears to demand. The patient is now quite well.

Case III. (related by Mr. Christophers.)—S. Tomlinson, aged 49, a painter, residing at No. 10, Rose Street, Greek Street, became a patient at the Blenheim Street Dispensary, July 13th, 1838; he stated his case as follows:—"I was helping to carry a heavy case down stairs, and when nearly at the bottom of the flight, I thought there were no more steps, which caused me

to make a false step, and my foot to slip under me. On getting up, I was unable to stand, and reached my home only with the assistance of my friends." The patient was first seen by Mr. Andrews (Mr. King's house pupil) and myself. Mr. A. discovered a fracture rather below the inferior third of the left fibula. At four o'clock, Mr. King applied the apparatus of Scutin, although there was considerable pain, with swelling about the ankle. Ten hours afterwards, upon the advice of Mr. King, I cut open the bandage, and applied my elastic India-rubber bands.

14th.—At eleven o'clock, the limb had swollen, and the margins of the apparatus were a little apart; but the patient was free from pain, and seemed comfortable: he kept his bed for the four following days, and was quite easy. On the fifth day, he was able to move from his bed with crutches, the leg being kept in a sling. Since this time he has been able to take more and more exercise; the swelling has subsided, and the lips of the bandage are in contact. He is now nearly well.

IMMENSE CALCULUS SUCCESSFULLY EXTRACTED

BY THE BILATERAL OPERATION.

With a Drawing of the Stone.

By Hugh Fraser, Surgeon,
King's Royal Rifle Corps.

The following case is conceived more in the spirit of professional vanity than from its offering any thing new to the experienced hospital surgeon; but as the extraction of a stone of such terrible dimensions, through the perineum, must at all times embarrass the operator, and prove most highly dangerous to the patient, the case seems worthy of record. Besides, calculus is a disease so very rare in the columns of military disorders, that I question if there be now serving in the army a surgeon who has had an opportunity of performing lithotomy; I have, therefore, a pleasure in making known the success of the present case to my military medical friends.

Dr. Bond, Inspector-General of Hospitals, who has served in the army nearly thirty five years, and in almost every British colony, and consequently amongst a diversified description of troops, has only seen one soldier besides the present affected with stone in the bladder. In a varied service of twenty-five years, this is the first that I have witnessed.

Education so magnifies the operation of lithotomy, that, I believe, the young operator always proceeds to its execution with timidity: this ought not to be the case; the operation is as easily performed as any other in surgery.

Corfu, February 18, 1838.—Corporal Robert Allan, aged 33, King's Royal Rifle Corps, from the detachment of the corps at Zante, where he has been under the medical treatment of Staff-Surgeon, Dr. Grant, who reports that the man labours under stone in the bladder. Upon examination, all the most unequivocal symptoms of that disease exist, and a sound introduced into the bladder is readily made to strike against a foreign body situated behind the prostate gland. Has often passed bloody urine, but none lately: the urine is charged with a copious mucous deposit, and is of a whey-white colour, indicating ulceration of the mucous coat of the bladder, according to Sir Astley Cooper.

By the medical registers of the corps, Allan seems to have had an urinary affection for upwards of three years, but the existence of a vesical calculus was never suspected until he came under the treatment of Dr. Grant. Was treated by Surgeon Melvin, of the regiment for eneuresis from the 7th to the 16th December, 1834; and by Assistant-Surgeon Lamond, also of the regiment, from the 29th July to the 6th August, 1835, for a severe attack of cholera; but upon this latter occasion, no mention is made of any urinary difficulty. After this, a period of twenty months elapses before he is again admitted into hospital; during which period he is promoted to his present rank, and is actively employed in performing his duties, at times sufficiently arduous.

On the 29th April, 1837, the patient first came under my care, complaining of a urinary affection: stricture was suspected, and the urethra was explored with a sound several times passed home into the bladder, but the idea of a stone never thought of, was, of course, not looked for. At this period, had the ex-

istence of a stone struck the mind, it must have been discovered; but its depth behind the prostate eluded the sound, unless that instrument had had a large curve and been driven directly down upon it, which the one used had not; even now, the sound then employed will not strike the stone without care. Finding the urethra free, and that there was bloody micturitic urine, the case was considered one of irritable bladder, and treated accordingly. At this time it was not known that he had before been the subject of eneuresis. He remained in the hospital for ten days, when, thinking himself well, he went out to duty, and I heard no more of him until his present admission.

On the 7th January, 1838, embarked with his company for Zante. At the time felt very well, but on the passage was greatly exposed, for several days, to tempestuous weather; got a severe wetting, and caught so heavy a cold that his urinary difficulties were kindled up to a pitch worse than he had ever before experienced. Upon arriving at Zante reported himself to Dr. Grant, who, as I have said, quickly discovered the true nature of his disease.

Physical condition good and unimpaired, though 13 years in the army, seven of which have been spent in these latitudes; complexion swarthy; stature tall; body thin and muscular. By trade a blacksmith; a native of Goldclift, Monmouth, in England, where his father is a farmer. States that until the period of his enlistment was in the daily habit of drinking freely of cider—on an average more than a quart a day, on some days more than a gallon; also, that he has not been the most sober man in the world since he entered the service.

Low diet.—℞. Liq. Potassæ, Tinct. Hyosciami, aa. ʒiss. Mist. Camphoræ ℥x. M. Dosis, ʒi. ter in die.

21st February.—The medicine has alleviated all the most painful symptoms, excepting the pain at point of penis; urine not so white; the acid test proclaims it free from purulence, but the mucous deposit is inordinate.

Low diet.—Cont. Mist. Potassæ. Bibat Infus. Lini Comp. ad libitum.

26th.—Examined by Dr. Bond, and other medical officers, and the presence of a stone in the bladder confirmed. An operation proposed, which the patient is to consider of. Urinary system comparatively quiescent, but the urine is greatly loaded with white mucus. The bladder cannot be far off the point of ulceration, if it is not already so.

Cont. med.

2nd March.—Positively declines an operation; wishes to return to England; symptoms quiescent; health and appetite good.

Half diet.—Discontinue medicine, in view to see the result.

16th.—The painful symptoms have all returned since the discontinuance of the medicine; distressing pain at point of penis, with tenesmus upon going to stool; the latter is much complained of; urine more healthful looking, but every alternate day it continues to be grievously charged with ropy mucus, forming an adherent jelly-like consistence at the bottom of the vessel. Says that the more tea he drinks the clearer and more easily is the urine passed.

Half diet. Half a pint of wine.—Rep. med.

20th.—Has assented to an operation. Urinary system tranquil; general health good; urethra habituated to the introduction of the sound, as is the rectum to the finger, but the stone cannot be satisfactorily felt by the finger per anum; however, its existence is certain, and there seems to be nothing to forbid an operation, excepting the doubtful condition of the mucous coat of the bladder.

Spoon diet.—A Seidlitz powder every morning.

26th.—Operation.—Assisted by Dr. Bond, who held the staff and scrotum, and by Dr. Gogevina, an able Italian practitioner, who kindly lent me his French instruments, and in the presence of numerous spectators, the operation was performed at one o'clock this day, the patient, since last report, having been in every particular well prepared for it.

The position of the operator, and the direction of the incision, were widely different from that followed in the common lateral operation. The patient was placed as in the lateral operation, and staff being introduced and given to Dr. Bond, the operator, in a kneeling attitude, having his left knee and right

foot firmly resting on the floor, placed the thumb of his left hand on the tuberosity of the right os ischii, and the forefinger of the same hand at the base of the scrotum, so as to put on the stretch the perineal skin. With the other hand, and with a common scalpel, he made round the verge of the anus a transverse semicircular incision; in fact, the lunated cut of Celsus, as described and represented in Charles Bell's Principles of Surgery, only much closer to the anus, and no finger introduced into the rectum. It was of this form:—

The centre of the incision in the raphé was eight or nine lines in front of the anus, and about eleven or twelve on each side the raphé, towards the tubera ischiorum. This done, the dissection was quickly prosecuted, the forefinger of the left hand guarding the rectum.

This incision, in the hand of a practised operator, might, and perhaps ought to be made down to the prostate, in three seconds, with one deep sweep of the scalpel; but as I had never before operated for stone, extreme caution in making the incisions had been determined upon. The membranous portion reached was slit open, and the beak of the lithotome planted in the groove of the staff, when, taking hold of the latter instrument and depressing it, the lithotome was steadily insinuated through the prostate, which made a little resistance; its entrance into the bladder was marked by a stream of urine. The lithotome was then deliberately turned from having its convexity looking sacral to the pudic aspect, and withdrawn having the beak directed to a point an inch below the umbilicus, the handle much depressed upon the rectum and anus. By this mode the prostate gland is cut into about two equal portions, the one pudic, the other sacral; so that the floor of the incision is the rectum, the *traject* from the prostate to the external wound almost perpendicular—an incision obviously much guarded against cellular infiltration.

The lithotome was set at nineteen lines and a fraction; if set at twenty-one lines the pudic arteries are hazarded. The cut into the bladder was sufficiently large for the extraction of any decently-sized stone; so ample was it that the two forefingers of a large hand, together with the forceps, were with facility introduced, and the stone grasped with the forceps; but as it was lying lengthways across the neck of the bladder, deeply sunk behind the prostate, the sacral division of which formed a bar betwixt the fingers and stone, it was impossible to bring it forth. It was endeavoured to be turned with the point of the fingers and forceps, but its profundity partially thwarted this. It was grasped again and again, and when pulled a considerable way out of the bladder along the wound, was dislodged from the forceps by striking against the narrow arch of the pubes, and resisted by the undivided fibres of the transversalis muscle, which is not entirely divided in this mode of operating, when it again retired into its deep bed.

It was tried to be crushed, but such was its solidity that the handles of a strong pair of forceps bent upon themselves and became powerless. Dr. Gogevina introduced a powerful lithontriptic instrument; he readily seized and fixed the stone, but the moment the drill-bow was forcibly applied, it escaped from the mouth of that instrument. When the stone was afterwards laid upon the table, this instrument was seen to be incapable of grasping so large a stone so as to smash it, even in this exposed situation.

Seeing every effort fruitless, and time running on, as every thing had been conducted with the greatest composure, a bistoury was laid hold of, and a *dernier coup* of every obstinate soft part resolved upon. After some feeling about with the fingers and forceps in the bladder (the viscus by this time having contracted itself over the stone), the stone was most luckily caught very firmly in the only diameter (as was afterwards proved on the dead body) that it could possibly have been drawn through the perineum; and moving the forceps forcibly but steadily about, in the dilative mode observed in the abstraction of im-

pacted children from the vagina, it was at last, to the gratification of all present, extracted, without the further assistance of the knife. It positively protruded in the perineum, and came plump out like the head of a child. I believe that the stone, partially turned with the finger from its transverse position in the bladder, was swung round by the contraction of the viscus, and so its smallest end presented at the wound.

The bladder was syringed, and without any dressing whatever to the wound, the patient was put into bed, apparently not in the least exhausted; indeed, he wanted to walk to bed, and throughout the whole proceedings displayed an uncommon fortitude. No artery required a ligature; the blood lost was inconsiderable. Forty drops of laudanum had been given an hour before the operation; he declined any more when put into bed.

It appeared that the stone was grasped in four and a half minutes after the first incision, and had it been of moderate size would have been brought away immediately, but the embarrass-

Above the line are seen deep-splintered indentations made upon the skin by the forceps; below is exhibited the rough, red, and bloody growing coat of the stone.

ments encountered kept the patient upon the table twenty-eight minutes. In shape and size the calculus greatly resembles the egg of a turkey, and it is somewhat flattened; it has a bloody rough growing coat, and would probably in no long while have become adherent to the bladder. Its weight is five and a quarter ounces—add a quarter splintered off its small end in the attempts at extraction, and you have a stone weighing five and an half ounces, apothecaries' weight. Measuring the long circumference it is exactly seven inches, and the circular, or short diameter, is six inches and three-tenths.

It is proper to confess that the force employed in the extraction was very great, but I do not know that any undue violence was used. Certainly, the soft parts sustained a heavy continued pressure, and could not avoid being somewhat bruised; but I do not think there was any laceration, as some persons supposed.

In a case of lithotomy which occurred to Mr. Lawrence, at St. Bartholomew's, and which is reported in the Lancet,

where a stone only weighing four ounces, seven drachms, occasioned that celebrated surgeon as much perplexity and violence in its extraction as the one in question; but the long circumference of this stone was eight and a half inches; and the short, six inches. This patient expired shortly after the operation.

The following quotation is so applicable to the present case, that I shall make no apology for giving it here, and the more especially as the sentiments expressed relate to a most important practical question—a question to be alone decided by extensive experience:—"All present seemed to agree in regarding the manual dexterity of the operator on this occasion as unequalled, but thought the violence used in dilating the opening, and pulling out the stone, extreme—if not unjustifiable. Mr. Lawrence, however, observed in the ward to which the patient had been returned to bed, that it was impracticable for any operator to have removed the stone with a less degree of force; and that a comparison of the dimensions of this enormous stone, with those of the inferior aperture of the pelvis, would immediately satisfy any person on that point. He stated that the incision in the prostate and neck of the bladder, whatever instrument is used, and however freely it is employed, will not allow the extraction of stones, even of ordinary magnitude, without more or less of dilatation and laceration; and that large stones cannot be extracted through the perineum without great force. In proof that the dilatation of the wound by the forceps, by which this object is effected, is not necessarily very injurious, he mentioned to the pupils that the late Mr. Martineau, of Norwich, who enjoyed the highest reputation as a skilful lithotomist, always operated with a blunt gorget, pushing it on into the bladder, after he had cut into the groove of the staff, then carrying in the forceps on the gorget, and trusting to dilatation by the forceps alone for making a sufficient aperture to allow the extraction of the stone."

27th.—Has passed the night beyond all expectation well. At bed time had half a grain of morphine, which has procured good sleep; not a pain excepting directly under the arch of the pubes, where he says the stone bruised him; some thirst; pulse 80; bed soaked with urinary and feculent discharges, the latter he felt incapable of restraining; has just risen from bed, unassisted, and washed himself in a warm bath.

Spoon diet. Soda powders at pleasure.

Meridie.—Disposition to singultus, and to vomit; pulse 100, firm; no particular pudic or perineal pains.

Venesectio ad ℥xx. A Seidlitz powder.

7 o'clock P.M.—Blood drawn healthy; says he feels very weak, and not at all well; but the countenance and pulse do not indicate exhaustion; the latter 118, and firm; constant nausea and frequent vomiting; bowels torpid.

Rep. Venesectio ad deliquium. Habeat Olei Ricini, ℥i.; Tinct. Opii, ♏xx. in haustu.

10 o'clock P.M.—Twenty more ounces of blood abstracted without his fainting; this portion shews a firm buff. No vomiting since the bleeding, and the stomach retains the oil; deluged in a healthful perspiration, and expresses himself as quite easy; aspect open and tranquil; pulse 100, small and frequent.

Haustus Anodyn. si opus sit, in nocte.

28th.—An excellent night's rest without the anodyne; pulse 88; very little nausea; bowels continue torpid; urine flows freely from the wound into the bed.

Descendat in balneum. Olei Ricini, ℥iss. Soda powders at pleasure.

10 o'clock P.M.—At five o'clock this afternoon great apprehensions were entertained, the body having suddenly become bedewed with a cold clammy perspiration, and he had a jaundiced look. However, a little stimulus seems to have dispelled these bad symptoms, and he again appears to be doing well. Pulse 120; oil has operated rather too powerfully; stomach tranquil.

To have a good glass of brandy punch, with one grain of Morphia.

29th.—Slept the whole night, and awoke this morning so strong and comfortable, that he said to his comrade, "I now feel I shall do well." Pulse 110; countenance open and good; wound making strong curative efforts; no infiltration about it.

Ginger beer at pleasure; rice and jelly at intervals.

6th April.—It is useless to prolong the daily reports: since the above has advanced in the most prosperous manner possible; very little urine passed through the wound yesterday, which is all but closed.

To sit up in a chair. Diet—soup, fowl, porter.

12th.—Wound firmly closed; appetite and digestion perfect; and he daily gains strength.

Diet continued.

16th.—Walking about stoutly, and looking healthful. A sound was introduced into the bladder this morning without difficulty or pain, and the viscus felt to be clear of all fragments of the stone, portions of which, it was feared might have remained behind. The urine continues to hold a quantity of mucus, but nothing like what it did before the operation.

26th June.—Three months have now elapsed since the operation, and the patient is still under treatment; but he is about to be discharged, and will only be requested to perform light duties. The general health is as good as it ever was, and the man is looking better than I ever saw him do; but the urinary organs remain at fault. The urine shews now very little deposit; nevertheless, in despite of the exhibition of various lithontriptics it has not assumed a deep natural colour, its appearance is that of an infusion of straw or green tea. In character it is albuminous, as shewn by its dropping a coagulum on the addition of nitric acid. This morbid condition assuredly rests with the kidneys. I believe that the mucous coat of the bladder has recovered its heathful integrity; but knowing that disease of one part of the urinary system is soon continued to other parts, it is not to be wondered at the kidneys being involved in the present instance. Upon any unusual exertion or gastric plethora he experiences an uneasiness in the region of these organs; however, I can perceive nothing but what a good digestion and time may be expected to overcome.

So far, therefore, as the mere operation is concerned, perhaps the annals of lithotomy do not furnish a more happy termination.

Corfu, 3rd July, 1838.

NOTES OF A LUSUS NATURÆ.

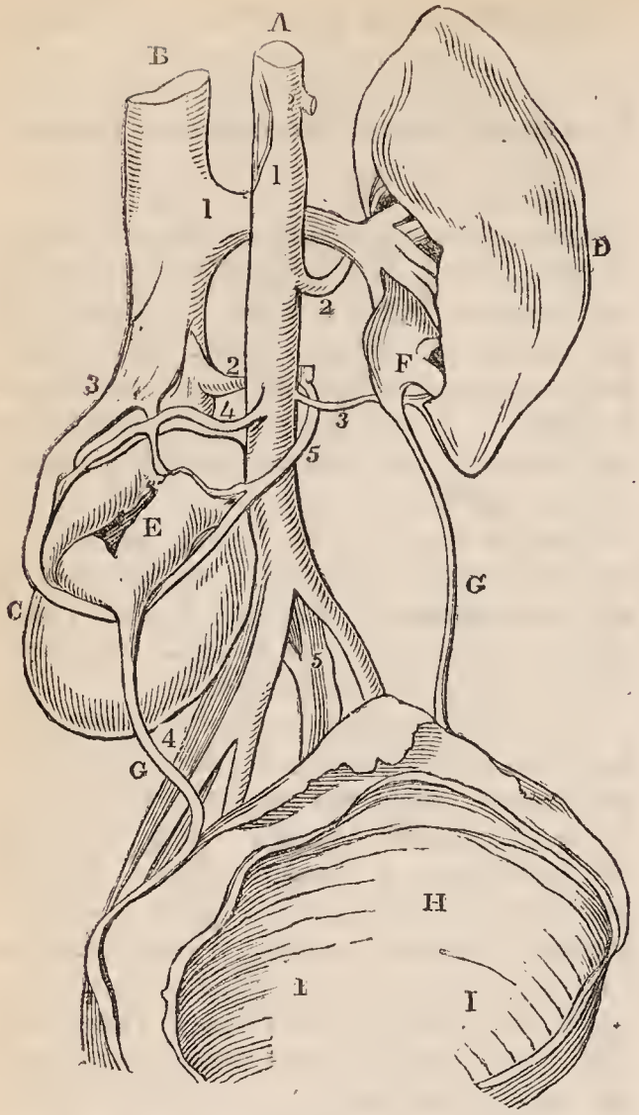
To the Editor of the Medical Gazette.

Sir,

Deeming every deviation from the usual situation and structure of the viscera of the human body worthy of being put on record, as likely, when sufficiently numerous examples have been collected, to lead to a more profound knowledge of the ways and means by which nature carries on the various functions of secretion in the kidney, liver, &c. &c., I am induced to send you an account of an extraordinary floating kidney, met with in a patient that died of consumption in the county of Sussex.

I chanced to place, during the post-mortem examination, my hand near the umbilicus, and was surprised to feel a hard moveable mass. On opening the abdomen a loose oviform kidney was found, with its anterior end projecting forward amongst the viscera, and its posterior crossing and resting upon the lowest part of the vena cava and aorta, just at the commencement of the right iliac artery. It was slightly attached, or rather slung by a very long fold of peritoneum to the brim of the pelvis, near the sacro-vertebral junction. This kidney was a compound one, its circumference being formed of five lobules. The pelves were not situated at the inner edge of the gland, as usual, but upon the centre of its upper surface; each of the latter sent off a tube, which, after uniting with the others, became a capacious white urinary sinus, two inches long (*Vide* sketch), which stood out from the kidney, and terminated by narrowing into a ureter of short dimensions. The posterior part of this viscus was impressed by a deep transverse sulcus, where it at times lodged on the brim of the pelvis, and by a perpendicular one where it lay upon the vena cava. Its vessels were very peculiar, as described in the latter part of this paper.

The left kidney was found very much higher than the right, and about two inches more outwards than ordinarily. It consisted of six or seven lobules, marked externally by sulci: this gland was nearly twice the common size, with the pelves in the natural situation. A separate tube arose from each lobule, all



A. *Aorta.*

1. Spermatic artery.
2. Left superior emulgent.
3. —— inferior emulgent.
4. Right superior emulgent.
5. —— inferior emulgent.

B. *Cava.*

1. Left emulgent vein.
2. Right inferior emulgent.
3. —— superior emulgent.
4. —— common iliac.
5. Left common iliac.

Kidneys.

C. Right floating kidney.
D. Left stationary kidney.
E. Urinary sinus.
F. Urinary sinus.
G. G. Ureters.
H. The bladder.
I. I. Entrance of the ureters.

of them afterwards uniting to form a large urinary sinus, similar to that found in the misplaced one, which narrowed into a ureter of common size and length.

The trunk of the abdominal Aorta was anterior to the Cava, and it was smaller than usual, about two inches in circumference, and sent off the—

1. *Right spermatic* artery; and next to that the

2. *Left superior emulgent*, which was smaller than common; it branched into four, and entered various parts of the upper portion of the pelvis of the kidney. Two inches lower down was sent off a

3. *Left inferior emulgent* artery, which passed direct, and entered, not the pelvis, but the lowest portion of the above-named viscus; it supplied the infundibula of the inferior lobe. A little below, on the opposite side, was sent off the

4. *Right superior emulgent*, which crossed before the cava, ran along the upper part of the urinary sinus, divided into four branches, and entered as many of the superior renal lobules by the pelvis of the right kidney.

From the centre of the aorta there arose, close to its bifurcation, a

5. *Right inferior emulgent* artery, which came off at the bifurcation; it divided; sent a branch to the upper lobules, above the pelvis of the kidney; the trunk then continued its course, descending along the lower margin of the urinary sinus, to reach and supply the inferior ones. Just at the termination of the aorta, two

6. *Arteriæ sacri mediæ* came off and ran down, distributing their branches as usual.

The Vena Cava was of extraordinary magnitude when compared with the aorta, being more than three inches in circumference. The size and distribution of its branches were uncommon, but such as well corresponded to that of the arteries, except that three large renal veins returned the blood from four small emulgent arteries. The

1. *Left emulgent* vein arose by five large branches from the pelvis of the kidney; but above, before, and anterior to the arteries, it was joined by a branch from the capsula renalis, and by the spermatic vein: all when united formed a trunk larger than the aorta, constituting an immense emulgent vein of twice the ordinary length, and of two inches in circumference; it passed in rather a tortuous course behind the aorta, and joined the trunk of the cava. The

2. *Right inferior emulgent* vein arose by slender branches from the upper and lower part of the pelvis of the kidney and urinary sinus; was very long; passed over the left iliac artery; encircled the aorta; formed abruptly a large vessel, which received two or three twigs from other viscera, and became a trunk of more than an inch in circumference, and entered the cava above the right superior emulgent vein. The

3. *Right superior emulgent* vein arose by two branches from the inferior part of the pelvis of the kidney, and by one from the superior lobules: it formed an arch over the kidney and urinary sinus, having become a trunk of near an inch in circumference, and entered the anterior emulgent of the cava, lower than the inferior emulgent of the same side. The

4. *Right common iliac vein* was very large, and almost resembled the continued trunk of the cava.

5. *Left common iliac vein* passed up as a middling-sized single trunk, but divided, and entered the cava by two distinct branches, leaving a triangular space between them.

6. *The sacra media veins* were double.

After contemplating the many extraordinary deviations from custom, as above noticed, a search was made to ascertain whether the mesenteric or portal veins were also double in this body; but scarcely any, except the vessels appertaining to the urinary organs, appeared to differ from their ordinary distribution. A preparation of the above parts is in my possession, and may be seen by my brother professionals.

I am, sir,
Your obedient servant,
Wm. H. Judd, M.R.C.S.
Surgeon in the Guards.

August 7, 1838.

FURTHER OBSERVATIONS ON MESMERISM.

To the Editor of the Medical Gazette.

Sir,

Permit me to send to you, for insertion in your journal, an account of some additional facts and views in mesmerism. The former, like the phenomena, accounts of which I have already placed before your readers, were shewn to me by Dr. Elliotson, having been elicited, during the treatment by mesmerism, of two of his patients, Elizabeth and Jane Okey. The means which I have had of verifying the marvellous facts that I venture to narrate, have been on the recent as on the former occasions, of which I have availed myself, such as to satisfy me fully of their truth. For the correctness of the views to which they seem to lead, I do not equally pledge myself. It is on the contrary presumable, that the latter will require to be greatly modified and altered, as fresh facts come to be recorded in this strange section of the physiology of the nervous system.

I. The mesmeric influence, whatever its source and nature may be, is capable of being communicated to, and retained in, certain substances, and not in others. Among the substances which are capable of receiving and retaining the mesmeric influence, are gold, silver, platina, water, the moisture of the skin. Among the substances which are not, are copper, zinc, tin, pewter. If you hold a sovereign in your hand a minute, and then carefully wipe from it the moisture it may have contracted, the metal is found to have had imparted to it, and to retain for a time, some of the mesmeric influence. If either of the two Okeys, when in mesmeric delirium, close her hand on a sovereign thus mesmerized, she either falls into mesmeric coma or into mesmeric trance, or her hand and arm, or her hand alone, become firmly fixed in mesmeric cramp. These four effects are graduated results of different doses of the mesmeric influence, the first indicating the largest dose.

In one of the sisters, the last or slightest effect, mesmeric cramp, is capable of being produced, *even when she is in her natural state*, by the contact of mesmerized metals. The cramp is just like common cramp; it is confined to the muscles of the part to which the mesmerized substance has been applied. It yields, when excited in the natural state, just as when excited in the state of delirium, to the influence of the fingers held pointed towards the cramped part at the distance of half an inch or an inch.

In experiments made with any one of the mesmerizable metals, the effects produced are observed to be proportionate—to the shortness of the interval that elapses between mesmerizing the metal and the commencement of the experiment;—to the quantity of metal used;—to the vigour of body and health

of the party who may have mesmerized the metal.

The effect of the first element is well exemplified in the following instances: Jane Okey was told to close her hand upon a strongly-mesmerized sovereign; she immediately, upon doing so, fell into mesmeric coma. Upon recovering, she was told to pick up the same sovereign which had fallen from her hand upon the floor. She stooped for this purpose, when, immediately upon taking hold of the sovereign, she fell into the mesmeric trance, and remained fixed in a stooping posture, her hand touching the floor. Upon recovering (the sovereign having been, as before, removed from her hand without being again touched, however, by another's hand) and being told again to pick it up, she stooped, and having taken it up, had partly raised herself before the state of trance supervened. Upon recovering, being again bade to do the same, she had raised herself more nearly upright before the trance supervened. In the end, the sovereign, after seven or eight of these trials, had so little virtue left in it, that when taken up by her, it was some seconds before it affected her, and it then only produced cramp of the hand. Finally, this effect vanished; the mesmerizing influence, originally communicated to the piece of money, having been expended.

If a piece of metal that has been mesmerized, is left some minutes *untouched*, the virtue of the metal is equally found gradually to evaporate.

The influence of quantity of metal in the experiment is thus shewn:— Instead of one sovereign being used, half a dozen may be mesmerized, and placed together in the hand of the patient; upon this, coma instantly supervenes, which is much more profound, and lasts longer than that produced when one sovereign only is tried.

To be shewn the influence of different states of constitution, we went into one of the wards of the North London Hospital, when a sovereign was held in the hand by several patients in succession, which produced very different results when taken from each by the somnambulist. A patient with fever first held the sovereign, which was then taken by Jane Okey, she being, as required in the experiment, in the state of mesmeric delirium, or somnambulism: no effect followed. No effect again followed when the same piece of money had been held by a young person who, having recently miscarried, was in a state of great bodily weakness. No effect again ensued when the sovereign had been held by a young woman, whose system was affected with mercury. On the other hand, a little girl in good health, but affected with ringworm, on holding the sovereign mesmerized it, so that it produced the state of trance in Jane Okey on her taking it from her.

It has been mentioned that silver and platina are less mesmerizable than gold; and that copper, tin, zinc, and pewter, are not mesmerizable at all. The latter circumstance was not ascertained at first; for the moisture of the skin is capable of retaining and conveying the mesmeric influence. A halfpenny that has been held in the hand, is thus rendered capable of exciting mesmeric effects. When, however, the slight quantity of perspired fluid that may have been contracted by contact, has been removed from their surface, neither of the metals last named retain any portion of mesmeric influence: nevertheless, one of the least of these metals (if not the rest) can *transmit* the mesmeric influence; which, indeed, has been shewn by former experiments, but is shewn still more remarkably by the present. Elizabeth Okey being in mesmeric delirium, held a pewter porringer in her hand by the handle: half a dozen mesmerized sovereigns were then placed in it; in a few minutes the trance supervened, and she was rapidly falling into coma. The sovereigns were then hastily removed, and in time to prevent the coma being perfected: so she recovered first into the state of trance, then into her ordinary state of delirium.

But although copper, zinc, tin, cast-iron, and pewter, are non-mesmerizable substances, yet are they not entirely inoperative upon mesmerized patients. When Elizabeth Okey wakes from the state of mesmeric trance into that of delirium, under ordinary circumstances the instant of transition appears attended by a pleasurable sensation, and her countenance lights up with a lively expression. But if, when she is yet in the trance, a piece of either of the non-mesmerizable metals is placed in her hand, the instant of waking into delirium is attended with a sudden start, as if she felt some unpleasant physical shock.

Again, when she has been thrown, by holding a mesmerized sovereign or shilling, into coma or delirium, the continued contact of the mesmerized metal prolongs these conditions. It is then difficult to extract the piece of money from her grasp, so firmly are her fingers closed upon it. The means that are on other occasions available to relax the mesmeric cramp of the muscles of the hand, do not now succeed. It is in vain that the operator holds his fingers pointed towards her closed hand. The mesmeric cramp is not relaxed. But if he now employs the agency of a non-mesmerizable substance, insinuating, for example, with this object, a tin or brass extinguisher into the closed hand, the cramp is found speedily to yield to the ordinary manipulation, and the hand opens as quickly as if the mesmerized metal were not in it. The influence of the mesmerized metal is thus exactly neutralized by that of the non-mesmerizable substance.

A third instance of the interference of the non-mesmerizable metals, is the following: — After having been thrown into profound coma, Jane Okey for several minutes, upon her recovery, appears intensely drowsy and heavy for sleep, nor do any common means rouse her: but if a piece of non-mesmerizable metal is pressed against her forehead, or her hand, she becomes in a few seconds awake and animated, and continues so if the contact of the metal is maintained. If the metal is removed, in a few seconds more she relapses into heavy drowsiness, from which again she may be roused by contact of the non-mesmerizable metal.

In this marvellous inquiry, wonder succeeds wonder. As soon as one has become reconciled to the idea that the contact of one's hand can communicate to certain substances the mesmerizing influence, one finds, beyond, a yet more unlikely fact to examine and to be compelled to admit. Simply looking upon a mesmerizable substance, placed at the distance of two feet from you, will mesmerize it. So if you look for a minute upon a sovereign placed at the distance indicated, and the patient then takes it and holds it, squeezing it in her hand, a slighter or greater degree of mesmeric effect follows. Nay, if you desire her to hold out both her hands (she being, of course, as for the other experiments, in the state of delirium), and you look for a minute intently upon one, and desire her to shut that hand, and to keep it so shut, squeezing it hard at the same time, the same result ensues. The moisture upon the hand became mesmerized, and has affected her. In this experiment, her closing the hand you have not looked at, is followed by no mesmeric effect.

There remains to be mentioned another instance, even more improbable than the last. If you look for a minute fixedly upon one spot of a mesmerizable surface, as, for instance, a stone mantle-shelf, and a sovereign is then placed by the patient herself on the spot which you have looked at (the sovereign having been ascertained, the instant before, not to be mesmerized), if the sovereign be allowed to remain a minute on the spot that you have so looked at, then, on the somnambulist taking up the piece of money, mesmeric effects follow. For the sake of brevity, I do not mention the precautions taken to prevent the patient in this and other experiments *guessing when the effect is to take place, and when it is not;* I shall only repeat, that I have seen these experiments so conducted, that ***I am certain*** the patient was not aware of the result which was expected to follow, and which did not fail to follow.

Thus by looking upon a mesmerizable body you may so mesmerize it, that another mesmerizable substance laid upon it shall from it be mesmerized sufficiently to produce decided mesmeric effects upon patients susceptible of this peculiar agency.

II. The facts which I have thus narrated, and others that I have before communicated through the pages of this journal, seem to me sufficient to establish the existence of a peculiar *physical influence* that emanates from living beings, and is capable of producing sensible effects upon a greater or less number of our own species, and of one or more species of the lower animals. What that proportion is — what its limits and essential conditions are — and to what good or harm mesmerism may tend, are points which have yet to be ascertained. I have looked into the subject, in its speculative bearings only, as a branch, not of therapeutics, but of physiological science. So considered, its interest goes far beyond that of any new class of phenomena which has come under my observation. It is,

indeed, not confined to the discovery of a new and powerful physical agent. The highest interest of the subject is psychical, and is displayed in some of its relations, to elucidate which I shall employ the remainder of this statement. These relations present themselves on contemplating the phenomena of mesmeric prevision, and of transposition of sensation.

Let me premise, however, before touching on these extraordinary inquiries, that the connexion of these classes of phenomena with mesmerism is probably accidental. There is reason to believe that mesmerism brings them out in those persons alone who are, without mesmerism, capable of, and likely to exhibit them. It appears to me probable, that mesmerism only acts in reference to these phenomena, by strongly disturbing the nervous system, so that in those who have a certain pre-existing peculiarity of bodily organization in reference to the mind, the phenomena in question are thus enabled to develope themselves.

In my last letter I mentioned two instances of prevision manifested by Elizabeth Okey, and observed that they might be explained by supposing either that she unintentionally caused the disorders which she imagined herself to foresee, or that she actually foresaw them. I will mention another, in which I find it very difficult to suppose the first of these two explanations sufficient. A month before the 14th of June, Elizabeth Okey declared during the mesmeric trance, that on that day her usual delirium would assume a new character; that she would then cease to be without common bodily feeling, as hitherto was her state (and as her sister remains) during the mesmeric delirium or somnambulism. On the evening of the 13th of June, Elizabeth Okey was placed in the state of delirium; and she was then found to be, as she had always before been, insensible to touch when in that state. On the morning of the 14th, she was again rendered delirious, when she now evidently had feeling, exactly as in her natural state. She has since continued to preserve common sensibility whenever the delirium has returned. It appears to me more difficult to suppose that the patient's imagination produced this difference in herself, than that she actually foresaw its occurrence.

Subsequently to my last communication I have witnessed in Elizabeth Okey the marvellous phenomenon of transposition of sensation. I have seen her on three occasions suddenly assume a new condition, and be without eye-sight, hearing, touch, or taste; while she has distinctly perceived some objects placed a few inches from her, through a sense, more akin to vision than to any other, that has been suddenly developed in her hand. In making this statement, indeed, I am painfully aware that my testimony will not be received, as I have reason to think it would be, if given in evidence of any thing popularly credible; nevertheless I venture, as the phenomenon is purely physiological, to expect thus much of my readers, that they will take the trouble of seeing the condition I describe, before they declare my interpretation of it to be false.

I have said that I consider the principal interest of these strange facts to be psychical. I venture to put the following interpretations upon them:—

Hitherto the tendency of physiological science has inclined towards materialism. Every new discovery regarding the nervous system up to the present time has tended only to shew more and more the dependence of the mind on the bodily organization, by parcelling out and assigning separate mental operations to separate parts of the brain or nerves. And, however, by reasoning drawn from other sources, one has shaken off or struggled against the weight of the physiological argument, one has always felt its influence in straitening those chinks and openings, through which one has caught glimpses of spiritual existence, and its force in habituating us to attribute more and more influence and compulsion to matter.

But the phenomena which I now mention that I have witnessed, and which are already admitted as part of Nature by some of the first surgeons and physicians of France and Germany, seem directly to lead to other conclusions, and to support those convictions which we derive from a higher philosophy.

I think that the phenomena of prevision and transposition of sensation (and those of clairvoyance likewise, if the latter are true, and there is any thing in them beyond the workings of an over-active imagination) naturally lead to the supposition, that they result from

the workings of a spiritual nature, in a certain independence of those bodily organs to which it is normally closely tied and bound. It is conceivable, that, in such cases as I have described, when all the common avenues of sensation are occluded—when eye-sight, taste, touch, hearing, are suspended—and when a sort of vision is sensibly exercised by some part of the common surface of the body—that these phenomena arise from the mind being in part dislocated and displaced from her corporeal tenement, holding on with misplaced attributes to unaccustomed points and corners of the frame. It is conceivable, again, that in that wrapt and mysterious state, in which the individual is giving utterance to remote anticipations that are strangely verified, the mind is acting independently of its usual organs, and, with the character of spirituality, is freed from the restraints of time, as in clairvoyance (if that state ever exist), it would appear to be partially free from the restraints of space. Man, we are told, was made in the image of God: these may be partial revealings of the parity of the spiritual nature of the created being to that of his Creator. There are many things in tradition and popular belief, which would lead one towards such a hypothesis as that which I have advanced, as they might equally find their solution and explanation in it.

From Homer to Shakspeare and Scott, the great observers of mankind and knowers of human nature, have represented the prophetic spirit as occasionally manifesting itself immediately before the approach of death, when it may be supposed that the soul is loosening herself from her corporeal residence. And who is there, who has not himself met with perfectly authenticated instances of communications strangely felt, rather than made, of the time of the dissolution of an absent friend? Perhaps in the origin of such superstitions (for superstitions we habitually consider them, although from their universality they seem to deserve the consideration even of philosophers) there may be something in common with the source of the wonders of mesmerism.

I am, sir,
Your obedient servant,
HERBERT MAYO.

August 8, 1838.

MEDICAL GAZETTE.

Saturday, August 11, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

NECESSITY OF LECTURES BEING PRACTICAL.

A REMARKABLE characteristic of the English system of medical education, and in which it differs from all those pursued on the continent, is the union of the principal medical schools with the large hospitals, the lecturers in the former being selected from amongst the medical officers of the latter. We have always regarded this arrangement as full of benefit to all the parties concerned in it, and we look upon all attempts to separate them as in the highest degree blameable;—for there is nothing which so certainly secures, that the medicine taught will be that which is purely and usefully practical as that its teachers should be actual practitioners. There can be no doubt, also, that as a general rule (though there may be some exceptions) the physician or surgeon who frequents an hospital, has his mind more constantly bent towards practical utility in all he says or does, than others have. From the desire which he naturally feels to effect the most speedy possible cure of the cases under his charge, he is led to disregard all useless theories and doctrines, and to exert all his energies to remove the disease, without caring much for its mere name as given out by the schoolmen. Now if this practitioner can, as a lecturer impart the knowledge he thus acquires—if he can teach his pupils to detect the prominent features of each disease, and to apply at once the appropriate remedies for its cure or alleviation, it matters not whether he teaches them

to call one a *neurose* and another a *phlogose*, nor whether they know the verbal meaning of those terms (and their real meaning who can tell?)—if he can instruct them in these essentially practical parts of the subject, he will supply them with all that nine-tenths of them stand in need of, and will confer a greater benefit on the public through them than a century of theorists would effect.

Such is the scheme of education on which the best, and we may add, the most truly scientific practitioners of the present day were brought up; and we regret to see that it is now frequently departed from. For while we are daily proud to hear of the high repute which English practitioners deservedly receive for their pre-eminent merits in their proper sphere — by the sick bed, let us look at the condition of practitioners in a country where a scheme of education nearly opposite to that we have mentioned is pursued—we mean Germany. There, while the theory of medicine and all its collateral sciences are carried on to an extent that is never thought of here, yet a more inefficient body of practitioners cannot be found in any intellectual country of Europe. We, of course, speak of the mass, as we do of the mass of English practitioners; and we have no fear of contradiction (for the confessions of the best men of their own country confirm our assertion) when we say that the mass of German surgeons and physicians are scarcely more fit to practise their professions than our chemists and druggists. The reason of all this is, that they study medicine at universities not connected with hospitals; that their lecturers are scarcely, or not at all, practitioners; and, above all, that their teachers of the sciences collateral to that of medical practice are strictly professors of those sciences, and unconnected with medicine.

It is true that this arrangement has secured to Germany the possession of some first-rate anatomists, chemists, and botanists, all of whom have been drawn from amongst the students of medicine; but this has been secured at the expense of the public good, for it has led to this, that only the comparatively inferior students devote themselves to practice. Such will always be the case so long as there remains in Germany a sufficient number of universities, with a superfluity of teachers in each, to secure to any one who will steadily pursue any other science than that of practical medicine, a sufficient income for his maintenance in the confined, but, to him, happy study of his favourite subject; and, unless driven by necessity to a more profitable pursuit, the majority of students will always prefer that branch of their education to which most importance and prominence is given by the talent or reputation of its teachers.

All this may be well in Germany; and if the German public are content that their practitioners should be ignorant of their profession, in order that their philosophers may be the wisest of the world, it is not important to us. But the same plan will not succeed in England; and we regret to see that the employment of other than medical practitioners to lecture on the collateral sciences is becoming daily more general in our schools, and that it is now even a subject of boasting with some, that none of their lecturers practise their profession in order that they may give their whole time and talent to the teaching of their classes. The innovation is as dangerous as the reasoning is bad, though we admit it to be plausible. Formerly none but physicians thought of teaching chemistry, or botany, or materia medica, in our medical schools, and none but surgeons or physicians occupied themselves with anatomy; and thus taught, those

subjects instructed the pupils in the principles that were to be applied every day in the Dispensary or at the bed-side. But now, a professed chemist teaching in every medical school, the chemistry is that of the laboratory, or of the chemist's study—not that of the medical practitioner; more than half the course is consumed in going over the doctrines of heat, light, and electricity, the applications of all of which to medicine (at least so far as the lecturer gives them) might be included in three lectures. Then succeed metallurgy, and the chemical processes of all the arts—but medicine; and this, under the name of animal chemistry, is slurred over in the last three or four lectures, when the pupils and the teacher are alike wearied and inattentive. Now we are far from objecting to a scientific knowledge of chemistry; but we assert that, as at present conducted, the medical applications of the subject are by far too much neglected.

It is the same with botany, which has become every year less applicable in medicine, as its special professors have built it up into the form of a science. And so to a less extent it is even with anatomy, though as yet it has not altogether passed from the hands of surgeons: it is now taught far more diffusely than formerly; there is not that single eye to practical utility which should govern all teaching, directed as it is to the mass of students, of whom ninety-nine will have to pursue practice alone for one who will pursue the philosophy of his subject.

We have always said that the education required of students has not been sufficiently practical, nor is it even yet so, although many great improvements have been made, and especially, we feel bound to say, by the College of Surgeons, whose last curriculum, shortening the whole period of medical education to four years, of which at least two are to be devoted to hospital practice, is full of benefit. The system, however, which we have above alluded to, by which the teaching of collateral sciences is given into the hands of any but medical practitioners, will overthrow much of the advantage which recent reforms would otherwise confer. Let us observe what is the case in Germany, where this system is most completely adopted—anatomy and physiology are the subjects most prominently taught, and most studied by all the students: the result is, that few students acquire that kind of knowledge which they should have to fit them for practitioners, for the anatomy is not that of surgery or medicine, but rather becomes that of zoology. Now it is well known that although anatomy is thus pursued, the German practitioners of a few years' standing are almost ignorant of it; for they soon find that what they had bestowed years to gain, is all but useless at the bed-side, and they let it slip as a useless commodity, for which they have paid too dear. Indeed, the average knowledge of anatomy is greater among English than German practitioners. Professor Müller remarked to us, when he was in England last year, that nothing surprised him more than the scientific knowledge of the practising surgeons and physicians whom he met with here: he observed the marked difference between the condition of medical men in the two countries, and said very truly, that in Germany every one was either a practitioner or a scientific anatomist, but that in England many were both. He was alluding to the pupils of Hunter, and Cline, and Abernethy, and Cooper, and Brodie—men who were at once hospital surgeons and anatomical lecturers, and who in themselves and in their pupils shewed that science and practice might, and ought, to be taught and learned together; while in his own country he found that, when taught separately, one

at least would suffer, because men will rarely care to occupy their time and thoughts in matters which are not of personal utility or interest to themselves.

If it were anticipated that one-tenth of those who embark in the study of medicine would have any other source for their maintenance in this country than medical practice, there might be an excuse for teaching them the collateral sciences in such a manner as might qualify them to be, in their turn, lecturers upon them. But such can never be the case, and therefore they should all be taught in the manner which may render them most useful in practice. In anatomy, for instance, the illustrations should be drawn from the sick bed, and from the operating theatre and the dead-house; and to make way for these, let all the doctrines of *homologues*, and *heterologues*, and of *potentials*, be banished with the imaginary analogics of *ptereal* and *herisseal* bones, and the theories of *morphological* and *histological* development; for in what mortal sick-room would these find their sphere of usefulness? We do not for an instant advance that the studies of these, the higher branches of the sciences, which certainly minister to the progress of medicine, should be neglected, but only that they should not be taught to the mass, *to the neglect of that which is practical.* To the mass they are utterly useless; and those who, from leisure or inclination, would be induced to pursue them, will not need to have heard of them in the routine of the lecture-room: the sciences themselves will not suffer by the comparative neglect of teachers, for they flourish most in a monopoly, as private lands are more productive than commons. But in any case, the main end of every general scheme of education should be to make the students practitioners, and not professors; for even if it could succeed in making them the latter, it would only bring them up to poverty. The same reasons will apply to chemistry and the other sciences with the more force, as they are more distantly removed from the practice of medicine. Professors of each may work on to unravel their greatest mysteries, but to those who have to employ them in practice, nothing but their several applications, and just so much of their general principles as may be needed to render them intelligible, should be taught. More than this is loss of time alike to both parties; for how many amongst those, who, as medical students, were taught by the most learned and exclusive professors, can be found as practitioners retaining or cultivating any knowledge of that on which they spent so much time, or ever thinking of it, except for its rough application to the practice in which all their working thoughts are employed?

THE LONDON UNIVERSITY.

[We beg to direct attention to the following letter, which it is impossible not to perceive is written by a member of the Senate of the London University. We shall probably recur to the subject in an early number.—Ed. Gaz.]

To the Editor of the Medical Gazette.

Sir,

The part which you have generally taken respecting the London University has been decided; but, like all partizanship, it has occasionally been gone too far.

The general impression that the Home Secretary is about to remodel the whole establishment, is, I believe, erroneous; and unless the public and the profession are alive and stirring, the remodelling will consist of very little else than the change of a few names in the list of the senate, and a few unimportant alterations in the conditions of the charter.

Very few will deny, as far as the medical profession is concerned, that an institution is highly desirable in this great metropolis, with so perfect a school of medicine, which will have the power

of conferring degrees, with or without a license to practice, without the necessity of seeking such distinctions in some other place, where medicine is less perfectly taught, or where religious distinctions are a bar to the usual honours. When the London University was in the course of formation, between two and three years ago, there was the most absurd mystery practised; great and alarming jealousy was displayed; and consequently no fair discussion could take place, by which a well-working body could be established. Now, however, such obstacles exist to a much less extent, time has been given to shew the imperfections of the present charter, and also to allay the heat and irritation which a supposed intention to subvert the present corporate bodies in the profession, could not fail to excite. The opportunity is now gained of instituting a University, which may be productive of the most important results to the future respectability of medical men; and if it be now neglected, there will be a repetition and continuation of all the evils which the present imperfect charter has given rise to. Now is the time for the highly qualified physicians and surgeons of this kingdom to step forward, and urge on the ministry the importance of rescuing the University from its present thraldom. Instead of consulting one or two individuals, let communications be opened with the Colleges of Physicians and Surgeons, and (why not?) the Apothecaries' Company. A way may be opened, without hurting the interests of either of these bodies, to form a governing head which will regulate such a course of theoretical and practical education as will be suitable for those who are to take the lead in the profession; and therefore it must be one which goes beyond any of the present more limited plans of instruction.

In a leading article of one of your recent numbers, you find fault most severely with some supposed regulations of the present Senate of the University. I say *supposed*, because nothing is yet settled, and many of the regulations you considered are only *proposed* for discussion and future alteration if necessary. But take them as they stand, and I maintain your strictures are overstated. You talk of the time and expense required, and of the enormous preponderance of theoretical over practical instruction. The first object is to make the degree a highly respectable one; and how can that be done unless the qualification be a high one? The expense will certainly exceed that of the education of a surgeon or an apothecary, in some respects, though there will be no apprentice-fee required; and the consequence will be, that the sons of persons moving in a higher class of society will alone go through such a course of education. Surely this is rather desirable than otherwise, if we wish to keep up the estimation which physicians *in this country* have always maintained. In regard to time, four years of study is surely not excessive, and it is not so much as is required by some of the medical bodies: it is not more than is ordered at Edinburgh; and how few who have attained eminence in the profession have been satisfied with so little. Oxford and Cambridge have claimed a greater title to respectability for their degrees, because there has been a foundation of high classical and mathematical attainment before the medical studies have been entered upon. The London University has thought proper to imitate that part of the regulations of the two Universities mentioned, and consequently has arranged, that no person shall have his degree in medicine without having passed through a degree in arts, or an examination equivalent to such a degree, before beginning the four years' curriculum in medicine. This degree, or examination in arts, will include several branches of general science and literature, beyond what has usually been required at Oxford or Cambridge; but it is not more stringent than will be readily passed by young men of 17 or 18, who have been educated properly, and not merely on the old-fashioned plan of leaving school or college ignorant of every thing but Latin, Greek, and some of the simpler mathematics. Already an improved system has been begun in the public schools; and it cannot stop there.

After this preliminary degree or examination, we come to the four years' medical study. You object to the waste of time in theoretical instruction to the neglect of practical acquirement. Except in anatomy and dissections, in the whole four years, only *one* course of lectures on the usual branches of medical and surgical knowledge is made *essential*. The arrangement very properly is, that the scientific and elemen-

tary instruction should precede the practical. In the first two years the prescribed instruction is confined to anatomy with dissections, physiology, chemistry, botany, materia medica, pharmacy, comparative anatomy, pathology, and therapeutics. In the last two years practical medicine, surgery, and midwifery, are to be taught, with forensic medicine and hygiene; but the principal part of the two years is to be devoted to clinical instruction, hospital and dispensary practice, with regulations that such instruction shall be given in the most regular and close manner, and not left to chance or volunteer information, or to careless and desultory attendance.

Does not this simple statement of facts entirely contradict your assumptions, and do away with your arguments? The length of the courses, and the making of a separate course of lectures on general pathology and the theory of medicine, are also objected to by you. The latter question has engaged wiser heads than mine; and by the discussion it has provoked in Edinburgh lately, I may at least be allowed to say that there is room for a difference of opinion, without the necessity of presuming that whoever disagrees with your views is, of course, in the wrong. In respect to the length of the courses, I must openly say that I believe the objectors to the number of lectures required by the London University, are all interested parties—are themselves lecturers*, very willing to save themselves trouble, and give as few lectures as they can in return for their students' fees. But let us look to facts. Twenty years ago the surgical lectures in London consisted of about 45 or 46 in each course. They began the first week in October, and were continued, three times a week, till the middle of January; a second course being given subsequently, of the same length, and, by those who read their lectures, it was given in the same words. At that time the teachers were satisfied with such a course. Are they so at present? Do not the lectures now average double that amount? And even as it is, I have heard lecturers declare, that if they had more time, they could give more complete courses, and that one of their present courses is much more valuable than two of those given when I was a student. If you wish to see the advantage of huddling a course of lectures on the practice of physic into a contracted compass, look at those recently published in the Lancet, and see numbers of important diseases docked off, one after another, in a dozen lines. Can a student find such curtailed and meagre descriptions guides to his knowledge of diagnosis or of treatment? Perhaps you will scarcely allow such an example a fair one, and I should be insulting the general run of medical lecturers if I were to insist upon it; but take the instances of the lectures on the practice of medicine given in London till within these few years, compared to the highly attractive and valuable ones as given in Edinburgh by the late Dr. Gregory; the former consisting of about 45 lectures, the latter of about 120. How is it that experience and improved knowledge have tended to nearly double the size of the London course, and yet not to lessen the number of the Edinburgh one? I think, sir, that though the curriculum *proposed* (but not yet *settled*) of the London Uuniversity may not be the best possible, it by no means deserves the severity of your strictures. I am sure you do not join with Mr. Wakley and his crew in denouncing curricula altogether—in repudiating the recognition of those schools only which have given proofs of their possessing proper *matériel* for teaching—and in trusting all to the imperfect test of a *vivâ voce* examination, the triumph of crammed ignorance and impudence over modest merit.

I wish, sir, to lead my medical brethren to form a fairer and a more dispassionate estimation of the London University than has hitherto existed. There has been such a fear of "the craft being in danger," that the cry has only been, "Great is Diana of the Ephesians." It is this absurd fear which has led to much of the mischief in which the University has been hitherto involved; it is this which frightened many of the best men in our profession from joining it, and consequently threw it into the hands of a number who have too little weight in public estimation to confer upon it that dignity and respectibility which such an institution ought to possess. I have no wish to defend Mr. Spring Rice or Lord John Russell

* If our correspondent means that the writer of the editorial articles in this journal, disapproving of certain parts of the proposed curriculum, is himself a lecturer, he is mistaken.—E. G.

in some of the steps which they took in the formation of the senate; but in regard to the members of which it originally consisted, their hands were tied. They would have had all of them first-rate men, if they could have got them, and therefore they were obliged to put up with many of inferior quality. However Lord John Russell may have deserved censure for a recent "*untoward*" appointment, the result will be of great and important benefit. That appointment roused up a spirit of resistance, which will prevent any future mistakes of the same nature, and do away with the *uncontrolled* nomination of the Senate by the minister of the day. That appointment is now at an end, but the disturbance it created has led to the proposal of a new charter, with as much modification as may be thought desirable. Now is the time, therefore, for those who wish well to the projected institution to come forward, to suggest improvements, and to give their best assistance to the good work. If the corporate bodies are willing, the new University may be so constituted as to work hand in hand with them, without detriment to the real interests or purposes of either, and with immense advantage to the profession at large.

I have trespassed at some length on your pages; but the period is too full of important results to allow me to be silent, and I trust that my well-intended remarks will not be without their effect.—I remain, sir,

Your obedient servant,

C. L.

August 7th, 1838.

GLASGOW EYE INFIRMARY.

Clinical Lectures by Dr. Mackenzie.

Lecture I.

Fungoid Disease of the Dura Mater.

Agnes Robertson (No. 8314) was admitted on the 6th March, 1838, aged 58 years. Two months before she applied here she had been seized with pain in the upper part of the head, extending down the right side of it, and down the neck and shoulder. About eight days before her admission she had been exposed to cold, which seemed to have induced partial paralysis of the right abductor oculi, so that she could not turn the right eye completely dextra. She saw double, and was affected with vertigo. Pulse 108. She was bled in the arm, and had a purge. The blood was buffy.

Next day she could turn her eye rather better outwards, and had less pain down the right side. A blister was applied to the right side of the head.

On the 8th, her tongue was foul and dry. She was ordered two aloetic and mercurial pills every night.

On the 12th, the power of the right abductor oculi was observed to be increasing.

The blister was repeated on the 15th.

On the 20th, the pills were diminished to one daily.

On the 24th, another blister.

On the 4th April, she moved the right eye still more naturally, but complained of dimness of vision, except when she viewed objects downwards and near.

On the 29th, she complained more of pain in the right side of the head, aggravated during the night; and the right upper eye-lid now drooped considerably, from a paralytic state of its levator.

She was ordered to rub the pained part of the head with Tr. Opii.

On the 8th May her mouth was sore, and she complained of pain in the brow. The former pills were omitted, and a colocynth pill ordered *pro re nata*. A blister was applied over the seat of the pain.

On the 12th July there was considerable protrusion of the right eye-ball out of tis socket, a symptom which is termed *exophthalmos*. The palsy of the right upper eye-lid was now complete, so that she could not open the right eye; she was unable to leave her bed, and was becoming less capable of assisting herself.

By the 18th the left eye had also become closed, and the vision of this eye considerably impaired. The submaxillary gland and the lymphatic glands of the neck on the right side *appeared* to be much enlarged.

On the 25th she was less able to articulate distinctly, and frequently lost the power of memory. She died on the 1st of August.

On inspection yesterday there was found over the upper and fore part of the right anterior lobe of the cerebrum, the fungous tumor of the dura mater which you see here. It is oblong, about the size of a small walnut, smooth externally, and covered with a serous membrane, firm, red, and fibrous-like internally. It grows, as you see, from the cerebral surface of the dura mater, while the cranial surface of the membrane is quite smooth and natural. There was a depression on the surface of the brain, corresponding in situation and size to the tumor; the cerebral substance under it, however, appeared healthy.

On further raising the brain from the base of the cranium, a very extensive mass of disease was exhibited. A large irregular fungoid mass was found in connexion with the dura mater, occupying chiefly the middle and right side of the cranial base. It was of a dirty yellowish colour and varied consistence, being in some parts soft and pulpy, in others firm and almost cartilaginous. Here is the portion of dura mater to which this mass was attached. The cerebral substance in immediate contact with the fungus was completely disorganized to a considerable depth. The inferior surface of the right middle lobe, and the posterior part of the anterior, were in a state of complete *ramollissement*, and indeed could not be distinguished from the softer parts of the fungoid mass. The optic nerves appeared healthy, but were somewhat flattened, from the pressure of the tumor. The diseased mass extended from the sella turcica, or rather from a little on the left side of this, over it, outwards and downwards to the right, and filled the large fossa in which the middle lobe of the brain is lodged. At the lower and posterior part of this fossa, in the great wing of the sphenoid, absorption of the bone had taken place, and produced a roundish opening, with rough, brittle, and irregular edges, through which the little finger could be passed. Through this aperture the fungoid growth passed out of the cranium, and made its appearance externally at the side of the neck, over and behind the angle of the jaw, in the form of a moveable, hard, and nodulated tumor, about the size of a large hen's egg. This external tumor, which, during life, had been taken for a glandular affection, was not examined further than to ascertain with certainty its connexion with the disease of the dura mater internally.

Within the cranium, the morbid growth, at its more anterior part, originated distinctly from the inner surface of the dura mater, the outer or cranial surface of the latter being quite sound and adherent to the bones: but posteriorly, and for the most part, the disease involved both surfaces of the membrane, which, as you see, is in some parts much thickened, and in a great measure confounded with the fungoid mass.

There was from half an ounce to an ounce of limpid fluid in each of the lateral ventricles, and the choroid plexuses were enlarged and interspersed with numerous vesicles.

In June last I lost a patient, in my private practice, from fungous disease of the dura mater. His age, when he died, was sixty-nine.

He first consulted me about ten years ago, on account of stuffing in the nostrils, loss of sense of smell, deafness, and gradually increasing amaurosis. These symptoms occurred in the order in which I have mentioned them. The amaurotic affection ended in total blindness in 1830; and about a year after that, his eyes were observed to protrude considerably from their sockets. This symptom went on increasing, and the right temple also became dilated and elevated, by some cause residing within the orbit. For a long time he suffered severely from headache, the pain commencing after the loss of sight, and about the time when the exophthalmos was first observed. About eighteen months before death, the right eye-ball was so much protruded that it burst, and was destroyed. About three years before death, he was attacked frequently, during six or eight months, with profuse epistaxis. The swelling in the right temple was very painful to the touch, or when he had the part shaved. At last he died dropsical.

At one period of his life, this patient snuffed a great deal, but he dropped that, and had recourse to smoking and chewing. He knew no cause of his disease; he could trace it to no fall or blow on the head. Indeed, previously to the stuffing of his nostrils, which was the first symptom, he had been always healthy, except that he was occasionally troubled with tremors, or rigors, for a few days at a time. He was not liable to headache before his sense of smell began to fail. He never had any fits, faintings, or paralytic symptoms.

The pain of his head was greatly relieved by the internal use of laudanum. From the state of complete deafness and blindness in which he was for some years before death, it was difficult to know how far his memory or judgment was affected.

On inspection, the brain was found to be in no respect materially diseased. The pituitary gland was sound, and the cerebral surface of the dura mater was entire. Under the dura mater, between it and the basis of the skull, and especially behind the sella turcica, there was an extensive fungous tumor, of a dark-red colour, soft and brainy in consistence. This tumor originated from the cranial surface of the dura mater. It spread across to each temporal bone, which were in a state of caries. It dipped into the nostrils and filled both orbits, their roof and posterior parts being removed by absorption, as was also the cribriform plate of the ethmoid bone, and the outer wall of the right orbit. The tumor, where it filled the right orbit and protruded into the temple, was unlike the rest, being firm and whitish, like cartilage. This portion could not be distinctly separated from the rest of the diseased mass, but it seemed probable that

this portion was the lacrymal gland, enlarged and changed in structure. The optic nerves, between their chiasma and the orbits, were pale and flat, like ribbons.

The following remarks occur to me, on considering the two cases I have quoted to you:—1. Fungus of the dura mater attacks either surface of the membrane. It appears to affect the proper fibrous substance, and not the tunica arachnoidea.

2. The tumor, as it increases in size, exercises pressure on the brain, and induces a variety of cerebral symptoms, as palsy, amaurosis, deafness, epilepsy, coma, &c., and it exercises pressure on the bones, and causes their absorption.

3. When the upper part of the dura mater is the seat of a fungous growth, it often happens that the first symptom observed is the tumor pushing up the pericranium and integuments. A soft elastic swelling is felt on the head, which has sometimes been mistaken for an abscess. In some instances the bone, pressed on by the fungus has been found reduced to a sort of honey-comb work, of which you see here a specimen; in the other cases, there is a single large aperture through the bone. When the basis of the skull is the seat of the disease, some affection of the senses is generally the earliest symptom; some affection of smell, hearing, or sight. Then the eye may protrude, and the growth may project by the nasal side of the eye, or in the temple.

4. Such tumors, when they project beneath the integuments, are said to be affected with a distinct pulsation synchronous with the pulse, and a heaving and sinking synchronous with respiration; but I have not observed this myself. I mention it as a thing to be noticed in any other cases which may come before you.

5. When there is only one large opening through the skull, by which the tumor protrudes, the edge of the opening may be felt sharp and circumscribed; not so in a case like this, where the bone is pressed outwards and reduced to a honey-comb state.

6. A similar remark holds as to the effect of pressure on the tumor, which will sink under the finger in the one case, but feel firm and resisting in the other. It may be difficult, therefore, to discriminate between this disease and osteo-sarcoma of the bones of the skull.

Exopthalmos.

You will not unfrequently meet with cases of protrusion of the eye-ball from the orbit. The causes are very various, and the diagnosis is often difficult, or actually impossible. The orbit must be carefully examined; the nostrils, the antra, the mouth, the temples, the forehead. The protrusion may arise from the state of the soft parts within the orbit, or even from ocular phlegmon, as it is termed by some—a severe inflammation originating in the retina, ending in protrusion and disorganization of the eye-ball, and not uncommonly proving fatal. On the other hand, the cause may be, as in the cases we have been reviewing, in the dura mater; it may be a fungus, perhaps a fungus hæmatodes of the dura mater.

Ophthalmoptosis.

One very curious variety of protrusion of the eye-ball is what is styled *ophthalmoptosis*. I have never seen but one case of this kind, and that is the case of Robert Dunn (No. 8123.) This man is still attending, and you will have an opportunity of seeing him. He is a cooper by trade, and was admitted for catarrho-rheumatic ophthalmia, chiefly affecting the right eye. After he had attended for a few days, we discovered that he required only to stoop forwards for a few minutes, when his eye fell, as it were, out of its socket. On raising his head, and rubbing the eye a little, it resumed its natural place. He complained of considerable pain in the orbit, which was relieved by V. S. and the use of mercurial purges. He stated that the protrusion of the eye commenced about five years ago, after carrying a heavy load upon his back.

PUTRESCENT EXHALATIONS.

To the Editor of the Medical Gazette.

Sir,

The Report of the Poor-law Commissioners on the sources of fever in London, extracts from which have been extensively circulated in the newspapers, contains certain statements, on the authority of Dr. S. Smith, of a very positive nature, as to the possibility of demonstrating by direct experiment the presence of vegetable and animal matter in a high state of putrescency in the air collected in certain (malarious) situations.

To use the precise words employed:—"If a quantity of air in which such exhalations are present be collected, the vapour may be condensed by cold and other agents; a residuum is obtained, which, on examination, is found to be composed of vegetable or animal matter, in a state of high putrefaction. This matter constitutes a deadly poison. A minute quantity of this poison, applied to an animal previously in sound health, destroys life, with the most intense symptoms of malignant

fever. If, for example, ten or twelve drops of a fluid containing this highly putrid matter be injected into the jugular vein of a dog, the animal is seized with acute fever; the action of the heart is inordinately excited; the respiration becomes accelerated; the heat increased; the prostration of strength extreme; the muscular power so exhausted that the animal lies on the ground wholly unable to stir, and, after a short time, is actually seized with the black vomit, identical in the nature of the matter evacuated with that which is thrown up by a person labouring under yellow fever. By varying the intensity and the dose of this poison, it is possible to produce fever of almost any type, endowed with almost any degree of mortal power.

"It is proved further, that when this poison is diffused in the atmosphere, and transported to the lungs, it enters directly into the blood, and produces various diseases, the nature of which is materially modified, according as the vegetable or animal matter predominates in the poison. In the exhalations from marshes, bogs, and other uncultivated and undrained places, vegetable matter predominates; such exhalations containing a poison which produces, principally, intermittent fever or ague, and remittent fever."

You will not fail to observe, that the above quotations contain not mere hypothetical assertions of possibilities or probabilities (in which case I should not have trespassed on your attention); but that they profess to be narratives of facts ascertained by direct experiment, and inferences deduced from such.

Without entering at present on the question, how far those statements are consistent in themselves, or reconcileable with certain striking and long-established facts in disproof of the injurious effects to the animal economy of putrescent animal matter, I should feel much gratified if Dr. Smith, or any other party, would, through your columns, indicate by whom such experiments as he refers to and reasons on have been performed; and where is any record of them to be met with.—I am, sir,

Your very obedient servant,
A Constant Reader of the Medical Gazette.

August 6th, 1838.

CHARGE OF REMOVING A BODY FOR DISSECTION.

Last week a charge was preferred by a woman against Mr. Perowne, veterinary surgeon, of taking away the body of her husband. He persisted in having purchased it during the life of the deceased, and contended that it was his property, and he cut it up for the purposes of science; the magistrates reminded him, however, that he was not in any practice that required a knowledge of the anatomy of the human frame, and that his premises were not licensed for anatomical studies. Mr. P. called a witness, who said he did not know any thing about the deceased selling himself, but he heard the woman give the defendant leave to take away the body. Mr. P. stated that he was a member of St. Bartholomew's Hospital. He was ultimately bound over to answer the charge at the assizes, himself in £80, and two sureties of £40 each.—*Suffolk Chronicle.*

UNIVERSITY OF EDINBURGH.

At the close of the sessions of the University of Edinburgh, on Wednesday, the 1st instant, 98 gentlemen received the degree of doctor in medicine.

ANIMAL MAGNETISM.

Sir Philip Crampton, the Surgeon-General for Ireland, denies that he has, as alleged, become a convert to the doctrines of animal magnetism.—*Times.*

WEEKLY ACCOUNT of BURIALS.

From Bills of Mortality, *Aug.* 7, 1838.

Abscess	2	Hæmorrhage	1
Age and Debility	32	Hooping Cough	2
Apoplexy	6	Inflammation	12
Asthma	1	Bowels & Stomach	6
Cancer	1	Lungs and Pleura	4
Consumption	31	Insanity	1
Convulsions	20	Measles	3
Croup	1	Paralysis	1
Dentition or Teething	4	Small-pox	18
Dropsy	3	Sore Throat and Quinsey	1
Dropsy in the Brain	5	Thrush	1
Dropsy in the Chest	1	Unknown Causes	71
Fever	7		
Fever, Scarlet	1	Casualties	2
Fever, Typhus	2		

Increase of Burials, as compared with the preceding week . . . 50

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

August.	Thermometer.	Barometer.
Thursday .23	from 57 to 67	29·80 to 29 68
Friday . . 4	57 68	29·66 29·72
Saturday . 5	60 68	29·64 29·59
Sunday . .	53 68	29·55 29·51
Monday . . 6	56 69	29·48 29·47
Tuesday . . 7	52 67	29·58 29·71
Wednesday 8	52 67	29 91 30 07

Wind, S.W.

Generally clear, except the 2nd, 4th, and 6th, when rain fell.

Rain fallen, ·525 of an inch.

Charles Henry Adams.

Wilson & Son, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 18, 1838.

LECTURES ON BLOOD-LETTING;

Delivered from time to time,
At the General Dispensary, Aldersgate Street.

By Henry Clutterbuck, M.D.

Formerly one of the Physicians to that Institution.

Lecture XI.

Of the Use of Blood-letting in Diseases of the Organs of Nutrition.

This set of organs consists of the whole alimentary tube, beginning at the mouth and terminating at the anus. It comprises, consequently, the parts contained within the mouth, the fauces, gullet, stomach, and intestinal canal. There are, besides, various accessory organs—namely, the liver, spleen, and pancreas, with their investing membrane, the peritoneum; which last also furnishes an exterior covering to the stomach and intestines, as well as lines the general cavity of the abdomen, in which the parts just mentioned are inclosed. Those different parts are all liable to inflammation, and are all more or less capable of being benefited by blood-letting; but in different degrees, and with some peculiarities that require notice.

I may first observe, that the alimentary canal, like the air-passages, is lined throughout by a continuous mucous membrane, which, in its diseases, follows a course very similar to catarrh, requiring much the same general treatment. The inflammation takes different names, according to the particular part of the canal in which it is seated. Thus in the mouth it is termed the thrush (*aphthæ*)—in the throat or fauces, quinsy (*cynanche* or *angina*)—in the œsophagus and stomach, heartburn or pyrosis—in the small intestines, diarrhœa or flux, on account of the watery stools that attend it—and in the large intestines, dysentery. In all these, the disease is of the same intrinsic nature, consisting in inflammation of the mucous membrane; yet there are shades of difference between them; and as the difference extends to the treatment, they require to be separately noticed.

Beginning then with the mouth, where the disease is termed aphthæ or thrush, I may observe that this is seldom a primary affection, but rather consequent upon some other disease, which then is the chief object of attention. Thus it appears as a symptom of many fevers, and those commonly of a dangerous nature; and at a stage of the fever, also, when blood-letting, even if it should have been proper at the onset (which may or may not have been the case), is hardly to be thought of. The thrush frequently makes its appearance in the last stage of hectic fever, when evacuations of all kinds are unadvisable. In short, generally speaking, aphthæ or thrush is not a disease to which blood-letting is adapted. When it arises in infants from the use of improper food, attention to diet is almost the only thing necessary.

When inflammation attacks the mucous membrane of the fauces, the case becomes somewhat complicated, by the extension of the disease to the tonsil glands, which often swell so considerably as to impede deglutition, and even respiration at times. The muscles of the pharynx, likewise, becoming involved in the inflammation, much pain is felt when these muscles are put into action.

This disease occurs equally in the strong and in the weak. In the former it is prone to terminate in abscess of the tonsils, and that usually within three or four days. The same thing may occur in persons of weak and irritable habits; but in such, instead of deep-seated abscess, the inflammation is more disposed to end in superficial ulceration of the affected surfaces. In by far the greater number of cases of inflamed fauces, (cynanche or an-

gina), blood-letting is not called for; and only, in fact, where the habit is strong and the disease quite recent, that is, within little more than twenty-four hours from the attack. After this period the probability is that the inflammation will proceed to suppuration, which blood-letting, if carried to any great extent, would only retard, but not prevent; hence the sufferings of the patient are likely to be prolonged, while the general health will suffer needlessly afterwards. Should any signs of the inflammation spreading to the larynx present themselves very early in the disease, or should the swelling of the tonsils and neighbouring parts appear to press on the glottis, so as to impede respiration, it becomes the more important to have immediate recourse to bleeding. But, as I mentioned before, if not done very early, it might be rather hurtful than beneficial, by retarding the suppuratory process, which, when bleeding fails, is the only effectual relief to be looked to.

In weak and irritable subjects, and where superficial ulceration, rather than phlegmonous inflammation, takes place, loss of blood can hardly be proper. In such cases, mild tonic remedies, such as the cinchona, are more useful. There is a specific inflammation of these parts, accompanied very generally with an efflorescence on the skin, the scarlet fever, over which art appears to have but little influence in the way of cure. This disease is frequently epidemic, and, in some seasons, is extremely fatal—in others, quite the contrary. There is always more or less of cerebral disorder combined with the inflammation on the skin and fauces. Some have asserted that blood-letting is of great advantage in this disease; but general experience seems to be against this practice. The general conclusion I have myself arrived at, from observation, is, that the disease, being specific, is, strictly speaking, not curable by art; and that a passive treatment is, upon the whole, the most advantageous—that is, keeping the skin cool, and exhibiting only simple saline remedies; or, at most, a few drops of the tincture of digitalis, on account of the great frequency of the pulse; thus giving time for the disease to pass quietly through its different stages. If the pulse is weak and soft, slight tonics, as the cinchona with a dilute mixture of wine and water, appear to be useful.

Inflammation of the mucous membrane of the œsophagus and stomach (*cardialgia*), is, for the most part, the consequence of the use of too stimulant food or drinks, and requires in general nothing beyond abstinence and mild aperients for its relief. Should the symptoms be unusually severe, and the habit strong, blood-letting might, no doubt, prove useful. Inflammation of the general substance of the stomach (*gastritis*), when acute, is so full of danger as to warrant a liberal use of blood-letting; but, of course, not without proper attention to the various circumstances of the case. The same may be said of intestinal inflammation (*enteritis*). Bleeding is generally indispensable in this case, and requires to be carried to a considerable extent.

With respect to inflammation of the mucous membrane lining the intestinal canal altogether, you may revert to what was stated in regard to inflammation of the mucous membrane in general. It is a disease which may be said generally to cure itself, by the increased secretion that takes place from the inflamed membrane at a certain stage. Should, however, the disease prove unusually violent, and be attended with pain of a continued kind, with febrile symptoms, bleeding is of the most unequivocal advantage. This is intelligible enough, provided you discard from your minds the long-prevailing notion that diarrhœa is a state of weakness—a relaxation of the bowels, as the common expression is. Instead of this, all the vital properties of the part affected in diarrhœa are in a state of preternatural excitement. The sensibility is heightened—the vascular action, and also the muscular, are increased—in short, diarrhœa is a state the very reverse of weakness, as far as the part itself is concerned; it is a state of violent action, though, from interfering with and disturbing the nutritive process, it is quickly productive of general weakness of system. And that the disease (diarrhœa) consists in inflammation is clearly deducible from the phenomena of the disease, as well as the consequences to which it ultimately leads—namely, ulceration and other changes of structure. It requires, therefore, a treatment adapted to inflammation, but modified, of course, by the peculiar circumstances of the case. As the natural cure, or rather termination, of the inflammation is by increased secretion, this ought not to be hastily checked, either by opiates or astringents, but rather left to itself for some hours; after which the remedies just mentioned may be properly resorted to.

The term dysentery (literally, disordered intestines) is applied to this disease when the inflammation is confined chiefly to the large intestines, the colon and rectum. Bleeding is still more necessary here than in the case of diarrhœa, the inflammation being of a more active kind, with a greater tendency to disorganization. The chief peculiarity in the treatment of dysentery, as compared with diarrhœa, is, the greater necessity for the employment of

purgatives from time to time, on account of the tendency there is to the lodgment of hardened fæces in this part of the canal.

The organs accessory to the process of nutrition, and which are seated in the abdomen, are the liver, spleen, and pancreas. These parts are all liable to inflammation in different degrees, and may require blood-letting for their relief, according to circumstances that I have repeatedly pointed out. Indeed, I should not think it necessary to dwell longer on the subject, were it not that an extraordinary degree of importance has been attached of late years to the liver and its diseases, as if this organ were the parent of a great number of other affections that used not to be attributed to this cause.

Acute inflammation in the liver, as well as in the spleen and pancreas, is in general readily detected by the seat of pain, and the other ordinary signs of inflammation, accompanied as they generally are by a disordered state of the stomach and other digestive organs. The treatment is also very simple, and should be more or less active according to the state of the patient at the time.

Slow or chronic inflammation in the liver is much more frequent than the acute, and at the same time more obscure in its symptoms, so as to be readily overlooked; as, indeed, it often is. The pain in the inflamed organ is often trifling, and only to be detected, perhaps, by strong pressure over the part. The disordered condition of the stomach, want of appetite, imperfect digestion, nausea, &c. are the symptoms which exclusively attract the patient's notice. He resorts, therefore, to stimulants of various kinds, and generally with temporary advantage; but no lasting benefit is thus procured. On the contrary, the real disease, the inflammation, is often rendered worse by such means; whereas a simple antiphlogistic treatment, such as a small bleeding or two, with the use of mild cathartics, aided by a rather abstemious diet, in general (provided disorganization has not taken place) soon puts an end to the symptoms, by removing their cause.

My attention was strongly and particularly attracted to this subject a few years back, by the case of a much-esteemed friend, a distinguished physician in this town, who had been suffering severely for several weeks from a variety of dyspeptic symptoms—namely, want of appetite, dislike for food, occasional nausea, and inactivity of the intestines, with clay-coloured evacuations; but all without positive pain. There was also a great feeling of langnor and depression of spirits. It was much the fashion at that time to talk of a "torpid liver," and "want of bile," in order to account for such symptoms; and to these were the complaints of the patient attributed by his medical friends and advisers. Accordingly, the supposed torpidity of the liver was to be roused by the *blue pill*, which was carried the length of making the mouth sore. And in order, in the meantime, to enable the stomach to do its office, gastric stimulants, such as bitters and spices, were prescribed, together with a moderate portion of wine. The wished-for relief not having followed this mode of treatment, I had occasion to see the patient. Instructed by previous observation of several similar cases, and reflecting attentively upon the nature of the symptoms present, I thought there was ground for suspecting the existence of slow inflammation in the liver, and that this was the probable cause of the symptoms under which the patient was labouring; in fact, that the effect had been mistaken for the cause. It is true there was a want of proper action in the stomach and intestines; while the clay colour of the evacuations shewed the absence of bile in that canal. Upon careful inquiry, however, into the feelings of the patient, as well as other circumstances, I came to the conclusion I have mentioned. The pulse at the wrist, as generally happens in these cases, was rather feeble, and not much accelerated; but a sensation of heat was experienced in the region of the liver, and considerable uneasiness felt when strong pressure was made on that part. These circumstances alone furnished no weak reason for suspecting the existence of inflammation; and all doubt of this was removed from my mind, when I observed the tongue to be thickly coated with a yellowish crust; such a state of the tongue being, according to my observation, one of the least equivocal signs of hepatic inflammation. This view of the case being admitted, it was not difficult to explain the leading characters of the disease. The inflamed state of the liver would naturally be attended with a suspension or disorder of the functions of the stomach, so as to give rise to the dyspeptic symptoms; while the hardness and swelling that belong to inflammation would (supposing the disease to be seated in the vicinity of the biliary ducts) necessarily compress these passages, thereby impeding the transit of the bile into the intestines; thus accounting for the colourless state of the evacuations, and the sallow hue of the skin. Nor was it difficult to understand why the treatment by tonics and stimulants should have failed; while there was good ground for hoping that the opposite — namely, an antiphlogistic practice,

would be more successful. Accordingly, this was resorted to, and in the simplest way. A moderate blood-letting was advised, to be followed by the use of the mildest purgatives. The blood drawn exhibited, as I had anticipated, unequivocal marks of long-continued inflammation of the liver. The crassamentum was thickly buffed and contracted, and the serum was of an intensely yellow hue, as was the urine, which, as well as the skin, thus betrayed the presence of an unusual quantity of bile in the system. The relief experienced was immediate; and by a continuance of the same simple plan, the symptoms altogether were in no long time removed. On several occasions subsequently the same train of symptoms have appeared, and have always readily yielded to the same simple mode of cure.

This is one of numerous instances of the same description which I could adduce as particularly connected with the present subject. I may here allude to jaundice, for the purpose of observing, that, on numerous occasions, you will find blood-letting a very effectual remedy for this disease, or rather symptom of disease, (for it can scarcely be considered otherwise.)

Jaundice, which is owing to the absorption of bile into the blood-vessels, and its deposition afterwards on the surfaces and in the interstices of the body, proves two things; first, that the liver is still capable of performing its office of secreting bile; and next, that the bile so secreted is prevented, by some obstructing cause, from passing through the gall-ducts into the duodenum. Now, one cause of the obstruction in these cases is, the presence of a biliary calculus, either in the common duct, or in the hepatic duct, which not only interrupts mechanically the passage of the bile, but also excites spasmodic pain of the most acute kind, and which recurs with more or less frequency, though at no certain periods. Another, and still more common cause of obstruction, is inflammation of the liver itself, or some neighbouring part, and which is so situated as, by the swelling that attends it, to press upon the biliary ducts, and produce an impediment to the passage of the bile; thus occasioning jaundice in the manner mentioned. When the inflammation is violent, it can hardly be either overlooked or mistreated. But when it is of a slow or chronic kind, it is often productive of so little pain as not at all to engage the patient's attention, which is directed solely to the stomach derangement, such as want of appetite, sickness, flatulency, and the like. These are commonly attempted to be relieved by stimulants of various kinds. This treatment gives momentary relief; but, as you will readily conceive, must rather tend to aggravate the real disease, the inflammation, that is the primary source of the mischief. In all cases of jaundice, therefore, you should inquire narrowly into the cause of obstruction, in order that if it be inflammation (which is really the case in a majority of instances,) a sufficiently active antiphlogistic treatment be pursued. The existence of inflammation, however indolent and inactive, may very generally be ascertained by careful inquiry. Thus, pain or tenderness felt when pressure is made in the epigastric region, or under the ribs on the right side, is alone an adequate sign of existing inflammation; and if to this be added a dry and coated tongue, there will really be nothing to doubt of. In the indolent form of the disease here described, the general vascular system is seldom much excited. On the contrary, the pulse is often low, and the feelings of the patient depressed; which is one reason, indeed, why the inflammation is apt to be overlooked, and the proper treatment mistaken. Now this is a case in which blood-letting is often of the greatest service; and, provided the disease be recent, so as to render it probable that the organization of the part has not materially suffered, it rarely fails to yield speedily to this remedy, conjoined, of course, with other antiphlogistic means, and a sufficiently abstemious regimen. You will readily understand, that only a moderate abstraction of blood is required in these indolent cases of inflammation.

For a good many years past—not so many, however, but that I well recollect the introduction of the practice—mercury has been looked up to as a sort of *specific* in these and other chronic affections of the liver; to the exclusion, not only of blood-letting, but of almost all other means. Patients, in consequence, have been doomed, almost without discrimination, to undergo torturing salivations, with a numerous train of other evils. This practice originated among the European faculty in the East Indies, where diseases of the liver are rife, and, as it were, endemic; and it was thence transferred to Europe towards the end of the last century. But, however essential such practice may be in hot climates (of which I can say nothing from my own experience, though I have reason to believe that it is now far less resorted to, even in those climates, than was formerly the case,) I am quite satisfied, from frequently repeated observations, that it is not entitled to such consideration in this quarter of the world; on the contrary, that it may, in most instances, be safely and effectually superseded by the most simple

antiphlogistic treatment, and that the carrying it to the extent that has been, and still is often done, is productive of much greater evil than any good that it is capable of, will compensate.

In regard to the other parts contained within the cavity of the abdomen, and also those of the pelvis, I have nothing of moment to observe. Inflammation in these parts is accompanied by the usual signs, and blood-letting is applicable to them under the same circumstances, and to the extent those circumstances indicate, just as other inflammations. I may safely remark, however, with respect to some of these organs—the uterus, for example, and the peritoneum altogether, that when affected by inflammation, the pulse is always small and frequent. This state of pulse, therefore, is not of itself a bar to the use of blood-letting, when justified by other signs.

Of the use of blood-letting in inflammation of ligamentous structures.—The fibrous, and, for the most part, inelastic textures, which come under this denomination, are found externally throughout the body. The ligaments of joints, the fasciæ or tendinous aponeuroses covering muscles, and the tendons by which these are attached to the parts to be moved, all come under this designation: the periosteum, too, has generally been considered in the same light. All these are very liable to inflammation, both acute and chronic; and rheumatism is the general term applied to inflammation of this particular structure.

Rheumatism, a rather unmeaning term, may be defined "an inflammation of ligamentous structure," and is properly applicable, therefore, wherever such a structure is found in an inflamed state. We are accustomed, however, to limit the term principally to affections of the joints, and the fasciæ or tendinous coverings of the muscles in connexion with them. Now you have here a striking example of the influence of structure, in modifying both the character and consequences of inflammation; and also the effects of remedies, more especially blood-letting, which is found to be far less efficacious in this variety of inflammation than in most others, although the circumstances which are usually considered as demanding the use of this remedy are more strongly marked than in perhaps any other. In no disease is the pain greater, or the febrile symptoms more violent, or the pulse more full and strong, as in many cases of acute rheumatism, or rheumatic fever, as it is termed. But notwithstanding these, and even although the patient be in high health and vigour, and in the prime of life, blood-letting, in a great majority of instances, seems to do little more than palliate; and if carried very far, it appears to have a tendency rather to prolong the disease, by interrupting or retarding that natural course and termination which are common to this with most other inflammations.

The continuance of the inflammation itself seems to wear out, and gradually exhaust, the disposition to the disease in the part; whereas, if put a stop to prematurely, whether by blood-letting, the application of cold, the colchicum, or any other means, the disposition is yet to remain, and the inflammation in consequence likely to be renewed; so that, upon the whole, the duration of the disease is prolonged by such treatment. Large bleeding, also, appears to favour the disposition to *metastasis*, which is one of the greatest dangers attending this disease. But although this is the general character of blood-letting, considered as a remedy for acute rheumatism, it now and then happens that the disease is quickly and effectually removed by an early bleeding or two to a moderate extent; so that in favourable circumstances it is worth while to make the attempt, as, should it fail to cure, it may contribute to mitigate the future violence of the symptoms.

A remarkable contrariety of opinion has prevailed among practitioners with respect to the use of blood-letting in this disease (acute rheumatism). We find Dr. Cullen recommending this remedy, with great freedom, in such cases; whereas Dr. Wells gives it as his opinion, that simple rheumatism, even in its most acute form, does not require bleeding, but is most successfully treated by the cinchona in large doses, a practice that was first introduced by the late Dr. George Fordyce, physician to St. Thomas's Hospital, and which, I believe, is not now wholly obsolete in that establishment *.

The late Dr. Fowler, of York, who took much pains in investigating the effects of different remedies in the treatment of rheumatism, gives us a history of 41 cases of this disease in the acute form, that occurred in the Stafford Infirmary: of these, it appears, three were cured chiefly by blood-letting; seven were much relieved by it; seven only moderately so; and twenty but little relieved; while four appeared to receive no benefit from it. And it deserves notice, that in 37 cases of chronic rheumatism, treated by the same remedy, the result was very nearly the

* See Transactions of a Society for promoting Medical and Surgical Knowledge, vol. iii. p. 373. Dr. W. goes so far as to say, that "it has been found in London, that bleeding is never required for the cure of acute rheumatism of the external parts, and that it sometimes proves highly injurious."—*Ibid.* 409.

same *. Perhaps the conclusion we ought to come to, amid this diversity of testimony, is, that the cures, after all, are more attributable to nature, as we term it, than to the remedies employed. And this, I am inclined to think, would be a just conclusion.

You have probably heard and read much of rheumatism of the heart; a subject that is worthy of your attention, inasmuch as the use of this language has, I think, sometimes led to erroneous practice. This expression was first used, I believe, by the late Dr. David Pitcairn, one of the physicians to St. Bartholomew's Hospital, and applied by him to inflammation of the heart, when it happened to arise during the course of acute rheumatism. Dr. Baillie, subsequently, in the 2nd edition of his Morbid Anatomy, 1797, took a similar view of the subject. From that time to the present, the same opinion has been pretty generally entertained. As a mere matter of speculation this would be of little moment; but a practical inference has been drawn from it that is of no small importance, namely, that the affection of the heart is itself rheumatic, and therefore requires a corresponding mode of treatment. This opinion appears open to objection; and, if acted upon, to be not without danger. One can see no sufficient analogy of structure here, to warrant the adoption of such an opinion. It seems more reasonable to attribute the inflammation of the heart, as well as that of the lungs, that so often appears in the course of rheumatic fever, to the general disposition to inflammation that prevails throughout the system in this disease. It is not uncommon to hear disorders of the head and chest called rheumatic, merely because symptoms of rheumatism appeared in other parts about the same time, or even long previously. Indeed, it is admitted by Dr. Wells himself that copious bleeding in the beginning is the proper remedy when the heart becomes affected in acute rheumatism. And I can say the same, from experience, when the lungs exhibit signs of inflammation in this disease.

Of the Use of Blood-letting in Gout.

This is a variety of ligamentous inflammation which one would be almost tempted to call specific, on account of the many peculiarities by which it is distinguished. Gout resembles rheumatism, in having its seat in ligamentous structures; yet, unlike this, it is confined, for the most part, to certain joints; as those of the feet first, especially the joint of the great toe, though spreading often to the instep and ankle, and, upon a repetition of the attacks, to the hands; and, occasionally, other joints. It commonly returns at somewhat regular periods, as yearly, or oftener; and when it has frequently recurred the intervals are shortened, and the duration of the fits protracted. But in all this there is much diversity. When the gout has become habitual, and the general health is much broken by its continuance, it is liable to metastasis; violent spasmodic pain taking place in the stomach; at other times, acute headache, &c.; and which are often termed gout in the stomach or head.

The gout rarely makes its appearance before the age of puberty, and, indeed, generally speaking, much later than this; but it seldom appears, for the first time, at an advanced period of life. It rarely attacks females in comparison with males. The tendency to it is often hereditary. It is brought on, in numerous instances, by luxurious living, and is therefore comparatively rare in the labouring classes of society. It would seem to have some connexion with wine-drinking, rather than the use of spirits; for the labouring classes are more addicted to the latter, yet are seldom the subjects of gout. This has led to the further notion that gout is founded in acidity; and accordingly alkalies have been recommended for its cure; but as far as I have seen this practice is of little avail. Earthy deposits (chalk-stones, as they are called) frequently take place about the joints that have suffered repeated attacks of gouty inflammation. These concretions consist of phosphate of lime, and not of chalk, as their appearance might lead one to suppose.

Gout consists in inflammation; but whether this is really of a peculiar or specific nature, is undetermined. The inflammation, however, is so violent in many cases, especially when it takes place in habits of tolerable vigour, and the febrile symptoms run so high in many cases, with great strength and fulness of pulse, that you will be strongly tempted to have recourse to blood-letting, as promising, under such circumstances, to subdue the inflammation. After many trials, however, of this remedy in such cases, my expectations have been disappointed; and I have been compelled, reluctantly I own, to fall back on the passive mode of treatment, which the experience of the most judicious practitioners in all ages has confirmed as the most safe. I consider blood-letting, therefore, to be merely useful as a palliative in gout; and only necessary or proper where the febrile symptoms happen to be unusually severe.

There is a variety of ligamentous inflammation intermediate, as it were,

* Medical Reports on Blood-letting, Sudorifics, and Blistering, in Acute and Chronic Rheumatism. By Thos. Fowler, M.D. 1795.

between gout and rheumatism, and which is commonly designated by the term rheumatic-gout. It affects chiefly the smaller joints, especially those of the fingers. The joints swell, and become gradually rigid and contracted, so as to render the hands in a great measure useless; and there is also much pain experienced. Unlike gout, however, it does not seem to arise out of luxurious living; while it appears to be even more frequent in women than in men. I know of nothing that is deserving of much confidence, as a means of relief, in this distressing affection; for my trials of all the remedies that have been recommended have generally ended in disappointment. I have only to add, that blood-letting appears to be as ineffectual as all the rest.

Our next object will be to consider the use of blood-letting in diseases of the nervous system.

OBSERVATIONS ON NÆVI, THEIR STRUCTURE AND TREATMENT.

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Amongst the many morbid productions the organization and characters of which have not yet been satisfactorily demonstrated, must be classed the congenital deformity or alteration in structure called Nævus Maternus. By some pathologists it is described as being composed of a number of cells into which blood is poured from arteries, and taken up again by veins, whilst the more general opinion appears to be, that it consists of an inextricable congeries of arteries and veins, which freely communicate with each other in every direction. The peculiarities of this abnormal growth did not escape the active mind of Dupuytren, who described both congenital nævi, and the pulsating tumors called by Mr. John Bell *aneurisms by anastamosis*, under the common denomination of *accidental erectile tissues*, believing that they are very analogous in structure to the peculiar vascular texture developed in various parts of the body, which, under certain circumstances, admits of a considerable increase in size from the sudden influx of blood. They are undoubtedly alike in several respects, but as Dupuytren only instituted the comparison, without giving us any clearer notions of the intimate structure of the erectile tissues, he can scarcely be said to have added to our knowledge on the subject *. The distinguished German physiologist, Müller, has recently announced the discovery of two sets of arteries in the penis, differing from one another in their size, their mode of termination, and their use, though arising from a common trunk; one, termed *rami nutritii*, being distributed upon the walls of the veins and throughout the spongy substance, and not differing from the nutrient arteries in other parts; the other, called *arteriæ helicinæ*, having a peculiar arrangement, and exclusively destined to supply the blood necessary in erection. It still remains open for inquiry whether there is any special provision for erection, similar to that observed in the penis, in other organs strictly erectile. That such exists is highly probable. But there is certainly no apparent necessity for any such arrangement in those parts, such as the placenta, comb of the cock, and nævi materni, which are only distended with blood in common with other vascular organs during any increase in the heart's action, or general excitement of the circulation.

In the year 1836 I met with the following remarkable case, upon which I purpose to found the observations that I have to make relating to the pathology of nævi, since it is well calculated to illustrate their structure and nature:—

Case I.—*Nævus on the Back, and Vascular Tumors in the Chest and connected with the Kidney and Ovary.*

Sarah Wilcox, a stout woman, aged 25, was brought into the physician's wards of the London Hospital, August 20th, 1836, in a state of extreme pros-

* I must protest against this general application of the term *erectile* to all these vascular growths, normal as well as morbid, which has been admitted by many British as well as Continental writers. This term can only be rightly applied to those parts so constructed as to allow of a remarkable increase in size, and alteration in position and shape, by a sudden determination of blood. To include under this denomination structures which in common with all the softer organized tissues of the body are subject to more or less distension in the varying states of the circulation, is manifestly an incorrect use of the term, calculated to convey an erroneous impression. A nævus is no more entitled to be called an *erectile* tissue than the mucous membrane of the stomach, to which we know the blood is sent in increased quantity during the process of digestion, or than those glands, such as the salivary and mammary, the circulation through which is more active during the performance of their appropriate but periodical functions.

tration, and suffering greatly from difficulty of breathing, but perfectly sensible. She was immediately placed on a warm bed, and cordials were administered. In the course of an hour she slightly revived, but shortly relapsed into the state of collapse, and died the same evening, about four hours after her admission.

I did not see her during life, but as the nature of the case was doubtful, I was requested to conduct an examination of the body.

Inspection 42 hours after death.—After examination of the important organs of the chest and abdomen, which were sound in structure, but congested, I turned the left lung over to the right side, in order to trace the course of the aorta. When the interior of this side of the chest was exposed, I was much struck at observing a large tumor on the side of the spine, extending from the second rib to the eighth, filling up the concavity of the ribs, and projecting into the chest. On making a section of the tumor in its whole length, it presented a remarkable reticular or spongy texture, not unlike that of the placenta or of the spleen. Before removing the parts, I turned the body over, to see whether there was any tumefaction behind, when I remarked in the skin of the back, at the part corresponding with the internal vascular tumor, a discolouration produced by a nævus nearly the size of the palm of the hand. On making a more particular examination, it was ascertained that the internal and external vascular structures were distinct tumors, but connected by a number of large tortuous veins ramifying between the deep-seated muscles of the back. The external did not differ in appearance or structure from ordinary nævi. There were many large veins connected with it, and all the vessels were more or less distended with blood, owing no doubt to the depending position of the part. The spongy tissue of the internal evidently resulted from a congeries of blood-vessels of various sizes, and freely communicating in every direction. Many were large enough to admit a goose's quill. I traced the course of several to their minute subdivisions, and was satisfied that the cellular appearance resulted from the tortuous arrangement and free anastamosing of vessels, and not from the existence of cells. No arteries of very large size were observed entering the substance of the tumor. On examining sections in the microscope, and with a powerful lens, no vessels could be detected which were not distinctly visible to the unassisted eye. A good deal of adipose tissue was mixed up with the vascular texture, a great part of which was filled with blood firmly coagulated. In continuing the examination of the body, I observed some large tortuous veins coming from the left kidney. They were traced back to three small tumors of different sizes, but the largest not exceeding that of a pigeon's egg. These tumors were arranged along the convex border of the gland, to which they were loosely attached by adipose tissue, and presented very much the same internal texture as the tumor in the chest. The tortuous veins being of large size, and turgid with blood, their ramifications were very distinct, and they appeared to commence from a network of vessels to which some small arteries were distributed. A number of varicose veins, and two or three smaller vascular tumors of similar character to those already described, were connected with the left ovary and fallopian tube.

On making inquiries of the friends respecting this case, it was ascertained that the mark on the back had existed from birth. Originally it was not larger than a shilling, but it had increased within the last twelve years, accompanied with considerable tumefaction of the part. During the last six months she had experienced great dyspnœa, and felt weak and exhausted upon making slight exertion, without being able to account for it*.

It is evident that the morbid vascular texture now under consideration is a deviation from the normal condition of the terminal divisions of the arteries and capillary commencement of the veins, an alteration regarded by Andral as a state of hypertrophy, in which the minute vessels are increased in size and apparently in number. A microscopical examination of the capillary circulation clearly displays the arteries terminating by direct continuity of tube in the veins, the communicating vessels being of different sizes, and their number varying

* I have not speculated on the connexion between the morbid appearances and these symptoms and the fatal issue, as it has no reference to the immediate object of this paper.

according to the vascular character of the tissue. Now in a state of preternatural enlargement, these vessels being no longer capillary would be expected to present very much the appearance exhibited by the tumors in the case related above. That such was the nature of the alteration I feel satisfied from a careful examination of their structure, and especially of the small bodies ranged along the convex border of the left kidney, which, from the large size and less intricate arrangement of the vessels entering into their composition, offered greater facilities for investigation. I am not acquainted with any other instance in which the congenital nævus has been so clearly identified with these internal vascular growths.

Mr. Fawdington, of Manchester, regards nævus as of two kinds, distinguished chiefly by the preponderance of arterial or venous tissue*. He asks, "may we not consider it as probable that the arterial nævus is formed by a dilatation of the anastamosing ramifications of the arteries exclusively, constituting a plexus of complicated and increasing intricacy, which is supplied with blood from continuous arterial branches, while the capillary extremities retain their integrity and communicate with the venous radicles in their usual and natural manner. And that on the other hand, the venous growth is constituted by a species of varix, generated on the minute and anastamosing veins, and bears the same relation to the trunks and capillaries as that which has been described in reference to the arterial nævus." I have examined nævi, both cutaneous and subcutaneous, but have never discovered any difference in the vessels which would justify a distinction into arterial and venous. The disease commonly known by the name of *aneurism by anastamosis*, appears to consist of an enlargement of the arterial trunks and extreme branches, of the nature alluded to; but I have not possessed sufficient opportunities of examining these tumors to be enabled to pass a decided opinion as to their true nature. They are rarely congenital; and the manifest enlargement of the arterial trunks, and the distressing pulsations which characterize the disease, seems essentially to distinguish it from the vascular growths considered in this paper. It has frequently struck me that in aneurism by anastamosis the size of the returning channels is by no means proportionate to that of the arteries; and if, as I am inclined to believe, the hypertrophied arteries communicate with the returning vessels without undergoing their customary subdivisions into minute branches, I can readily understand that the force of the heart's action not being expended upon the capillaries as usual, the blood would be transmitted with such velocity that the ordinary channels would prove sufficient to bring back the increased quantity of the circulating fluid which the enlarged arteries must necessarily convey to the part, without any occasion for venous plexuses and dilated veins, such as exist in nævus, and which are always signs of a languid and retarded circulation.

Although nævus consists of an enlargement of the capillary vessels, the chief arteries supplying the part do not remain altogether of their normal size. They are often somewhat enlarged, but the hypertrophy of these vessels is much less remarkable than in aneurism by anastamosis. In a subcutaneous nævus occupying nearly the whole of the middle finger of a girl aged 14, which I recently examined, the digital arteries were found to be larger than natural; and when the finger was amputated at the first joint, six arteries which bled freely were required to be secured. Before the operation the pulsation of the digital arteries was readily felt, and the pulse at the wrist, both of the radial and ulnar artery, was rather more distinct than in the other arm. The pulsation, however, was not, as in aneurism by anastamosis, perceptible throughout the whole tumor, but was confined to the sides of the finger, in the course of the digital arteries. In another case of nævus of the finger, of a similar character, I could detect no unusual pulsation of either digital or radial and ulnar arteries. In few nævi, indeed, can any thing more than a thrill be perceived; and whenever they are found to pulsate, it is generally owing to the accidental circumstance of their development in the vicinity of an artery or of arteries of some size. When nævi are wounded, the arterial blood does not escape in jets.

* North of England Medical and Surgical Journal, vol. 1, p. 56. Similar views have been taken by other writers.

Morbid growths essentially composed of a congeries of vessels are occasionally found in the parenchyma of the internal organs. I possess a section of a production of this kind found in the liver. Its texture closely resembles that of the tumor in the chest, already described; and Mr. Kiernan, to whom I am indebted for this specimen, informs me that he has on several occasions met with similar tumors in this organ*. I once discovered a tumor somewhat larger than a good sized walnut, and consisting of a plexus of blood-vessels mixed up with some dense fibrous tissue pressing upon the left crus cerebri, and covered by the pia mater, from which it derived its vessels.

It has often appeared to me, that whilst anatomists were nearly, if not quite agreed, in considering congenital nævi and accidental vascular tumors as a congeries of vessels, no clear and satisfactory views have been taken of the properties of the abnormal tissue. Being almost wholly composed of vessels, it has usually been regarded as a highly organized as well as a highly vascular structure; and these two characters being closely associated in the mind of the physiologist, no one that I am aware of has ventured to question the validity of its title to the first. That this peculiar tissue is, however, highly organized, is demonstrably an error. Conceiving that the actions of an organized structure are performed, and can only be effected by *capillary* vessels, I cannot see what claim these reticular vascular structures can have to this distinction. The blood circulating through them is as foreign to their nutrition—to their actions in health and disease, as the blood constantly passing through the aorta, or any other large artery; and the contrary, that these parts are lowly organized, is a view supported by analogical reasoning, and confirmed by pathological evidence.

That the nutrient branches of a spongy erectile tissue are different from the network of comparatively large vessels distended in erection, must have appeared obvious to every reflective mind, although it was reserved for Müller to demonstrate the fact, and describe the arrangement in the most interesting of all these structures. For it is as reasonable to assume that the vessels constituting the spongy tissue are not those by which that tissue is nourished, as that the digital artery or vein, or any other small vessel, has its *vasa vasorum*, instead of being indebted for its nourishment to the blood constantly passing through it. Believing that in this respect these morbid vascular growths are strictly analogous to the erectile tissues, I cannot regard them as more vascular than the coats of an artery or vein, structures which we know to be by no means high in the scale of organization. The appearance presented, therefore, by this tissue, is deceptive, and it may be truly likened to those textures in allusion to which Mr. Hunter remarks, "Many parts appear to be much more vascular than they really are, from their vessels dividing and anastamosing, and taking a winding course before they terminate; for it is by the number of terminations of an artery in a given space, that a part is made vascular or not vascular*."

The varieties in the appearance of nævi, some of which exhibit a bright scarlet hue, and others a purple or livid, have presented to the minds of several a difficulty in comprehending their nature†. Differences, however, of this kind are observable in many parts of the capillary system, and are certainly not peculiar to nævi. A familiar example is afforded by the skin covering the end of the nose and lower palpebræ, which in persons of a debilitated constitution, especially in cold weather, often presents a livid appearance, but which, in an improved state of health, or when the circulation is excited, assumes a more natural colour. It is owing to a state of partial congestion upon a feeble circulation.

In case 2, detailed at page 795, the nævus exhibited, when I first saw it, a bright scarlet colour; but afterwards, when the powers had been greatly reduced by profuse suppuration, this was changed to a leaden or purple hue, but

* Dr. Willis informs me that, when dissecting in Paris, he once met with a vascular growth of this kind, which he was able to inflate with his blow-pipe.

* Mr. Lawrence asks, after describing a case of subcutaneous nævus, made up of numerous blood-vessels of large size, "What is the nature of such vessels? The colour of the part during life, the size of the tubes, and the appearance of their coats, lead us to suppose that they are veins. How then does it happen that the profuse bleeding, which takes place when the tumor is cut, is arterial?" — Vide *Med. Chir. Trans.*, vol. 13, page 431.

† Works, by Palmer, vol. 3, 193.

the small part that remained regained its former appearance, after the extension of eicatrization, as the infant recovered from its weakened state. The darker hue is more commonly remarked on the surface of subcutaneous nævi, in which the vessels are larger, and the circulation thereof slower, than the closely-arranged vessels of cuticular*. In the latter, however, these changes are often seen in the varying states of the circulation. It is a circumstance well known in the poultry yard, that the comb and gills of the cock, and wattles of the turkey, become purple and flaccid when the birds are out of health.

Although the structure of the vascular plexuses constituting these growths approximates closely to that of the venous canals, the blood circulating through them in health possesses more of an arterial than a venous character. The mode by which arterial blood is converted into venous is so complete a mystery, that it is difficult to speculate with safety respecting the causes which may operate in preventing this change taking place. But allowing these organs to be made up of extreme vessels preternaturally enlarged, it would appear as a necessary consequence that in their hypertrophied state they must be incapable of performing the functions required of them when capillary; that they can no longer be able to nourish and to secrete. Under such circumstances it does not seem unreasonable to conclude, that the blood circulating through them would not undergo the ordinary changes, but would still retain its arterial character. Even in those subcutaneous nævi the aspect of which would indicate their contents to be venous, the blood flowing from an incision into them must still be arterial. There would then be no retardation of the circulation, no collecting of the blood in convoluted plexuses; but it would at once escape as soon as it arrived from the arteries. The profuseness of the bleeding is satisfactorily accounted for by the number of the vessels divided, and the nature of the tissue not allowing of their retracting, or of contraction of their open mouths.

This vascular tissue possesses but little sensibility. It is well known that cutaneous nævi are less sensible than the surrounding skin. But little suffering is occasioned by incising them, and free division of their interior by acupuncturation is almost a painless operation. The injection of stimulating fluids likewise causes but little suffering. But perhaps the best proof of the low organization of nævi is afforded by what we know of their pathology, especially as seen in the chronic nature of their actions, and in their great tendency to lose their vitality. They evince great insusceptibility to the action of stimuli, as is observed in the difficulty of raising a vesication by the application of a blister to their surface, and in their indisposition to become inflamed when injected with highly irritating fluids, and after the breaking up of their texture by means of a needle. Inflammation of this tissue is marked by but little activity, and all the changes which follow this process, the effusion of lymph, obliteration of the spongy tissue, and subsequent absorption, are invariably slow and tedious. Soon after birth, a period when nutrition is active, and the growth of the body rapidly advancing, it frequently occurs that sloughing takes place spontaneously in the centre of the growth, and extends until a natural cure of the disease is thereby effected. I have witnessed several instances of this kind; but one of the most remarkable which has recently come under my notice is of sufficient interest to be detailed.

Case II. — *Extensive Nævus of the upper extremity cured spontaneously by sloughing.*

A robust healthy child, born November 28, 1837, was brought to me December 12, with a large nævus situated on the right upper extremity. It extended from a little below the middle of the arm on the outer part, over the whole of the fore-arm, except a little of the skin at the inner side; covered the back and the greater part of the palm of the hand, and the posterior part of the fingers as far as the second articulations. It was slightly elevated above the level of the skin, and of a bright scarlet colour, except towards the centre

* In cuticular nævi the scarlet discolouration on the surface completely obscures the larger veins ramifying in the cellular tissue immediately beneath. In subcutaneous nævi, these vesicles are often very perceptible through the thin skin, and being larger and more varicose, owing to the little resistance afforded to their increase, the circulation is consequently slower, and the blood of a more venous nature.

of the fore-arm, where it gradually assumed a purple tinge around a small sloughing ulcer about the size of a sixpence. The mother stated that this sore first appeared five days after birth as a small pimple, and had since gradually extended. The nævus, however, had not increased. For several weeks the sloughing process went on rapidly, at the expense of the morbid vascular tissue*.

Jan. 10th.—The sloughing sore half encircled the fore-arm; it was spreading in one direction whilst cicatrization was going on in another, at a part where the sloughing process, having entirely destroyed the abnormal growth, had stopped at the sound skin, from which had commenced the action of repair. There was a free discharge from a considerable but healthy granulating surface; the child's health did not appear to suffer; and the nævus remained stationary. Mortification still continued to extend. At times a long line of slough, nearly a quarter of an inch in thickness, might be observed at the edge of the sore; and the integuments were so completely destroyed, that the outline of the superficial muscles of the fore-arm could be easily distinguished at the bottom of the sore.

March 3d.—That part of the nævus occupying the hand and the commencement of the fingers was entirely destroyed; indeed, the only portion of the morbid tissue that remained was a narrow slip here and there of the border of the original nævus on the arm and fore-arm, the destructive process having been, as it were, prematurely arrested just before the completion of its task. Cicatrization had advanced with great rapidity, but there was still exposed an extensive suppurating surface, with pale granulations. The child was much emaciated, and very feeble. The mother was directed to give it port-wine occasionally.

A few days afterwards the child was brought to me evidently much worse. Apprehending that there was great risk of the infant sinking under the discharge, and that if recovery took place the extremity would be rendered almost a useless limb by the contractions of the cicatrix, I thought it right to propose amputation; but the mother being unwilling to consent, it was not urged upon her. The quantity of port-wine was ordered to be increased.

20th.—The suppurating surface was much reduced in size; indeed, there was only a small sore at the back of the hand, the whole of the fore-arm being covered with new skin. The fingers were much swollen and œdematous, the circulation being impeded by the contraction of the large cicatrix. The granulations had a better character, and the child's health was evidently improved.

May 1st.—The child has completely regained her health, but the limb remains so much deformed that I fear it will be of little use. The whole hand is much swollen, and the fingers are drawn backwards into a most awkward position. There is still a small sore at the back of the hand, which seems indisposed to heal, but there is scarcely any trace of the nævus remaining.

This case, which was watched with much interest, strikingly exemplifies the want of power evinced by these vascular tissues. In a strong healthy child a process of mortification was set up in a nævus circulating arterial blood, and this extended until it arrived at the sound skin, no part of which was destroyed, but, on the contrary, as soon as it was reached, the sloughing process was immediately arrested, the dead parts separated, and reparation soon commenced and advanced with rapidity. There are several circumstances which seem to indicate that this tissue is chiefly indebted for its nutrition to the vessels coming from the adjacent skin, and not from the parts beneath. Thus I have observed that mortification occurs generally in nævi of large size, and commences in the central part. The cessation, too, of the sloughing process before the entire destruction of the nævus, the part remaining being the marginal, likewise conduces to the same opinion. This was remarked particularly in two cases lately shewn me by my colleague, Mr. Hamilton. In one, mortification commenced in the centre of a nævus about the diameter of a good-sized orange, situated on the back of a child's head, and ceased, leaving undestroyed an entire ring of the scarlet vascular tissue, about a quarter of an inch in

* The only application made to the part was a bread-and-water poultice.

width. In the other case the nævus occupied the inner part of the buttock, and partly surrounded the anus, of a child three years of age. It was at least two inches in diameter, and the sloughing procees commenced and was arrested in a similar manner.

The subsequent contraction of the cicatrix seems, however, to act in constricting the vessels, as in both these cases, and in Case II., the remaining portions are now gradually disappearing. In another case, also under the care of Mr. Hamilton, of a boy five years of age, the sloughing process had completely destroyed a nævus, larger than a shilling, on the front of the leg, which, from its depending and exposed situation, must have been well disposed to take on this action.

The preceding observations on the structure and pathology of nævi and similar vascular growths, induce me to conclude that they essentially consist of an hypertrophied state of the capillary system of vessels, through which the circulation is retarded, and that the peculiar tissue which results is but lowly organized—that it possesses very little sensibility, and is extremely unsusceptible of the impression of stimuli—that its actions in health and disease are languid and chronic—and that it readily dies and takes on the sloughing process; characters which, I believe, have been in some degree overlooked, but which are important to be borne in mind in adapting the necessary treatment.

Before concluding this part of the subject, it is necessary to notice certain circumstances which might appear to be in opposition to the view that has been taken of the low organization of this abnormal tissue. These are—1. the difficulty of arresting their progress by securing the arteries leading to them; 2. the great rapidity with which they are frequently found to increase; and 3. the well-known experiments of Mr. Hunter, in which, after transplanting one of the spurs of the cock to the comb, he always found that the spur on the comb, when it took root, grew much faster, and became much larger, than that left on the leg, which he attributed to a greater power of action in the comb than in the leg*. In regard to the first, it must be observed that tying the main artery supplying a part—the operation usually performed in these cases—multiplied experience has shewn to be generally inadequate to arrest more than temporarily the progress of morbid growths, in consequence of the collateral circulation being so freely and quickly established, that the parts beyond are very soon as well nourished as they were before the circulation was interfered with. Instead, therefore, of any evidence of a high degree of organization being afforded by the repeated failure of this operation, the fact that it sometimes succeeds in stopping their progress, and more frequently perhaps than that of other morbid growths, is really an argument in favour of the contrary opinion. The rapidity of their increase is no ways more remarkable than the quick enlargement of the vessels by which the circulation is maintained when a large artery is tied. In both cases the change is one of hypertrophy, and not of new formation. It is more particularly remarked in subcutaneous nævi where the yielding nature of the surrounding tissues offers but slight resistance to their increase. In cutaneous nævi, the dense chorion does not so readily admit of such rapid growth. With reference to the third point, I must remark that, allowing the justness of the analogy between the structure of the highly vascular part of the cock's comb and the reticular texture of a nævus, it will be distinctly seen, on examining Mr. Hunter's preparations, that the peculiar vascular tissue of the comb is entirely superficial, and that the spur has taken root in, and grown from, the dense structure beneath.

[To be continued.]

* *Vide* Works, by Palmer, vol. iii. p. 273.

OF THE

PATHOLOGY AND MORBID ANATOMY OF VEINS.

By James Copland, M.D. F.R.S. &c.

It is chiefly to John Hunter that we are indebted for the earliest and best information respecting diseases of the veins. Since his time the researches of Baillie, Hodgson, Cruveilhier, Meckel, Breschet, Davis, Dance, Lee, Gendrin, Ribes, and others, have tended most essentially to advance our knowledge of these important subjects. Diseases of the

veins resemble those of the lymphatics and arteries in some respects, and differ from them in others, particularly as regards the constitutional symptoms. The veins never exhibit an alteration in all respects similar to aneurism, because their coats yield equally to pressure, and are not subject to the forcible impetus of the blood; besides, their inner coats are more susceptible of dilatation without rupture than those of the arteries; they are, however, more subject to inflammation, and to varicose dilatation, than the latter vessels.

Ossific formations, which are so often met with in arteries, are seldom found in veins. The difference of texture is not sufficient to account for this, for, as M. Andral remarks, the structure of the pulmonary artery is the same as that of the aorta, and the right side of the heart is organized precisely as the left; and yet ossifications are much more frequent in the aorta and left side of the heart than in the pulmonary artery and right side. The more abundant supply of nerves to the arteries than to the veins may, perhaps, tend to create a difference as to the nature and frequency of the diseases of those two orders of vessels. The constitution of the blood, and peculiarities of the circulation in each, may also contribute to diversify their maladies. The circumstance of the blood being oftener coagulated and organized in the veins than in the arteries, may chiefly be imputed to this latter cause, and to the more frequent occurrence of inflammation in the former vessels. Pus is more commonly found in the veins than in the arteries; this is owing to two causes—1st, to the greater frequency of inflammation; and 2d, to the circumstance of this fluid being carried into the veins by absorption. Perhaps this latter source is the more frequent of the two.

The constitutional effects occasioned by diseases of the veins differ very considerably from those which characterize the maladies of the other orders of vessels composing the vascular system; but the difference is chiefly apparent in acute inflammations of those vessels, as will be observed upon referring to what I shall hereafter add respecting these diseases.

Inflammation of veins gives rise to redness, or to a reddish-brown or violet tint, of their coats. But similar changes, usually however of a more uniform character, are often produced in these vessels after death, by the imbibition of the colouring matter of the blood. Indeed, this is the most frequent source of the different shades of colour observed in the veins, these shades varying with the state of the blood, and with the period after death at which the examination had been made.

The redness proceeding from this source is much more frequently met with in the veins than in the arteries, evidently owing to the constant presence of blood in the former after death. This change, however, differs, in pervading all the coats of the former vessels, whereas it is generally confined to the inner membrane of the latter. Redness, therefore, unaccompanied by other changes, cannot be considered as a proof of disease.

The veins, like other parts of the body, present changes arising from the secretion of coagulable lymph or albumen. This plastic and organizable matter, in which a number of morbid formations originate, is frequently found in the veins, either extended into membranes or accumulated in amorphous masses. It is always to be viewed, particularly when connected with redness or vascular injection, as a result of the inflammatory act. The experiments which M. Gendrin instituted upon the veins, as well as upon the arteries, have fully illustrated this point.

Coagulated lymph is found—1st, in the interior of the veins; 2nd, between their coats; and 3rd, on their external surface. When this substance is considerable, or obliterates the canal of the vessel, it generally becomes partially organized, and is changed to cellular tissue, if the life of the patient continue for some time. When the lymph is secreted in smaller quantity, so as not to interrupt the circulation in the vessel, it generally presents the form of a firm albuminous layer, without any trace of organization; but in some instances, as pointed out by Ribes, Gendrin, and Andral, it forms a delicate membrane, and presents evident traces of organization, being traversed by minute vessels. The surface of the vessel opposed to those membranes is sometimes, most commonly in the more recent cases, of a deep red colour; but in other cases, particularly those of older date, it is perfectly colourless. The coagulated albumen thus formed in the interior of

the vein may constitute small patches merely, or small circumscribed masses, or a complete continuous layer extending through the whole of one or several vessels. The polypous concretions described by Reil (*Fieberlehre*, b. ii., pp. 215, 297), belong to the second of those varieties.

Purulent matter is frequently found in veins. M. Andral states that coagulable lymph may gradually lose its physical characters, and be insensibly transformed into pus. That this may occasionally happen I will not deny; but it must not be supposed that the purulent matter formed in the veins is generally, or even frequently, produced in this manner. When it is found in the vein, it evidently does not proceed from a transformation of the plastic matter already noticed, but from a modification of the morbid action of the extreme vessels which secreted that matter, and from a change in the vital condition and cohesion of the internal membrane of the vessel; this membrane being somewhat softened, and frequently tumefied or thickened. When the purulent matter is formed from the vein itself, it is found—1st, in the cavity of the vessel; 2nd, infiltrated between its coats; and 3rd, surrounding it.

The purulent matter found in the interior of the veins is, however, more frequently conveyed there from some other part with the blood, than secreted by an inflamed vein. When detected in a vein, it is either pure, or mixed with the blood, or with coagula. When the pus is found connected with coagula, it has in some cases been found external to them, and in others contained in them. This latter phenomenon has led to some speculation on the part of some French pathologists. M. Andral states that pus contained within a coagulum has been formed there in consequence of some peculiar modification of the blood itself. This is possible, but is merely a supposition. It is more probable that the pus, whether poured into the vessel from its inflamed internal surface, or conveyed from a distant part with the blood, but particularly when it proceeds from the former source, is first formed; the blood coagulating around it owing to some obstruction to the circulation in the vessel, or to the effect produced by the morbid secretion on the fibrine of the blood.

The irritation and inflammation producing suppuration of veins, arise from various causes. These I shall point out when I treat of inflammation of veins; as well as shew that inflammation originating in a part of a vein may be propagated both towards the heart and in the course of the smaller branches. The connexion of redness, thickening, softening, &c. of the coats of veins, with the formation of purulent matter in them, has been fully illustrated by the researches of Ribes, Davis, Velpeau, Gendrin, Louis, Arnott, Lee, Louis, Tonnelé, and Dance; and similar appearances have come before me in the dissection of fatal cases of puerperal diseases. But pus is often found in the veins, without any change of structure of their parietes; particularly in those veins which arise in parts in a state of suppuration. I have observed in several cases, in which the uterus of puerperal patients contained purulent matter either in its cavity or in its sinuses, the uterine veins and the trunks in which they terminated nearly filled with a purulent matter. Similar appearances have been noticed by MM. Dance and Louis. Abercrombie and Tonnelé have found pus in the sinuses of the dura mater, in cases of caries of the bones of the head, &c. Pus has often been found in the veins near diseased joints, suppurating fractures, and unhealthy stumps, by MM. Ribes and Velpeau. Blanchard long ago found pus in the vena cava, in a case of abscess of the liver. M. Gendrin found pus in the veins in the vicinity of ulcers in the intestines. M. Andral has met with similar appearances. I have stated, in the article on "Dysentery," in my Dictionary of Practical Medicine, that abscess of the liver not unfrequently proceeds from the absorption of pus from the ulcerated intestines into the veins, which, circulating into the vena portæ, excites diffusive or asthenic inflammatory action, rapidly followed by the formation of purulent matter in the substance of the liver. This view is confirmed by the researches of M. Ribes, who has demonstrated that the villi of the intestines are principally composed of minute branches of veins. M. Andral found, on the examination of a case of diseased intestines and liver, the vena portæ and its branches lined with a false membrane. When purulent matter is formed in a part, and afterwards conveyed into the veins, as

in the instances now alluded to, their coats are in some cases apparently sound, and in others inflamed or softened, or their interior is lined with a false membrane, &c. In the latter cases, either the inflammatory action has extended itself to the veins, from the part primarily diseased, or the morbid secretions which have passed into them have irritated and inflamed their internal membrane, at the same time that they have induced serious changes in the blood and system generally.

Thus the morbid states of veins may either originate in themselves, or in the parts in which the veins commence. But, whether they be thus primary or consecutive affections, they may exercise a powerful influence on the system, and on distinct organs, both through the medium of the blood, and by continuity of tissue. 1st. Purulent matter secreted from an inflamed vein, or conveyed into the veins from an adjoining part, may mix with the blood, occasion febrile symptoms of an adynamic character, and be eliminated from the circulation nearly as fast as it passes into it, by the several emunctories. 2nd. This matter, when once carried into the circulation, may be secreted from it, into the parenchyma of some organ, the cavity of some joint, or even into cellular or muscular parts. 3rd. Purulent matter, circulating in the vessels, may induce inflammatory irritation, or such a state of the capillaries of an organ or part previously disposed to disease, as will rapidly be followed by disorganization and the formation of purulent matter in it. 4th. Inflammatory irritation excited in the internal membrane of a vein, may extend in every direction to other veins, especially in that of the heart, and thus implicate other organs or parts. This diffusive form of phlebitis generally occurs in debilitated or unhealthy states of the frame, or when the disease is consecutive on other maladies. 6th. Inflammatory disease of the internal surface of the veins will give rise to a secretion varying with the state of the vital powers of the system, and the secretion, mixing with the circulating current, will influence the state of those powers, and depress or otherwise modify them. 7th. Inflammation attacking all the coats of a vein is more likely to occur in a healthy body than that confined to the inner surface, and is more commonly productive of an effusion of coagulable lymph, which tends to limit the inflammation, to prevent the admixture of the products of inflammation with the blood, and thus to preserve the blood from contamination. 8th. This sthenic form of inflammation most frequently occasions obstruction and obliteration of veins; and, if the obliteration be seated in large veins, serious local and general effects may result, owing to the mechanical obstacle thereby presented to the circulation.

Softening of the inner membrane of veins is often observed, and is generally conjoined with redness, when it is the result of inflammation. Sometimes the softening is so great, that the inner surface of the vessel is readily reduced to a pulpy state by gently scraping it with the back of the scalpel.

Thickening of this membrane is often associated with the foregoing changes: it may be either partial or general. In the former case the inner surface of the vessel presents an uneven appearance.

But softening and thickening of the inner membrane of the veins are not always connected with redness; this membrane may present either of those alterations, or both of them, accompanied by paleness or by different shades of colour.

The valves of the veins are subject to the same alterations as the inner membrane. They are often deprived of their transparency, thickened, or partially destroyed; presenting the appearance of irregular fringes crossing the cavity of the vessel, and having generally coagulated blood adhering to them.

[To be continued.]

NERVOUS PAINS.

To the Editor of the Medical Gazette.

Sir,

A few months ago you did me the honour to insert in your useful journal a case I sent for your perusal, of successful relief of distressing crural and femoral pains, by tracing up the same to irritation of the lumbar part of the spinal column, and establishing there an active counter-irritation, after the plan of Dr. Griffin.

Another case about the same time presented itself to me, rather more com-

plicated in some respects, yet requiring similar treatment. If you should think it sufficiently interesting to insert in your columns, and at any time have a vacant space to fill up, by so doing at your own discretion you would further oblige, sir,

Your obedient servant,
GEO. C. WATSON, M.D.
Surgeon, &c.

Edge Hill, Liverpool,
July 26, 1838.

On the 8th of July, 1836, E. S., æt. 31, nursemaid, of a clear rosy complexion, and rather of a full habit, not having been *regular* for eight weeks past, came to me complaining of an aching pain which she had felt constantly in the arm and shoulder of the left side. The pain extended down the arm to the elbow, and radiating from the acromial region along the superior border of the scapula, up along the muscles of the neck towards the ear, through the mamma of that side, and along the clavicle. The shoulder itself was somewhat swollen, and painful to the touch. She had a similar affection about twelve months ago, but slighter and less obstinate. She has latterly suffered much from headache; bowels are rather torpid; no leucorrhœa or other ailment, but looks in good health, and is chiefly employed in nursing an infant, which she does almost always with the affected arm.

From the localized character of the pain, and from her making at the time no other complaint, I ordered a lotion to be applied, consisting of the acetates of lead and zinc, liq. ammon. acet. and Tra. opii, along with pills composed of equal parts of pil. al. c. myrrh, and pil. aloes c.

10th.—She called upon me again. Had not continued the lotion assiduously, because she did not perceive any benefit from it, and complained of the breast particularly being more painful at night.

12th.—Has used the lotion more frequently, and derived some relief in the shoulder and breast; still, however, the pain continues, and pressure on the humeral end of the clavicle gives increased pain. Complains of a feeling of weakness in the arm.

Ordered Poppy Head Fomentations to the shoulder at bed-time, and to continue as before the lotion and pills.

14th. — The fomentations have relieved the pain; bowels are well opened by the pills.

16th. — Catamenia have appeared, though scantily, yet with much relief to the pain in the shoulder, &c. She remarked that the aching pain seemed transferred from the shoulder to the loins, just before being "*unwell.*" I ordered her to leave off all medicines, and to rest the shoulder as much as possible; and in order to secure this, I requested her to nurse always with the other arm. She was desired to resume the poppy fomentations if the pain should return in the evening.

18th.—Was obliged to use the fomentation last night, on account of the pain. I ordered six leeches on the shoulder, and a resumption of the pills, which the bowels required.

19th. — Leeches relieved the pain; tumefaction lessened.

25th.—Has been into the country, and whilst there put on leeches again. She has returned to her situation again, the pain still tormenting her; she feels it now most in the breast, along with a general soreness of the part. A small lymphatic gland near the clavicle, being rather enlarged, is all that can be discovered.

I prescribed a blister for the shoulder.

28th.—The blister, she stated, increased the pain whilst rising, but then procured for her some remission of this troublesome symptom. I ordered a purgative pill containing alterative doses of pil. hydrarg., and also a grain of sulph. quinæ three or four times a day.

Aug. 2nd.—The quina of no use; changed it for the carb. of iron, in drachm doses three times a day.

8th.—No marked benefit from the last tonic; changed all the medicines for full doses of ol. terebinth. and ol. ricini.

14th.—None of the medicines seem able to control the pain; blister allowed to heal several days back; the aching is constant, without benefit from remedies prescribed, except a transient mitigation from the last purgative. I again examined minutely every spot where she complained of pain, thinking that I might have hitherto overlooked some obscure lesion of the shoulder-joint, or surrounding soft parts, but without detecting any thing. I then determined to try acupuncturation over the deltoid muscle, which gave for the hour or two after using it some relief. After making use of them three or four

times, and finding the pain always obstinately to recur in a few hours, I laid them aside. On again questioning herstill more particularly about her back, she admits that occasionally both in the present and in the former slight attack she has felt a trifling tenderness in the spine, between the shoulders, but only when the edges of the stays meeting behind were pressed against her backbone by the back of the chair she might be sitting upon, which was so entirely relieved by a slight change of posture, that she "felt and thought no more of it." I carefully, therefore, manipulated each cervical spine without giving her the least pain, until I came to the third and fourth dorsal vertebræ, which were found tender on steady and strong pressure. She distinctly asserted that she never felt any thing of it in common, nor at any time except in the transient way I have just mentioned. Above and below these two vertebræ she felt no pain at all from the same pressure. I was now satisfied that I had hit upon the source of the disorder for the first time, and ordered immediately a long blister beside those spinal projections, requesting her to keep it open for some time with the usual dressings. I ordered her also a more active purgative of aloes and gamboge, because her bowels were difficult to keep open, though she regulated her diet. The blister rose well, and she declared that it had relieved her more than any thing hitherto tried. I ordered her to go home to her country air, as the town never seemed to suit her; for in the latter the menses were always checked, either becoming very scanty, or ceasing entirely for two or three months at a time. In October she returned to her place, with the pain in the shoulder scarcely perceptible; headaches were gone; the catamenia had been sufficiently abundant, and her health improved. The town life again affected her health; headache occurred; menses ceased; bowels got sluggish; and the old pain returned in its former severity, and extending farther down the side even to the thigh and leg, sometimes stretching from the left breast to the sternum, along with the tenderness of the dorsal vertebræ, for which I ordered what had succeeded before. To give immediate ease, I requested her to keep up a successive crop of pustules by means of the ung. ant. tart., which seemed the only way to anticipate the symptoms of spinal irritation whilst she remained in her town situation. I need hardly add that she was advised to change the town for the country as soon as possible, as the latter always improved her health, though it clearly failed in removing the complaint.

The two-fold periodicity of this pain is remarkable; the remission of it in one case depending on the alternations of day and night apparently; the other on constitutional changes connected with the influence of the sexual system. The only question in this case is whether the continuance of tonics for more than a fair trial of sixteen days would have removed the disease. I feel quite satisfied it would not.

SURGICAL CONTRIBUTIONS.

By John Grantham,
Surgeon.

Gun-shot Wound. — Loss of Seven Inches of Rib.

In reflecting on the nature and properties of serous membranes, it has often appeared to me that the large serous surfaces which envelop the vital organs are not merely intended as a tunic, or simply for the secretion of serum, but possess a protective influence. Bichat remarks, that a very important use of the serous membranes is to contribute to the independence of the vital powers and functions of different parts, by separating the respective organs.

The following case of gun-shot wound exemplifies the amazing extent of protection the pleura must have afforded to the right lung, amidst extensive inflammation, laceration, and ulceration of the cutaneous, subcutaneous, muscular, and ligamentous structures connected with the pleura costalis. It also further supports my practice of not reducing either the normal temperature or vitality of the superficial structures in inflammation of the tendinous or cartilaginous parts of the body*.

August 19th, 1832.—Mr. John M., a stock-broker, by accident received the contents of a loaded gun, at four feet distance, on the seventh rib of the right side, about an inch from the sternum; the shot, clothes, &c. traversing the rib, and causing a compound comminuted fracture of the bone, and finally lodging under the integuments over the latissimus dorsi muscle, near the spine. I cut down on the part,

* *Vide* Medical Gazette, vol. xiv. page 673.

and took out what appeared a very full charge of shot, with an inch and a half of the rib. He was bled from the arm to the extent of twenty ounces of blood, and took Hydr. submur. gr. vj. immediately, with twenty-five drops of the tincture of opium. The opiate draught was repeated every four hours, and he had a large hot bread-and-water poultice applied to the side. In the evening I found him much relieved, expressing great comfort from the warmth of the poultice. I enjoined a spare diet of gruel or barley-water; the feet to be kept warm by means of hot water.

20th.—Breathing short, and pain extending up the chest and toward the right axilla.

Continue the opiates, poultice, and diet; repeat the calomel at bed-time.

21st.—The pain subsided.

Ordered a cathartic draught every four hours. Continue the low diet and poultices.

22nd.—Another portion of the rib came away in the poultice. The bowels not having acted, I administered an enema, which afforded great relief.

A cup of mutton-broth allowed in the twenty-four hours.

25th.—Another portion of rib, with several shot-corns and pieces of clothing, came away at the posterior wound; the purulent discharge very copious.

Ordered a milk diet, with strong beef-tea.

27th.—I removed another portion of the rib from the posterior wound, three inches and a half in length. The treatment of poultices was continued till Sept. 9th, during which time the discharge very much abated. Shot, pieces of cloth, and fragments of bone, kept coming away at intervals. A roller was applied, with dry lint dressings, so as to afford moderate support. On the 21st he returned home to Wapping, convalescent, although the wound remained open, and continued discharging slightly until the latter part of December; shot occasionally coming away. I consider he must have lost at least seven inches of the rib. The case terminated without any pectoral affection, the sounds of the chest becoming natural, and I am inclined to believe there is no adhesion of the two surfaces of the pleura.

Bony Tumor in the Meatus Auditorius.

Dec. 9th, 1838.—Thomas Middleton, aged 45 years, applied to me in consequence of complete deafness of the right ear, which, on examination, I found to be caused by an osseous tumor growing from the upper and back part of the meatus auditorius, extending across and filling up the passage so as to interrupt the ingress of sound. I broke off the tumor, which was attached to the meatus by means of a narrowed peduncle, with a strong pair of dissecting forceps. The patient compared the separation of the tumor to the firing of a large cannon close to the head. The hearing gradually became restored. The annexed figure represents the osseous tumor in its natural size, and showing its narrowed stem:—

Apparatus for Fractured Clavicle.

In presenting the following plan for the treatment of fractured clavicle, it may be right to premise—what are its advantages? First, the parts are kept in apposition without excoriation. Secondly, the arm is not suspended from the neck. Thirdly, any application can

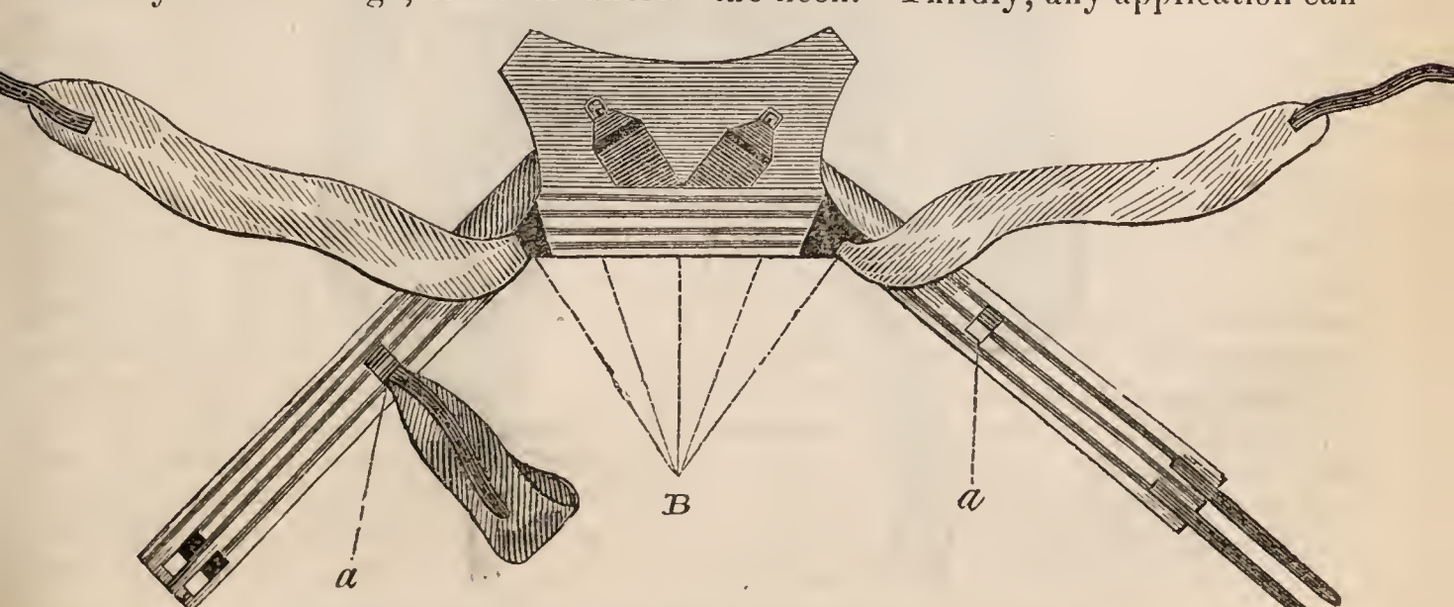

Fig. 1

be applied to the region of the fracture without taking off the support. Fourthly, the patient can undergo exercise either in riding or walking, with comparatively no pain.

Figure 1 is a view of the bandage as laid flat on the table; *a*, *a*, are two loops to which a strap may be affixed, according to the side or sides of the fracture, for suspending the fore-arm. *B* is intended to point out the part of the bandage where it is padded.

Figure 2 represents a front view of its application.

Figure 3 represents a back view of its application.

Crayford, Kent, Aug. 3rd, 1838.

SPASM OF THE GLOTTIS.

To the Editor of the Medical Gazette.

Sir,

There is one method for the relief of hysterical spasm of the glottis, which, so far as I know, has not been mentioned among the various means recommended by systematic writers; it consists simply in irritating the fauces or pharynx with a feather, or, in the absence of any thing more convenient, with the forefinger. I have found it very successful. Retching, of course, follows the irritation of the pharynx. If the patient be insensible, she is restored to consciousness immediately.

Sometimes the hysterical fit assumes the epileptic form; in this case I have always been unable to open the mouth. The muscles attached to the hyoid bone (these will be found exceedingly tense) should be quickly and repeatedly compressed between the finger and thumb. This plan will not always succeed, but I have never failed in putting an end to the paroxysm by combining it with extension of the forearm and fingers sufficiently forcible to overcome their tetanic flexion. The relief is but temporary; an opportunity is, however, afforded for the administration of assafœtida or valerian.

In a case of spasm of the glottis in an adult male which came under my notice, an inspiration immediately followed pressure of the thyroid cartilage.

Might not this simple remedial means of irritating the pharynx be made very available during the paroxysm of infantile spasm of the glottis? Dr. H. Ley mentions it in his Essay on Laryngismus Stridulus (p. 270), but not so impressively, I think, as it deserves; nor as if he had any practical knowledge of its utility.

The action of tartar emetic in acute croup, or in cases so called, may possibly be analogous. By the act of vomiting, the muscles of the larynx are combined with the other respiratory muscles in an expiratory act, and so the spasmodic closure of the *rima glottidis* is overcome.

I am, sir,
Your obedient servant,
Thomas Laycock.

House-Surgeon to the York County Hospital.
August 1838.

CLINICAL REPORTS.

By John Burne, M.D.,

Physician to the Westminster Hospital.

Creasote in Gastro-enteritic Irritation; and Sesquioxide of Iron and Port-wine in Chronic Diarrhœa.

The confidence with which Dr. Elliotson * speaks of the "extraordinary power" of creasote in "arresting vomiting," led me to make trial of it in various forms of irritation of the stomach and intestinal canal; and the result has been to establish in my mind creasote as a most valuable addition to the materia medica. The patients speak of it as "warming, comforting, and nourishing" the stomach; few object to the tar flavour, many rather like it.

In no cases has the creasote been more strikingly efficacious than in relieving the nausea and morning sickness of intemperate persons; a good example of which occurred very recently.

Case VI.—A tradesman's wife, rather given to indulgence in fermented liquors, had been affected with irritable bowels and incessant nausea for many weeks; had lost all appetite; and was reduced from a state of *embonpoint* to one of rather alarming emaciation. She had been under the judicious treatment of a practitioner who had tried many remedies without benefit. When we met in consultation I suggested the creasote in the dose of one minim three times a day. From the first dose advantage was derived, and more and more from every repetition; the irritability of the stomach and intestinal canal was appeased, and in the course of a week cured; the appetite returned, and with it a better state of health than she had enjoyed for a long time previously.

The dose in which I have given the creasote is from ♏i. to ♏ii. mixed with 3i. of mucilage and 3ix. of water.

In the following case of organic disease of the stomach it was productive of great comfort and relief:—

Case VII.—I was desired to see a gentleman's coachman in the last stage of organic disease of the stomach, as evidenced by extreme universal atrophy; by the linear face characteristic of

* On the Medicinal Properties of Creasote. By John Elliotson, M.D.—Med.-Chir. Trans. vol. xix. p. 217.

pyloric disease; by constant vomiting after food; inability to take food, except in the most trifling quantities; and a pulse not greater than a thread.

In this hopeless state I prescribed the creasote in the dose of ♏i. every six hours.

It agreed well, and appeased the irritability of the stomach so that it retained food; and so much did it "comfort and nourish him," to use his own expression, that at my next visit I found him with the creasote mixture beside him in bed, where he kept it for the convenience of taking some frequently. He died in a short time, and on examination the pyloric extremity of the stomach was found in a state of extensive cancerous ulceration.

CASE VIII.—*Dysenteric Diarrhœa.*

Jane Colles, age 45, a patient at the Public Dispensary, in April 1836, applied on account of a violent bowel complaint which had troubled her for several days, attended with severe griping pains and looseness, loss of appetite and strength.

To take the creasote three times a day.

In a few days she reported herself cured: improvement had taken place immediately after she commenced the medicine; the pain left her; the bowels became regular; and the appetite returned. She said it warmed and nourished the stomach.

CASE IX.—*Violent Dysenteric Diarrhœa.*

Mary Hewit, age 41, an out-patient at the Westminster Hospital, who had been under treatment for an affection of the chest, was seized, on the 15th of February, 1838, with a violent diarrhœa and griping pains. The dejections were watery, and so numerous that she had no idea of the number. She had crampy pains and cold sweats; was languid and faint; passed very little urine; was thirsty; had headache, and a frequent feeble pulse.

Creasote every four hours.

After the second dose the symptoms were in some degree relieved, and after the fifth began to abate very decidedly. The pains soon ceased, the purging gradually subsided, and convalescence was established in a few days.

CASE X.—*Chronic Diarrhœa.*

Mrs. Baldry, age 28, suckling a child eleven months old, applied to me on account of a looseness of the bowels and griping pains, which had harassed her for the last three months, obliging her to retire to the closet five and six times a-day, and not unfrequently disturbing her in the night. The tongue was rather foul, the urine variable, the appetite indifferent, and she was much reduced by loss of flesh.

Creasote three times a-day.

In the course of four days the pain was materially relieved, and the action of the bowels reduced to once or twice a-day; the dejections having lost their slimy character and become more natural. The appetite was still indifferent, and the debility great; on which which account I recommended

Disulphate of Quinine and Sulphate of Iron, aa. gr. j. twice a-day, in addition to the Creasote.

By these means her health was reestablished.

CASE XI.—*Gastro-enteritic Irritation.*

Mr. Wheeldon, age 60, was attacked, on the 25th of January, 1836, with vomiting and purging (the latter every ten minutes), crampy pains in the limbs, and violent griping pains in the bowels, with distressing tenesmus. The matter voided from the bowels was watery, and clear frothy water was ejected from the stomach. He took, of his own accord, first castor oil and tincture of rhubarb, then tincture of rhubarb and laudanum; but finding he gained no relief, and that he was becoming very low, weak, and trembling, he applied to me next day.

Creasote, gtt. j.; Mucilage, 3ss.; Water, ℥ss. every three hours. A mustard poultice to the abdomen.

Immediately after he had commenced this treatment, the vomiting, purging, and all the other symptoms, were mitigated, and in the course of two days entirely removed.

CASE XII. — *Gastro-enteritic Irritation.*

Mary Hincks, age 25, an out-patient at the Westminster Hospital, was seized, on the 16th of August, 1837, at five o'clock, P.M. with sickness and vomiting, excruciating pains in the bowels, followed by diarrhœa, great thirst, heat at stomach, and diminished secretion of urine. Next day, when she applied for

relief, all the symptoms continued urgent, the bowels acting every quarter of an hour. She attributed the attack to having eaten French beans at dinner.

Creasote every four hours.

Immediate relief followed the use of the creasote. The sickness abated, the pain subsided, the looseness was checked, and the alimentary canal soon restored to its usual tranquil state.

CASE XIII.—*Gastro-enteritic Irritation.*

Samuel Brimner, age four years, a healthy boy, was taken ill on the 5th of August, 1837, with diarrhœa; sickness supervened on the 7th; and on the 10th he applied as an out-patient at the Westminster Hospital. He was affected with pain in the bowels and looseness; he was sick almost constantly, and vomited frequently; he screamed with pain night and day, disturbing his rest, and making him very ill.

Mistura Cardiacia,

from which no relief.

12th. — Ol. Ricini, ʒj. Tinct. Opii, gtt. j. in addition.

17th.—The pain in the bowels was relieved by the oil and laudanum, but the sickness continued.

Creasoti, gtt. iij.; Mist. Acaciæ, ʒiij.; Aq. distillatæ, ʒiij. M. ʒij. ter die.

This at once soothed the irritability of the stomach, and in the course of a week the boy was discharged well.

CASE XIV.—*Gastro-enteritic Irritation amounting to Cholera.*

The subject of this case was Mr. Pennington, one of the pupils of the Westminster School of Medicine, of fair complexion and sanguine temperament. He had been working hard in the dissecting-room, and on the day previous to the attack had been engaged for some time in inflating the intestines and bladder of the subject under dissection. He got up on Thursday morning, the 25th of January, as well as usual, and ate a hearty breakfast, soon after which he was troubled with fœtid eructations, and in two hours was suddenly seized with nausea and griping pains, succeeded first by violent purging, then by vomiting, the dejections being dark, offensive, and very copious. These symptoms continued through the day, and at night the pain had become so intolerable, the other symptoms persisting, that he apprehended danger, and called to his aid a neighbouring practitioner. Opium was administered and relieved the pain, but did not arrest the purging or allay the sickness. The night was passed restlessly; and the purging and sickness still harassing him through the day, he requested me to visit him on the subsequent evening, Friday. He then complained of rigors, lowness of spirits, prostration of strength, loss of appetite, and thirst; of a sense of weight in the epigastric region, and general soreness of the abdomen; said that the quantity discharged from the bowels amounted to gallons, which alarmed him.

Mustard cataplasm to the abdomen, and creasote, ℥j. every three hours. Warm brandy and water, weak.

The creasote, to use his own words, acted as a charm; the first dose removed the weight in the epigastric region, appeased the sickness, and stopped the purging; after the second the thirst diminished; the cataplasm relieved the tenderness, and the brandy and water the rigors. From this period his recovery was uninterrupted.

In the MEDICAL GAZETTE, July 9 and 16, 1831, I published an account of an epidemic dysenteric diarrhœa, which was effectually controlled and cured by castor oil and laudanum. I do not in the present communication propose the creasote as a substitute, for in dysenteric diarrhœa the oil and laudanum are invaluable, and often indispensable, but I should recommend the use of the creasote in addition; the oil and laudanum being administered every second or third day, and the creasote twice or thrice daily; the magnesia, as employed in the article above referred to, being omitted.

CASE XV.—*Sesquioxide of Iron and Port-wine in Chronic Diarrhœa.*

Richard Hill, age 38, a footman, of a tall strong frame of body, in the habit of living freely, said that about three years ago his bowels began first to trouble him, being disordered for several weeks at a time, then regular, and then again disordered, which variable state has continued to affect him more or less ever since. Within the last three months the looseness has increased to an urgent

degree, and impaired his general health seriously. He has been under treatment, and has taken a variety of astringent and other medicines.

On the 19th of December, 1837, he consulted me: he complained of being worn down by a violent purging, to the extent of from 20 to 30 dejections in a day, of a light ochre colour, frothy, and containing granules, like hop-seed or rice; of having pain in either hypochondrium, of flatulence and of griping before going to the closet; of feeling full and bloated in the stomach; of nausea almost constant, and of frequent retching. The appetite was tolerably good, but the mouth hot and clammy; the pulse frequent, small, and weak; some cough, and very considerable emaciation. The exsangueous and emaciated state of this patient, the pearly-white conjunctiva, and the short cough, gave reason to suspect that tubercular disease of the lungs was setting in, and that the diarrhœa depended on tubercular ulceration of the mucous membrane of the bowels.

Ol. Ricini, ℥iii.; Tinct. Opii, ♏viii.; Aquæ Cassiæ, ℥ss. alternis auroris. Ferri Sesquioxidi, ℈j. ter quotidie ex vini rubri (Port) ℥i.

26th.—The oil and laudanum quieted the bowels for six hours, and then produced three more natural dejections. The iron and wine have agreed perfectly; the bowels are become steady, the dejections feculent, and not exceeding two daily; the flatulence and bloated sensation relieved, as also the griping pains and nausea; the appetite improved, the strength is returning, and there is more power in the pulse. He is doing well in every respect.

To continue the iron and wine, and to repeat the oil and laudanum twice a week.

The medicines were persevered in with regularity for three weeks longer, the report on the 19th of January saying, that the bowels feel strong, and act spontaneously once or twice a day; he is gaining flesh and strength, and is in every respect convalescent.

I was led to prescribe the iron and wine in consequence of having been informed, accidentally, that soldiers labouring under the flux, in the Peninsular war, cured themselves by scraping off the rust of iron hoops and taking it in grog. In the above instance the effect was certainly striking, and soon I had the opportunity of giving it a further trial in two cases of severe diarrhœa, in constitutions impaired by intemperence. One was a postboy, with signs of diseased liver, with excessive œdema of legs, thighs, and scrotum, emaciation, and a pale exsangueous face. He suffered under an incessant diarrhœa, which was hurrying him fast to the grave, and which resisted the sulphate of copper and opium, &c. In him the iron and wine had an immediate good effect, controlling the action of the bowels, and improving his health, so that he gained strength, got rid of the œdema, and was able to leave the hospital fairly recovered. The other case was similar as regarded the diarrhœa, but there were pulmonary signs indicating tubercles, and there was no œdema. Here, also, the iron and wine controlled a most severe chronic diarrhœa, and restored the strength in a markable degree.

24, Lower Brook Street,
Aug. 13, 1838.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Die Pathologie und Diagnose der Krankheiten der Brust, insbesondere erläutert durch eine rationelle Erklärung ihrer physikalischen Zeichen nebst neuen Untersuchungen über die Töne des Herzens. Von CHARLES J. B. WILLIAMS, Doctor der Medicin, &c. Mit zwei lithographirten Tafeln. Bonn, 1838. 8vo. pp. 244.

THIS is the second edition of a German translation of Dr. Williams's excellent treatise on the pathology and diagnosis of diseases of the chest. It is executed by Dr. Hermann Velten, who has done good service to his brethren by presenting them with so useful a work in a native dress. He observes in his preface that, by the author's kindness, he has been enabled to enrich his translation with many interesting additions, which have not yet been published in England.

The student who is learning German, and possesses the original treatise, by comparing it with Dr. Velten's translation might hasten his progress in that important language, as well as in the art of auscultation.

The Power, Wisdom, and Goodness of God, as displayed in the Animal Creation; shewing the remarkable Agreement between this Department of Nature and Revelation. In a Series of Letters. By C. M. Burnett, Esq. Member of the Royal College of Surgeons. London, 1838. 8vo. pp. 549.

We have often wondered at the bequest of the Earl of Bridgewater, and have been tempted to suppose that he must have been unacquainted with Paley's unrivalled Treatise on Natural Theology. One who had studied this masterly work with the attention it deserved, would no more have proposed the same subject to the scientific men of the day, than he would have ventured to give Paradise Lost as a theme to the poets. Nevertheless, the bold attempt has been made, and though few would set the Bridgewater Treatises on a level with their matchless predecessor, they are valuable works, and will furnish ample stores for all future commentators upon Paley. The success of these works has encouraged other competitors to come into the field, and among them is to be numbered the author before us. In the first two letters he combats the theories of modern geologists, as to the age of the earth and the effects of the Mosaic deluge. In the remaining thirteen he gives some account of the classification of animals; the composition of animal bodies, the resemblance of electricity to the vital principle; and the physical conformation of man; and dilates on the evident proofs of design exhibited in the adaptation of all creatures to their various stations. Mr. Burnett's work shews much reading and industry.

MEDICAL GAZETTE.

Saturday, August 18, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
Cicero.

LAY MEDICINE.

SPECIFICS FOR GOUT.

In our article upon this subject the week before last, we observed that Dr. Skrimshire's work*, however valuable as a guide to the young but educated practitioner, might possibly do harm in the hands of the mere layman; and that doses of powerful medicines were sometimes recommended in this manual, which should be prescribed only by the regular medical attendant. Extreme cases, it is true, require extreme remedies;

> ——— "immedicabile vulnus
> Ense recidendum est, ne pars sincera trahatur:"

But we do not call in the ignorant bystander either to judge of the necessity of the operation, or to perform it, if inevitable.

In gout of the stomach Cullen sometimes increased the dose of opium to ten grains twice a day; but if our female physicians and clerical practitioners of medicine and surgery were to prescribe it after this fashion, we should feel inclined to imitate the Chinese, and altogether prohibit the importation of opium. Dr. Darwin, indeed, once went so far in the administration of some potent drug, that being congratulated on having unexpectedly cured a patient, he answered, "No, sir; I poisoned him into existence." This kind of medication, however, far from being held out as an example to the amateur, is to be admired rather than imitated even by the skilful physician.

It is remarkable, that although the author says that gout is a disease of the rich only†, yet he teaches his son how to cure it; and the dyspepsia, of which he discusses the symptoms and the cure, is evidently produced by venison and champagne, and not by water-gruel and bad potatoes. Take an example:—

"Sound beer of moderate strength, such as is brewed from twelve to fifteen gallons from the bushel of malt, is, as I observed before, the fittest beverage for a labouring or otherwise active man;

* The Village Pastor's Surgical and Medical Guide, &c.
† Ibid. p. 386.

and to such a person two half-pints may be allowed daily. When wine is preferred to beer, it should be sherry; and the quantity most conducive to the health and comfort of a dyspeptic is two glasses, one taken with water during dinner, and the other without water at the conclusion of the meal. A person in the enjoyment of health and a good stomach ought not to indulge in more. When brandy is thought preferable*," &c.

Surely these sentences contain an abrupt transition from "a labouring or otherwise active man" to your comfortable tradesman, and corpulent farmer of the highest class, easy in their circumstances, but uneasy in their health; with wine and brandy at their command, but stomachs that can turn Cognac into hydrochloric, or rather, perhaps, into acetic acid. The puzzle is, how Mr. Skrimshire the curate, or the other clergymen for whom the book is written, are to get these queasy patients. How is it that the opulent cultivators of the Isle of Ely, labouring under gout and dyspepsia, and limited by their medico-clerical adviser to two glasses of sherry a day, do not send for the real doctors, but are supposed to squeeze gratuitous advice out of amateurs? In villages where there is no doctor, there can be no dyspepsia crapulenta; and in those jovial corporate towns where aldermen's gowns and gout have been handed down from one generation to another, practitioners of every grade must unquestionably abound. We hope that lay doctors do not treat their more regular brethren as auxiliaries have sometimes done the native troops—overpowering their allies, and taking possession of the country they were engaged to defend!

It is worth remarking, that this aristocratic disease, the gout, has called forth a pamphlet addressed to the laity by a lay sufferer*, which, though anterior in date to Dr. Skrimshire's book, has but lately fallen into our hands. It is a smart, clever, little affair, though perhaps partaking too much of the characteristic of lay practitioners—infallibility. "When I was young in practice," said Dr. Radcliffe, "I had twenty remedies for every disease; now that I have grown old, there are twenty diseases for which I have not a single remedy." Now, your lay practitioner enjoys an eternal youth of knowledge. Exceptions in treatment, difficulty of diagnosis, impossibility of prognosis, exist not for him; and in his hands medicine shakes off its character of a tentative art, and becomes one of the exact sciences. In the opinion of all who know nothing about the matter, a medical difficulty is like a quadratic equation—the solution is quite certain, if the professor understands his business. Philanthropists sometimes wonder that the obstinate persist in bearing toothache, and suffering the pains, without the honours, of martyrdom, when there is not a practitioner of physic, except the professional ones, but has an infallible cure for it. The "Fellow-sufferer" is quite sure that the waters of Wisbaden will cure the gout, satisfying the "gouty or rheumatic that they are the only specifics; and that the phrase attached to gout of *opprobrium medicorum*, applies rather to the ignorance or selfish concealment of the remedy in that learned body, than to any absence of the remedy itself. It is in truth a God-send, which has been almost withheld from our general knowledge till now†."

Nevertheless, if the gouty had not heard of Wisbaden, it was not from the virtues of its springs having been merely whispered about, or dilated upon

* Skrimshire, p. 392-3.

* Wisbaden recommended to the Gouty and Rheumatic. By a Fellow-sufferer. London. 1837.

† Wisbaden, &c., p. 13-14.

in a few exclusive MSS. Hufeland, in his well-known Treatise on the Mineral Waters of Germany, when recounting the diseases cured by the waters of Wisbaden, put at the very head of the list the gout in all its forms, including the most obstinate ones *; and we have no doubt that their merits are abundantly set forth in all the works on the mineral springs of Germany in which the literature of our neighbours is so rich. As for the English reader, he might pick up a hint or two in the Bubble-book, or find an account in the useful and elaborate treatise of Dr. Granville, on the Spas of Germany. Yet, perhaps, in the Palace of Truth, a good many medical men might plead guilty to a part of the accusation, and confess, that, until very recently, they had never heard of these waters, far less read a detailed account of their real or supposed merits. As to imagining that their merits have been concealed by our learned body, nothing can be more unfounded. Concealment of the real is not the fault of the day; the ordinary error is sending forth so much of the probable, the possible, and the fanciful, to keep company with it, that *noscitur a sociis* is applied to plain fact, and truth suffers from temporary disbelief.

Granting that the waters of Wisbaden cured the "Fellow-sufferer," it remains to be seen whether the cures are numerous, permanent, and safe. If a single cure or two were enough to justify a particular mode of treating a disease, and entitle it to be called a remedy, we should have at least a couple of hundred cures for Asiatic cholera; from croton oil down to cold water.

If unsafe cures of gout were to be admitted as valid, the Portland powder might have stood its ground to this day; but it was unfortunately found that those who had finished their course of the medicine were "attacked with apoplexy, asthma, or dropsy, which proved fatal *." If we mistake not, colchicum has lost much of its vogue from the same cause, especially in the numerous instances where patients have undertaken the management of their own cases.

It may be mentioned as a curious coincidence between Dr. Skrimshire's book and the pamphlet just mentioned, that while the former supposes the gout to be treated by clerical advisers, for want of doctors of course, the latter expressly informs us of the deficiency of physicians at Wisbaden, and therefore teaches the author's brethren how to treat themselves. One clever, well-informed doctor monopolizes the practice; the consequence of which is, that some of the patients complain that at each interview they must repeat their tale from the beginning. Then this monopolist is a German, and as few English speak his language, and he does not speak theirs, the complaints and answers must be carried on in French, a tongue foreign to both. Mutual misapprehensions must necessarily arise; a thing unpleasant at all times, but unbearable by an irritable patient suffering under chalkstones and gouty dyspepsia, while the pointed paving-pebbles of Wisbaden lend additional fire to his feet. Hence "the Fellow-sufferer" wishes the Duke of Nassau to import a sufficient supply of good physicians, skilled in French and English, or else to allow English physicans to practise there. At present the unlicensed intruder would find himself in an uncomfortable position. He must not expect to be favoured with a letter from an attorney giving him notice of an action to be tried next Trinity Term —the Duke of Nassau *v.* Dr. X.—but just be satisfied with a simple order enforced by the police, that within 24

* Die Gicht in allen ihren und den hartnäckigsten Formen.—(*Praktische Uebersicht der vorzüglichsten Heilquellen Teutschlands.* 2d Edit. 1820, p. 181.) He afterwards adds gout in the head and gout in the neck. The German physicians call many forms of disease gout which we should class under rheumatism.

* Cullen's First Lines, § 557.

hours he must quit the dominions of his highness.

The author recommends his patients to travel by way of Antwerp, from which town he anticipates, and we believe correctly, that they may this year proceed to Liege by the rail-road in five hours*. Instead of sleeping at Liege, they are to go on to Chaudes Fontaines, seven miles farther, "where there is, in a beautiful valley, an excellent inn, and natural warm baths." (p. 17.) The next day the patient reaches Cologne, and Ems on the following one. Here he is to take a few baths, and then to be cured by drinking and bathing at Wisbaden. In bad cases two or three courses may be requisite.

In our opinion, the mode of proceeding from London to Wisbaden admits of and requires considerable variety, according to the state of the patient. If motion is very painful, and every jolt an agony, let him go by Rotterdam and the Rhine to Mayence, whence he may get to Wisbaden in an hour. If he can bear motion, and wishes for a shorter sea-voyage, let him go by Antwerp, or, better still, by Ostend; and if the sea is his horror, and nausea his *bête noire*, he may journey by Calais, Lille, and Brussels, or by Paris, Verdun, and Metz. How the author contrived to get bad dinners at Wisbaden, we know not. He says the diet of the restaurateurs is very adverse to health; we should say the same of the table d'hôte at the *Quatre Saisons*, but for the opposite reason: while the restaurateurs' dinners are too shabby for a gouty man, the table d'hôte is too good for any one who does not wish to become gouty.

On the whole, the pamphlet is instructive as well as amusing, and tends to justify the old axiom, that the gouty are often clever men, with clear heads.

* There is now a complete line of railroad from Ostend, as well as Antwerp, to Liege; so that, with a favourable passage, it is possible to reach the Rhine in a little more than thirty hours after leaving London.

GLASGOW EYE INFIRMARY.

Clinical Lectures by Dr. Mackenzie.

Lecture II.

Antral Polypus.—Abscess in the Orbit.—Effusion of Matter on the Brain.

Janet Anderson (No. 8362), aged 44 years, applied here on the 25th March, 1838. She stated that nine weeks before that date her left lower eyelid had assumed a dark red colour, as if affected with ecchymosis from a blow. For twelve months she had been much troubled with pain in the left side of her head, attended by a feeling of pressure and stoppage in the left nostril, which was deficient in moisture and in the sense of smell. She complained of numbness over the left side of her face, and the hearing of the left ear was impaired. Both left eyelids, as well as the conjunctiva, were œdematous at her admission—a symptom which generally attends cases of pressure on the orbit, from whatever quarter or from whatever cause the pressure comes. There were no symptoms of dacryocystitis. The vision and the power of moving the eye were natural. Tongue clean; pulse 96. She stated that since the swelling of the eyelids commenced, her head had been relieved.

She was ordered to be bled at the arm, and to take two grains of Calomel with half a grain of Opium at bedtime.

June 13th.—Has not attended since last report, and went away without being bled or getting the pills. Pain gone. Still considerable swelling between the upper eyelid and brow, with a feeling of fluctuation towards the inner angle. Skin of a dark red colour; conjunctiva of lower eyelid red and swollen. Says that during the last fortnight there has been a discharge of matter from the eye. No discharge of matter takes place at present when the swelling is compressed. The eyeball seems to be somewhat displaced towards the outer angle of the socket. Says she is troubled with menorrhagia. Belly costive.

An incision being made through upper eyelid into the swelling, some matter was discharged, and a considerable cavity is felt by the probe, extending to the periosteum of the orbit.

A poultice was ordered, and two aloes and blue pills to be taken each night.

15th.—Complains of chilliness about six o'clock in the evening, followed by a hot fit.

The pills were continued, and 1½ grain of sulphate of quina ordered thrice a day.

18th.—Chilliness and feverishness diminished. The dose of quinine increased to 2 grains.

24th.—Feverishness gone.

Quinine continued.

28th.—A swelling projects from beneath the eyeball of considerable size, and having an indistinct feeling of fluctuation. The discharge from the opening above the eyeball has become very trifling. She states that there is frequently purulent matter discharged through the left nostril.

July 2d. — Complains of shivering. Swelling of face increased.

Was ordered 8 grains of Dover's powder at bed-time.

5th.—Last night the swelling opened spontaneously a little below inner canthus, and discharged a considerable quantity of thick matter. A probe introduced by this aperture passes through the substance of the lower eyelid, nearly to the outer canthus, and also inwards towards the orbit, to the depth of about an inch.

9th.—A feeling of fluctuation, apparently extending to the bottom of the orbit.

13th.—A feeling of fluctuation having been observed below eyeball, an incision was made with a lancet to the depth of an inch, but no matter flowed.

23d.—She was ordered to omit the quinine, and to take 2 grains of calomel with half a grain of opium thrice a-day.

29th.—Feeling her strength declining, she left the Infirmary, and died on the morning of the 4th of August.

Dr. Paterson took a cast from the eye a few days before she left the Infirmary; and the wax model which you see here gives a correct representation of the appearances.

On inspection after death, the eyeball was found in a state of exophthalmos, and pressed towards the temple; the conjunctiva in a thickened and fleshy state.

The whole surface of the left hemisphere of the brain was covered with thick, green, and very fœtid pus. At the forepart of the left anterior lobe there existed a very considerable depression of the cerebral substance, to the extent of about a half-crown piece, and the membranes covering this spot were very much discoloured.

On raising the left anterior lobe from the orbital process, purulent matter was seen to issue from the orbit through an ulcerated spot of the dura mater, at the nasal side of the orbital roof, in the centre of which there was a small carious perforation of the bone. This opening corresponded to the depression on the surface of the brain already mentioned. It seemed probable that the matter issuing from the orbit into the cavity of the cranium had caused that depression, and that the matter had been confined there for a time, but, accumulating in quantity, had spread itself out over the surface of the brain, as has already been mentioned.

The orbit, besides containing a considerable quantity of pus, was filled behind, and to the nasal side of the eye-ball, with a tumor of a whitish-yellow colour and rather more than cheesy consistence, which, along with the collection of purulent matter, had protruded the eye-ball forwards and towards the temple. The muscles were pressed in the same direction.

The tumor, which had the usual appearance of antral polypus, filled completely the antrum Highmorianum, the bony partition between this cavity and the orbit being destroyed.

The posterior part of the nasal wall of the orbit, and the greater part of the ethmoid bone, were also reduced to carious fragments, mixed with thick offensive pus.

This case was considered by my colleagues and myself as one of orbital abscess; but the dissection proves it to have been originally one of antral polypus, the orbital disease being merely secondary, as well as the inflammation of the brain, of which the patient died. The circumstances which led us to consider it as an orbital abscess simply, were—1st, the external appearances; 2d, the discharge of pus when the swelling was punctured; and 3d, the total absence of any enlargement of the cheek, or any external dilatation of the antrum. The only circumstance upon which, perhaps, we did not lay sufficient stress in forming our diagnosis, was the state of the nostril.

Polypous tumors of the antrum are, it is well known, generally as insidious in their progress as they are destructive. Their growth being slow, so little pain or uneasiness may be occasioned in the part, that the attention even of the patient is only first directed to the disease when, from the gradual dilatation or destruction of the bony parietes of the sinus, considerable external deformation of the countenance has been produced, or the tumor, by encroaching on some one of the neighbouring cavities, has given rise to symptoms too urgent to be overlooked. In the case before us the tumor first pressed upon the nostril, and then, producing absorption of the upper wall of the sinus, made its way into the orbit. Here inflammation was set up, ending in suppuration, which both pointed outwards through the lids, and affected the roof of the orbit, ending in caries of the orbital process, perforation of the dura mater, and extensive inflammation of the tunica

arachnoidea. It was not so much the simple increase of the polypus, as the secondary disorders which it excited in the orbit and within the cranium, which proved destructive in this case.

The usual course of antral polypus is different. Here, for instance, is a wax-model of a fatal case which occurred some years ago in the Royal Infirmary, under the care of Dr. Auchencloss. Here we have great external dilatation of the antrum, with little or no displacement of the eye. In this case, the floor of the antrum, as well as its roof, was completely absorbed, but there was no matter in the orbit, nor any appearance of inflammation within the cavity of the cranium. The external wall of the antrum was expanded into the enormous swelling here represented. In Anderson we have no external dilatation of the antrum; the tumor makes its way up into the orbit, causes suppuration there, displaces the eye-ball, and fatally affects the brain.

CHILDREN'S HOSPITAL AT PARIS.

Case I.—*Laryngitis Stridula.*

A girl, named Godefroy, aged five, was admitted on the 15th of April, and placed in No. 3 of St. Anne's ward. She was labouring under inflammation of the eye-lid, which yielded to the application of four lecches, applied at twice to the right temple, and a lenitive collyrium. She was nearly cured, when, on the evening of the 1st of May, she was attacked with feverishness, which lasted till the 8th of May, together with some cough. The little patient then put her hand in front of the trachea, where she said she was in pain; at the same time there was frequent and stridulous cough, and her voice was feeble and altered in tone. No false membrane could be seen on looking into the pharynx, nor was there any thing in the expectoration, which was not copious. Two cupping glasses (with scarificators) were applied to the part in pain; and eight grains of ipecacuanha were ordered to be given immediately, but were omitted by mistake.

May 9th.—Skin very hot; pulse 96; the cough still of the same character; the voice growing feebler and feebler. No suffocation; permanent pain in front of the larynx; respiration good over the whole chest.

Eight grains of Ipecacuanha to be taken immediately. Milk.

10th.—She vomited yesterday, but there was neither false membrane nor any thing else remarkable in the matters brought up. Towards evening, the breathing being more laborious than usual, three leeches were applied to the lower part of the trachea. To-day the pulse is 112; the skin very hot; respiration hoarse, especially during inspiration; some opaque mucous sputa; stridulous cough; voice as weak as yesterday. The prescriptions were mallow ptisan sweetened; gum julep, with a grain of kermes mineral; and the lowest diet.

11th. — All the symptoms much relieved; no fever; medicine and diet the same as yesterday, with the addition of a little milk.

12th and 14th.—The cough is less frequent, and has lost its previous sound. No pain in front of the larynx; voice stronger. The dose of kermes was reduced to half a grain, and the patient was allowed soup and broth. On the following days the improvement continued; the voice regained its regular tone, and there was but little cough.

On the 25th of May Godefroy was quite cured, and M. Guersant advised her parents to take her from the hospital. There was this point in the case, that when the disease was at the worst it might be asked whether it was merely laryngitis, or whether it was croup? According to authors, the latter is known by the voice and cough, which they call croupy (and which resemble various sounds), and by the expectoration of false membranes. This last characteristic is certainly the most important one; and according to M. Andral, in real croup there is neither voice nor cough. In this patient there was a cough, but altered in its tone; and the voice, though growing more and more feeble, was never entirely lost. Moreover, there were no false membranes; so that these symptoms, combined with the local pain, justified the diagnosis which was given.

The treatment was equally suited to laryngitis and to croup. Supposing, in fact, that false membranes had existed, and had not been coughed up, the emetics which were administered might very probably have discharged them.

Case II. — *Stomatitis, with formations of False Membranes.*

A girl, aged 7, born at Villiers, in the department of Loire and Cher, was admitted into the hospital on the 25th of April. Her father said that he had had the same disease as his daughter, and that it is very common in their department.

For some days the child had had several whitish spots, as large as a lentil, on the mucous membrane lining the cheeks, particularly on the right side, and behind the under lip. These whitish points were

films, which came off on rubbing with the tongue, and when detached, exposed an ulcerated and bleeding surface. The gums were red, swelled, bleeding, and painful; the tongue had a greyish dirty coat, and there were several decayed teeth. The patient's appetite was not diminished.

Such was her state on the 1st of May, when we saw her for the first time. M. Guersant ordered the white points to be touched with a pencil dipped in pure muriatic acid. The disease remained stationary for several days, the false membranes neither increasing nor diminishing in number. M. Guersant suspecting, and perhaps with justice, that the acid employed was of bad quality, ordered sulphuric acid to be used instead; but the result was not more favourable.

After this treatment had been continued without advantage for a fortnight, he had recourse to a gargle composed of decoction of bark, the caustic application being still continued. Under the influence of this new medicine considerable improvement took place; the false membranes became fewer, and the gums firmer, losing their redness and their swelling. On the 25th of May there were three or four false membranes still; and for these the sulphuric acid and the decoction of bark were still used.

[It is curious, that in the original the last words of the heading of this case are *amélioration douteuse*; whereas, in the case itself, it is stated that the effect of the decoction of bark was an *amélioration notable*. The state of the patient's general health, and the diet employed, are not mentioned. Ought not tonics and a generous diet to have been used; and was not this a case where, from the constitutional origin of a local disease, general remedies were of more importance than local ones?—*Translator.*]

Case III.—*Purpura Hæmorrhagica accompanied by Hematemesis, probably arising from an attempt at the establishment of the Catamenia.*

A girl, aged 14, of a strong constitution, of habitually good health, and having a good colour, was admitted on the 9th of May, 1838. The patient had not yet menstruated. She had suffered from general uneasiness for a week, and a feeling as if she was bent; and the disorder began with a sore throat, which lasted four or five days. Two or three days before her admission she began to have pain in the loins, and headache; at the same time there were attacks of heat and slight shiverings, followed by colic and vomiting. The matters brought up consisted of food and blood; the vomitings, which occurred four or five times a day, continued till the 9th of May. After that period there was acute pain at the epigastrium and umbilicus, increased by pressure; and constipation for twenty-four hours: the fæces had been hard, and did not contain blood. There were considerable thirst; the tongue was whitish above, but red at the point and edges. Respiration was regular over the whole chest; the pulse was frequent and full; no palpitation. The patient has been feverish since the disorder began. There was nothing out of order in the genito-urinary apparatus, nor in the organs of relation. The patient's abode was suitable, and her diet wholesome. Before her admission, twelve leeches had been applied to the groins and hypogastrium; the patient had lost a great deal of blood, and, when admitted, was much weakened.

Besides these symptoms, since the 6th of May there have been a great number of spots spread over the inside of the legs and the lower part of the thighs. These spots were red when they first appeared, but to-day, May 10th, some of them are very pale, while others are of a deeper colour, and even yellow. They do not disappear when pressed. They are irregularly rounded, and vary from the size of a flea-bite to that of a franc piece, or more. Around the instep there are several very small ones, of a pale yellow at their circumference, and a more or less deep red in the centre. Hæmorrhage therefore has taken place only from the stomach and the skin. The prescriptions were, two jugs of lemonade, an emollient clyster, the hip-bath, and the lowest diet.

May 11th.—Yesterday evening she had sharp pains in the abdomen and epigastrium; she vomited black matter, (consisting of blood, which could be distinguished by the clots), her drink, and mucus, the whole amounting to two quarts of fluid. This morning the abdomen is very sensitive when touched; the hypogastrium and the groins are painful on pressure; no fresh spots have appeared on the skin. The breathing is short and hasty; no stools; considerable thirst; action of the heart strong; pulse 80; slight headache; face red.

She was ordered to have pure water for drink; an ounce of the oily clyster, and afterwards two ounces of the clyster made with mercurialis honey*; a poultice to the abdomen; a whole bath; eight leeches to the anus; and the lowest diet.

May 12th.—Better after the bath and the leech-bleeding, which was copious. No vomiting, and hardly any pain in the abdomen; pulse 66; four stools yester-

* This remedy is directed in the Codex to be composed of equal parts of honey and of the juice of the Mercurialis annua, or French mercury, a plant which enjoys the reputation of being an emmenagogue.—*Translator's Note.*

day; the hæmorrhagic spots grow paler and paler; the patient has a good appetite. A ptisan of mallow sweetened, a clyster, and milk, were ordered.

13th and 14th.—The patient is quite well; there is no vomiting, and the spots have disappeared, except the deepest and broadest ones, which are still visible. The patient was taken away by her parents in the course of the day.

In this case there was a tendency to hæmorrhage, which was displayed in the skin by the purpura, and in the stomach by the vomiting of blood. These spontaneous fluxes of blood must be referred to the efforts made by the constitution of the girl to establish the catamenia. We must recollect that she was fourteen years of age, strong, and of a good complexion; that she had had pain in the loins and the hypogastrium, with sudden heats in the face, headache, a general feeling of lassitude, and a quick pulse. Now all these symptoms are usual before the first appearance of the catamenia. The discharge did not appear, it is true; but was not its place supplied by the hæmorrhage in other parts of the body?—*Gazette des Hôpitaux*, June 2nd, 1838.

DEATH OF DR. THRESHIE.

Died, on the 20th April last, of fever, off the old Calibar River, on the coast of Africa, Doctor Alexander Threshie, surgeon of H.M.S. Viper; and in a few days, also, after his successor had joined, he too fell a victim to the same disease; so that within a few months, we understand, ten of the most efficient and finest young men, as medical officers, in the country's service, have fallen a sacrifice to the baneful effects of that terrible coast. Dr. Threshie's many amiable qualities, his gentlemanly deportment, his gentle prepossessing manners, added to his high professional attainments, will long endear his memory to those friends who knew him best, and therefore appreciated his many good qualities.

Dr. Threshie was a native of Dumfries, an alumnus of the University of Glasgow, where he took his doctor's degrees in 1832. In that year he was first appointed assistant-surgeon to Hasler Hospital, where he served twelve months: he was afterwards assistant-surgeon of the Thunderer, 84 guns, in the Mediterranean, for upwards of three years; and he had served about a year on the coast of Africa, in the Viper, where he perished as before stated. By his parents, the writer of this short record well knows, his death will be most acutely felt and deeply deplored.

A. C. H.

BOOKS RECEIVED FOR REVIEW.

Cyclopædia of Anatomy and Physiology; containing—Gland—Glosso-pharyngeal Nerve—Glutæal Region—Hæmatosine—Hand—Hearing, organ of. Illustrated with numerous engravings. Sherwood and Co.

Counter-Irritation: its Principles and Practice illustrated by one hundred Cases of the most painful and important diseases effectually cured by external Applications. By A. B. Granville, M.D, F.R.S., &c.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, August 2.

William Goldsmith Rudland, Dartmouth.—Charles Watson, Cambridge.—John Chapman.—William Roderick, Pontesbury.—Joseph Jowett.—John William Smith Coward.—William Henry Acret.—Charles Meymott, Ludlow.—Charles Brown Parkinson, Northampton.—Thomas Stainthorpe, Hexham.—William Henry Hilliard.—Charles Gabriel Jackson Washrough, Bristol.—Frederick Holt.—William Thomas Delph.

WEEKLY ACCOUNT OF BURIALS.

From Bills of Mortality, *Aug.* 14, 1838.

Age and Debility	17	Fever, Typhus	3
Apoplexy	2	Hæmorrhage	1
Asthma	2	Heart, diseased	4
Cancer	1	Hooping Cough	1
Childbirth	5	Inflammation	9
Consumption	32	Bowels & Stomach	6
Convulsions	24	Insanity	2
Croup	2	Miscarriage	1
Dentition or Teething	2	Mortification	1
Dropsy	8	Small-pox	13
Dropsy in the Brain	3	Thrush	1
Dropsy in the Chest	1	Tumor	1
Epilepsy	1	Unknown Causes	63
Fever	10		
Fever, Scarlet	3	Casualties	5

Decrease of Burials, as compared with the preceding week . . . 16

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

August.	Thermometer.		Barometer.	
Thursday . 9	from 45	to 68	30·10	to 30 13
Friday . . 10	56	73	29·98	29 98
Saturday . 11	61	75	29·98	29·99
Sunday . . 12	58	75	30·02	Stat.
Monday . . 13	60	73	29·99	30 04
Tuesday . . 14	43	74	30·12	30·09
Wednesday 15	50	67	30 05	30 07

Wind, S.W. and N.W.

Generally clear, except the 9th, 10th, and 12th.

Charles Henry Adams.

ERRATUM.

In the first sentence of L. C.'s letter in our last, "but like all partizanship it has occasionally *been* gone too far," dele the word "been."

Wilson & Son, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, AUGUST 25, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXVI.

Functional Disorders of the Heart.—Increased or inordinate Action.—Causes, with Illustrations.—Pathological Effects of Increased Action.—General Symptoms; Palpitation.—Physical Signs of increased Action.—Irregular Action of the Heart.—Irregularities in Rhythm; in Strength.—Signs and Causes of Irregularity.—Defective Action of the Heart.—Syncope; its Causes.—Symptoms and Signs of Syncope.—Habitual Weakness of the Heart.—Its Causes and Effects.—Increased Sensibility of the Heart.—Phenomena and Causes of Angina Pectoris.

Having said as much as our time would allow, on the heart, its vital and physical properties and phenomena, and the modes of examining them, we now come to its special pathology—to trace the modes in which its functions and structures may be affected by particular kinds of disease, and the phenomena that it may present under these affections. In doing this, we need not adopt the same order as in diseases of the organs of respiration. These organs are more complicated, and their functions are constituted by so many parts (the nervous system, the vascular system and its blood, and the mechanism of respiration), that their functional diseases are often less simple than the inflammatory, which, therefore, we first considered. The function of the heart, however, is so complete in itself, and its mode of action so determinate, that we may well introduce its diseases by a notice of the disorders of its function, which may be independent of inflammation or change of structure, although these are not unfrequently their causes.

Functional disorders of the heart may be divided into those of *increased* action, those of *irregular*, and those of *defective* action.

The action of the heart may be increased by three classes of causes. 1. An undue irritation by its proper stimulus, the blood.

2. Extraneous irritations, acting either mechanically or through the nerves.

3. Increased irritability of the heart itself.

1. The blood may become unusually stimulating, from its quantity, or from its quality. The increased and quickened action of the heart resulting from exercise is caused by the greater quantity of blood that is returned to the organ in a given time, chiefly by the pressure of the muscles on the veins. This acceleration ceases as soon as the blood returns only in its usual quantity. But the increased quantity may be permanent; as when there is an excess of blood in the system; the increased action may then be either in frequency or in force, or in both, and is reducible by blood-letting. But this plethora may be only local, and confined to the heart and other internal organs; as when, from a contraction of the superficial vessels under the influence of cold or sudden mental emotion, the blood is accumulated inwardly; or as when pressure on the great vessels, impeded respiration, or other cause of obstructed circulation, interferes with the flow of blood from the heart.

Any of these circumstances may excite the heart to inordinate action, which may begin suddenly, continue until they be relieved, and even afterwards, if the heart be in itself irritable, and cease either suddenly or gradually. Again, there can be no doubt that the quality of the blood may, under some circumstances, become unusually stimulating. The injection of diluted ammonia or spirit into the veins of animals is immediately followed by an increase of the heart's action; and it is reasonable to suppose that a similar effect from the ingestion of stimulating liquids is in part owing to the same cause. It is highly probable, too, that the accelerated pulse of several constitutional affections in which the blood is known to be altered, such as gout, rheumatism, urinary disorders, and certain fevers, is, in part at least, due to the stimulating quality of the blood.

2. It is very common for the heart to be excited by extraneous causes. The pressure of an effusion or tumor in the chest, a distended stomach, or other abdominal tumor, may quicken its action by their mechanical irritation. But the more frequent irritations are those of sympathy through the nerves, especially the par vagum and great sympathetic. Through these channels strong mental emotions, acidity, flatus, or improper food in the stomach, a congested state of the liver, feculent accumulations or worms in the intestines, or disordered uterine function, become common causes of sympathetic irritation of the heart. To this class may be added all fevers, and those inflammations of any part of the body which are attended with increased action of the heart. In many of these, the quality and quantity of the blood may also contribute to the irritation, by a direct effect on the heart; and this organ may likewise sometimes acquire increased irritability, which conspires to quicken its action; but the earlier and more general cause of the excitement of the heart is, most probably, sympathetic. In all these instances the increased action of the heart is but a symptom; and although a very important one, as indicating the amount of the irritation, and tending to spread its effects throughout the system, it can scarcely be considered as even a functional disease of that organ, which it leaves healthy and uninjured, only perhaps weaker from the fatigue of long excitement, as it would have been from the excitement of exercise.

3. Although, however, the inordinate action of the heart induced by the two classes of causes to which I have adverted may be considered as only symptomatic, and secondary to conditions foreign to the heart, yet it puts to a trial the capacities of the organ, and if there be any defect in its power or in its structure, it is under the circumstance of unusual excitement that this is likely to be detected. Hence a heart that is unnaturally irritable, whether from functional or structural causes, is excited by any of these sources of irritation more readily and strongly than one in a natural condition; and it not unfrequently happens that it is only by their additional influence that the extraordinary irritability becomes manifest. This consideration has introduced to us the third class of cases of increased action of the heart—those in which its irritability is increased, and which are, more properly than those hitherto noticed, of the number of cardiac diseases. The irritability of the heart, as of other organs, may be exalted by two conditions which are developed in very different circumstances: one may be called nervous irritability, because it is associated with other phenomena of a nervous character, in nervous, hysteric, or epileptic subjects, and in those in whom a train of nervous symptoms have been developed in consequence of excessive blood-letting, or inanition of any kind, which tends to give an undue preponderance to the nervous function. The other condition of the heart which increases its irritability belongs more properly to the vascular function, being that which accompanies the various degrees of vascular distension or the inflammation of its tissues.

These several circumstances inducing increased action of the heart may occur separately, but they more commonly occur combined, or they lead to one another. Thus the heart, when long irritated unduly, often acquires an increased irritability; whilst at the same time being, like other muscles, fatigued by the excess of exertion, its contractions are weak although frequent. You may find an example of this in the effect of excessive or long-continued exertion: a smallness and frequency of the pulse may continue for a long time, and contribute to the protracted weakness and prostration which great fatigue so often induces in delicate frames. Again, long continued irritation of the heart by plethora or congestion is very apt to induce a more permanent morbid condition of the organ: thus the increased action arising from continued distension of the heart with blood is calculated to modify its irritability, and especially disposes the tissues to change. Such a plethoric or internally congestive state is a common forerunner of gout and acute rheumatism, and exists in other instances in which the excretions are defective or depraved. I have met with the signs of it in several cases where the secretions of the liver and of the kidneys were at fault;

how far secondarily, I cannot take upon myself to say, but the distended size of the heart as measured by percussion, and its increased action, sometimes breaking into irregularity or palpitation, continued until the internal plethora was reduced, temporarily by blood-letting, and permanently by a restoration of the due quantity and quality of the excretions. It is especially where the vascular functions are chiefly disordered, even independently of inflammation, that excitement of the heart is likely to lead to more permanent disease either of its properties or of its structure. I have known the heart to continue more or less excited, sometimes to an excessive degree, for many days in succession, nay for weeks, from merely nervous causes, without any permanent mischief ensuing. I remember an hysterical girl, whose pulse was from 120 to 160 for nearly three weeks: she complained of no other inconvenience than weakness, short breath, and a feeling of *flurry;* and the pulse gradually subsided to a perfectly healthy standard of about 80. But when this excitement is more peculiarly that of the vessels, and even when it is of the mixed character which is presented by many kinds of prolonged febrile irritation, the heart continues to be irritable and weak for a considerable time, and if it do not shew some permanent derangement of its properties or structure, its defective power becomes manifest through the period of tedious convalescence which generally ensues. I sometimes meet with patients who complain of the tardiness of their recovery from a fever or some other severe illness; the pulse is quick, but weak, and easily accelerated; the sound of the heart is loud, but brief, and its size, as determined by percussion, is large; it has not the strength to contract fully on its contents, and unless these be diminished, or its strength be improved, some ulterior consequences of its imperfect function, and a consequently defective circulation, may ensue. Whether evacuants for the former purpose, or tonics for the latter, or both combined, shall be best suited to remedy the evil, will depend on other features of the case, and the condition of other organs, which must all have a due share of attention; but I now adduce the case as an example of another origin of cardiac disorder.

You may perceive that a comprehensive view of the functional pathology of the heart would embrace the numberless cases of disease in which its action is modified, which would form a large proportion of any nosologieal catalogue; but this would be far beyond the purpose and limits of these lectures, and I have adverted to this bearing of the subject only to remind you of the very numerous relations of this most important organ, and the various modes in which these relations may become the channels of its idiopathic as well as of its sympathetic affections. The study of the causes of functional disorders of the heart is further important in relation to its structural lesions; for the most distressing and dangerous effects of these often depend on the extent to which the action of the heart becomes disordered by additional functional disturbance, rather than by the amount of the permanent structural lesion. Hence slight organic disease in an irritable heart, or one readily disturbed by sympathetic relations, will be more severely felt than a more considerable lesion in one whose properties are less excitable. We shall have occasion to advert to this again, but we must now examine a little further the phenomena of functional disease.

The general symptoms of inordinate action of the heart are commonly either *palpitation*, which is the action of the heart perceptible to the patient, and is sometimes very distressing; or a feeling of fluttering, agitation, or anxiety, within the chest, without any distinct throbbing. There is frequently greatly increased action, without the patient being conscious of any palpitation; the feeling complained of is rather breathlessness, although the affection of the breath is only secondary. It is great violence, rather than frequency of the pulsations, that renders them perceptible to the patient: and if we take this character as constituting palpitation, we may, contrary to the statement of Laennce, have palpitation with a slow pulse. In fact, the feeling of palpitation may be induced by augmented sensibility of the heart or of the adjoining parts, when there is very little increase of action; and many sensitive and nervous persons can always feel the motions of the heart independently of any inordinate action. The feeling of palpitation cannot, therefore, be taken as a general measure of the action of the heart, but rather of its sensibility. When the sensibility is great, there may be pain with the palpitation; and this is sometimes very acute, being of the lancinating, constricting, or burning kind, shooting to the back, left shoulder, and arm; which is called *angina*. Sometimes it is duller, and more like the feeling of soreness. The pulses of an over-active heart are sometimes felt in some of the chief arteries more than in the organ itself: this is especially the case in the carotid and vertebral arteries, in the arch and descent of the aorta; hence the throbbing in the head, neck, at the top of the sternum, and in the epigastrium, which is sometimes as distressing as palpitation. We

often observe that the chief force of inordinate action of the heart seems to be expended in the first parts of the arterial tube, and does not reach the extreme branches, the pulses in which are not unfrequently weaker than usual. For the same reason also, whilst the head is hot and the face flushed, the extremities are often cold, or if they retain heat, they are dry and unperspiring. Hence, too, if the inordinate action be prolonged, may arise disorders of the secreting and nutrient functions: the irregular distribution of blood overloading the vessels of some parts, and imperfectly supplying those of others.

On physically examining an excited heart, we find the impulse and sounds much increased, as after violent exertion. We feel it abruptly and forcibly knocking against the ribs; but if it be not enlarged, we can perceive that its stroke is short and circumscribed, like that of a moderate-sized body, and we find on percussion that the extent of dulness is as limited as usual. The first sound is shorter than usual, and so loud that it can often be heard without the actual contact of the ear or the stethoscope with the chest. The second sound is sometimes also loud; but if the pulsations be very rapid, which they sometimes are by double or treble their usual rate, the second sound becomes indistinct and a mere appendix to the first; the quantity of blood thrown into the arteries at each pulse being insufficient to make their valves flap freely and with force. When the action of the heart is increased in force more than in frequency, which occasionally happens in excitement from plethora and sthenic conditions of the vascular system, the impulse is strong, but less abrupt; the first sound longer, and the second sound louder, than where the pulsations are frequent. These characters are also permanently found in the pulsations of some enlarged hearts, but then there is more dulness than usual on percussion in the region of the organ. In violent palpitation, the enlarged pulsations of the arch of the aorta may be felt and heard loudly at the top of the sternum, those of the subclavians below the clavicles, and of the carotids in the neck, where also the jugular veins are often seen pulsating, from the retropulsive action of the right ventricle.

When the inordinate action of the heart is excited by transient causes, its duration may be temporary, constituting what are called fits of palpitation; and the heart, when permanently diseased, is frequently worked into these fits by slight additional irritations. They are often most painful and distressing; being attended with feelings of oppression, of extreme faintness, of indescribable agitation, &c.; sometimes of severe anginal pain, cutting or tightening across the chest, and sometimes shooting to the back, neck, and arms. They are in some cases aggravated by fits of spasmodic asthma, of which they may be either the cause or the effect. These formidable symptoms belong chiefly, but not exclusively, to organic diseases of the heart.

When inordinate action of the heart depends on inflammation increasing the irritability of its tissues, it is more permanent in its duration, partaking of the course of the inflammation. But as the inflammation does not affect all parts of the heart alike, some may become more irritable than others, and contracting more abruptly, or with greater force, may modify the character of the systole and its sound. This happens especially when the irregular irritability includes the muscular pillars which regulate the motions of the mitral valve. If these act too suddenly, but perfectly, the flapping commencement of the first sound may only be exaggerated; but if they act irregularly, so as not, with the pressure of the blood, to close the orifice of the auricle, the blood will regurgitate through it, causing a blowing murmur, which is generally heard best where the apex is felt beating. This regurgitation, if considerable, may develop another cause of excitement in the obstruction which it causes to the onward progress of the blood: this subject we shall notice under the head of valvular disease.

I think we might well class together the cases of *irregular* action and those of *defective* action of the heart; for in almost all cases of irregularity, if the action be not defective, the effective power is. The irregularity may be either in the *rhythm* or *period* of the pulsations, or in their *strength*, or in *both*. These present great varieties, but I do not consider it profitable to occupy your time by too many distinctions, and I would therefore only notice those that may illustrate the properties of the heart, or suggest some practical inference.

Slight irregularity in the rhythm of the heart is by no means uncommon in persons who are weak or nervous. Thus we often find a pulse now and then occur sooner or later than in the natural order, constituting *reduplications* or *intermissions* of the pulse. Such phenomena are very common in those who are suffering from much bodily fatigue. I have also known such an irregularity produced at will, by assuming a constrained posture or by holding the breath. They may occur quite independently of disease of the heart, although they are also frequent

concomitants of it. An intermittent pulse may be said to be constitutional in some persons; but they generally have other signs of a weak or an easily disordered circulation. When there is no disease of the heart, the sounds of the irregular pulsations are natural, except that those which occur before their time may be shorter and more abrupt, and the retarded ones are stronger and more prolonged than usual, which are the simple results of the different quantities of blood which the contractions have to propel. In the absence of signs of other disease which may cause it, simple irregularity of the pulse may be considered a result of defective tone of the heart and vessels, and may sometimes be taken as an indication for the use of animal food and tonics.

Irregularity in the strength of the heart's pulsations commonly depends on organic disease. It may be more evident in the arterial pulse than it is in the region of the heart, for being there reduced by distance, the weaker beats may be scarcely, or not at all perceptible; and the artery, consequently, often has a pulse much less frequent and regular than the heart. Many of the instances that have been recorded of extremely slow pulse are probably of this kind. I have met with several cases in which the pulse at the wrist was below 30, and irregular, when that of the heart was 60 or 90, and pretty regular as to rhythm, but varying much in strength, so that the strong beats only reached the wrist. But it is more common for the rhythm to be also disordered, so that on listening to the heart, instead of beats at regular intervals, they occur in all varieties of measures, very unequal both in time and strength; and at the wrist the inequality is perhaps still more evident. Amidst all this irregularity, when perhaps no three consecutive pulses are of the same period or strength, there is very often something like an attempt at order, every third, fourth, or fifth beat being stronger, and followed by a longer interval, so that a sort of tattoo measure is more or less kept up. In persons whose pulse only occasionally presents these irregularities, we may sometimes observe another curious phenomenon: the heart which was beating regularly at 60, will suddenly double or treble its number of pulsations, still preserving its regularity at 120 or 180, producing the feelings of flurry, oppression, faintness, or angina, that have already been noticed as attendant on inordinate action. After a while the pulsations may, as suddenly as they quickened, fall back to their slow regular standard. But sometimes, instead of this sudden and complete change of measure, the two rates of pulsation seem to be variously mixed; three or four beats of the quick movement being followed by a strong one of the slow, and a pause, or some other such combination. This is a common kind of the permanently irregular pulse that we meet with in certain forms of organic disease of the heart, particularly those affecting the left auricular valve; and in the tendency to periodicity in the recurrence of the stronger and slower beats, we still see the remains of a natural rhythm, which prevails most in the more tranquil conditions of the circulation. I have known cases, however, in which a pulse habitually irregular became regular during the prevalence of feverish irritation; but the converse is more commonly observed, that the irregularity is increased with excitement. Thus it often happens that pericarditis, supervening on organic diseases of the heart, increases the irregularity, or occasions it where none existed before. In these more transient forms of irregularity there is not the measure or period in the unequal beats which is manifest where the irregularity is more habitual; it would seem more of the varying character of the excitement of a heart that has lost its natural rhythm without having had time to form a new one; whereas in permanent irregularity the heart has, under its circumstances of varying excitement, become endowed with a new and more rapid rhythm, between which and its natural one it is continually fluctuating, in something like a regular order. This is an exemplification of that tendency which the animal body so much exhibits, to retain some order even in the midst of anomalies.

On listening to the pulsations of a heart acting irregularly, you may find a good illustration of the principle that the character of the sounds depends in great measure on the mode of action. Here is the same heart giving in succession a considerable variety of sounds, the quick pulses being short, loud, with little impulse, and without a second sound; and the slower ones every now and then coming in with a longer heavier sound, strong impulse, and with a second sound, whilst the valvular disease that is frequently also present in these cases may add a murmur which modifies some or other of these sounds, according as it may occasion the blood to pass in an unnatural manner. Commonly the short quick pulses, belonging to the high rhythm, prevail the most, the strong pulsations being only occasional; but sometimes in a pulse which is slow and pretty regular, there will be now and then two or more minor quick pulsations, which often do not reach the pulse

at the wrist. These being short, and unaccompanied by a second sound, were supposed by Laennec to be repetitions of the second sound, which he referred to the contraction of the auricles. But they may often be felt in the carotids, and are thus proved to be minor ventricular pulses; in fact, they exactly resemble, and are examples of, the short rapid pulsations of a high rhythm. When this rhythm prevails altogether, to the amount of 160 or 200 in a minute, there is such a vibratory commotion in the region of the heart, that one is almost tempted to suppose that the two ventricles are acting alternately; but this supposition is hardly tenable when we consider that their fibres are in great measure continuous, and not adapted for separate action. In judging of the size or strength of a heart acting irregularly, we must attend chiefly to the strong slow pulsations, which best represent the true capacity and properties of the organ; for the frequent rhythm, with short sound and no impulse, may take place in any kind of heart, large or small, being perhaps produced by a twitching or quivering movement of the fibres, rather than a real and complete systolic action. The strong pulsations also may inform us, by the murmur which may accompany them, as to the condition of the orifices. Lesions of some part of the valvular apparatus of the heart are generally the foundation of permanently irregular action, although its immediate occurrence may be determined by more transitory causes. Of many cases of habitual irregularity in old people, I have never failed to find signs of disease of the orifices or valves to a greater or less extent, although in several instances the constitution had, in a measure, adapted itself to the anomaly, and the individuals suffered only occasionally from shortness of breath, palpitation or fluttering at the heart, faintness, or some similar symptom of inefficient central circulation. But these persons cannot undertake great exertion without suffering much from it; and attacks of catarrh, or such affections, which might be trivial in others, fall heavily on them. I have met with several examples of irregular action of the heart beginning after unusual bodily exertion and mental excitement, chiefly in stout persons, and in those of a gouty habit. In most of these the irregularity subsided in time, after quiet moderate living, and a due action on the secretions; or more rapidly on the appearance of gout in an extremity. In one case the irregularity has become habitual, without, however, material disorder of the general health, although there is evidently a slightly defective action of the left auricular valves. We shall have to notice such cases by and by.

I shall detain you with but few remarks on the subject of *defective* action of the heart, for unless it be accompanied with irregularity, or amount to syncope, it is more commonly a part of general weakness of the whole system than an affection of the heart in particular.

The action of the heart may fail in force or in frequency, or in both; and its failure in either respect, when extreme, produces the phenomena of *syncope* or *fainting*. Some circumstances render the action of the heart very slow, intermittent, and may stop it altogether; such are the influence of some sedative poisons, such as digitalis, tobacco, and some fungi; sudden mental emotions; severe injuries to the nervous system, as by the removal of blood from the brain by change of posture when the vessels are relaxed or ill filled; or by a strong shock to any considerable portion of the body, as by crushing a limb, by a blow upon the epigastrium, or by the ingestion of a large bulk of cold liquid, or of various articles which are noxious from idiosynerasy. These and similar causes appear to act by diminishing the irritability of the heart, so that its contents do not stimulate it as usual. Other causes impair the heart's action rather by withdrawing its proper stimulus, and by diminishing the strength of the contractions, which become very frequent before they fail; this is the case in syncope from hæmorrhage, and from various inflammatory and febrile diseases, especially those of the abdomen, which commonly prove fatal in this way. Syncope of either kind may be induced in cases of diseased heart, and be the mode of their fatal termination, either quite suddenly, or by permitting the formation of a polypus or fibrinous coagulum, the obstruction of which may cause death in a few hours. A great variety of other causes, mental and physical, may produce syncope, in those that are predisposed to it.

The general symptoms of syncope are, feelings of sickness, giddiness, swimming in the head, noise in the ears, indistinct vision, sudden pallidity or change of the countenance, occasionally with quivering of the lip, chattering of the teeth, involuntary groaning, slight convulsion, and ultimately loss of consciousness and apparent death, which may last for some seconds, minutes, or even hours. Restoration is often accompanied by the breaking out of perspiration, eructation of wind, vomiting, slight convulsions, or palpitation; and sensations far more unpleasant than those which preceded the loss of consciousness, and which some have described as rather

agreeable than otherwise. The radial pulse is either very weak or altogether imperceptible during syncope. The sounds of the heart are also commonly feeble, very short, and generally without a second sound; sometimes they are frequent, in other cases very slow, and they are generally irregular or unsteady. I have never met with a case of syncope in which they were inaudible; but we can readily conceive that pulsations may take place with so small a force as to produce no audible sound, and yet to propel the blood enough to preserve life. In some of my experiments on animals, slight contractions recurred for some time after all audible sound had ceased; and in one instance the auricles alone continued to act alternately for several minutes after the ventricles had lost all motion. It is possible that by either of these remnants of power in the central organ of circulation, with the aid of the powers which assist in the capillary vessels, there may be kept up a movement of the blood sufficient to sustain such a low degree of organic life as may resist decomposition, and keep the frame for a time in a condition from which it can be again excited to its proper standard of vitality. This condition of prolonged syncope, or suspended animation, closely resembles the torpid condition of hybernating animals, which I had occasion to notice in a former part of this course.

But the action of the heart may be defective, yet short of the degree which causes the phenomena of syncope. This defective action may be caused by the same circumstances which occasion syncope, in which case it may be of temporary duration; but it may also arise from a more permanent weakness or defective contractile power of the heart. Such a weakness may be the effect of low diet; of long-continued illnesses, especially those attended by much vascular excitement, such as fevers; of long breathing vitiated air; of sedative medicines, such as digitalis, hydrocyanic acid, &c., and, lastly, the heart may be constitutionally weak, from natural deficiency of its size or strength, in proportion to the body through which it has to propel the blood. Under any of these circumstances there will arise more or less of the general effects of imperfect or languid circulation of the blood. Such are, coldness of the extremities, often attended with flushing or a feeling of heat in the head and face; a general feeling of languor, with pains in the back and limbs; shortness of breath, and fainting on slight exertion; œdema of the feet, sick headaches, a loaded tongue, weak digestion, constipated bowels, and a defective or disordered condition of all the secretions. The pulse is weak and small, and so is the impulse in the region of the heart, although it may be abrupt; and the accompanying sound is pretty loud, but short. The action of the heart may be either frequent or slow; but it is most readily excited by exertion or any other cause, to palpitation or other kinds of irregular inordinate action. It is often this palpitation that alone draws attention to the heart; and not unfrequently a lowering treatment has been adopted to reduce this inordinate action, when it has all the while been the result of the mere weakness of the organ, which struggles at a task to which it is unequal. An excitable condition of the nervous system sometimes accompanies this weak state of the circulation, and may variously modify it, increasing the irritability of the heart, but not its power. Such a combination is often met with in chlorotic females, in whom also the defective quality and quantity of the blood form another conspicuous morbid feature. Weakness of the heart not uncommonly manifests its effects in those who have grown rapidly, whether in height or bulk. Young persons who have shot up in stature in a short time without a proportionate increase of strength, sometimes suffer from palpitation and shortness of breath on exertion, and their generally weak pulse, cold hands and feet, with liability to chilblains, and livid redness of the nose, which are effects of languid circulation. So, also, individuals of a very different description sometimes suffer from the same cause; those who have become very fat. The short breath and palpitation which such persons often suffer from, may partly arise from the deposition of fat in the mediastinum and around the heart, encumbering its movements; but in part they also depend on the inadequacy of the heart to the increased weight of matter through which it has to propel the blood. Hence such subjects seldom bear well bleeding, or any other kind of sudden lowering influence, which does not at the same time reduce the bulk of the body. After a while, under favourable circumstances, the heart acquires such an increased size and strength as to enable it to distribute blood more effectually throughout the body; and in proportion as this takes place, the individuals, although as stout as ever, gain more activity and strength of circulation, and no longer suffer in the same degree from palpitation or shortness of breath. And should they suffer a sudden diminution of their bulk, the heart will then be too strong for their bodies, and they will suffer from the symptoms of hypertrophy or enlargement of the organ which we shall have occasion to notice hereafter.

The general symptoms of many organic

diseases of the heart are also those arising from deficient effective action of the organ, although its apparent power may at the same time be very great. It is of great importance to hold this in view in relation to the treatment; for in many cases of such affections, as well as in others in which inordinate action arises from want of power rather than from its excess, more benefit may be derived from a judiciously directed tonic plan, with especial precautions not to overtax the weak organ, than from measures directed against its occasional excitement.

We have not time to give a distinct notice of the exalted sensibility of the nerves of the heart, which causes the phenomena described under the names, *angina pectoris*, *syncope anginosa*, *sternalgia*, &c. Pain in the region of the heart, accompanied by a feeling of faintness, is in truth rather a symptom than a disease; and it may occur in various degrees in various affections of the heart and great vessels, functional as well as organic. The kind of pain, cutting, piercing, tearing, burning, crushing, shooting to the back, arm, and throat, and so forth, as it is described in different cases; the suddenness of its attacks, and the temperament of the individuals in whom it occurs independently of organic disease, evidently refer it to the class of neuralgic pains. I have known it in a very severe form attack a patient who had before been a sufferer from tic douloureux. It is commonly associated with organic lesions, especially, but not exclusively, that affecting the aorta and its valves; but such affections frequently exist without it. So, on the other hand, anginal or neuralgic pains sometimes occur in persons who have no organic disease; thus resembling other forms of nervous disorder, which, although occasionally excited by the irritation of bony deposits, or other permanent lesions, may arise from an excessive sensibility developed by more transient causes. The circumstances which generally determine the immediate attack of a paroxysm of angina are those which excite the heart's action, such as walking quickly up a stair, up a hill, or against a strong wind, strong mental emotions, distension of the stomach, and the like. To avoid these circumstances, and to employ measures which may lower the nervous sensibility of the heart, form the chief indications of treatment; indications which will be the more difficult to fulfil, in proportion as the organs are affected by a permanent disease. We have so little time left for the consideration of diseases of the heart, that I think it best to condense into one final lecture what I have to say on their treatment, which will prevent repetitions and enable me to give more in one comprehensive view.

REPORT

OF THE

LUNATIC ASYLUM OF ABERDEEN,

For the Year ending May 1, 1838.

TABLE I.—*Of the Number of Patients admitted during the Year.*

Number of patients in the asylum, 1st May, 1837		106
Admitted between 1st May 1837, and 1st May, 1838		43
		149
Whereof were considered curable (of the former number)	11	
Ditto ditto (of the latter number)	18	
Total number of supposed curable cases during the year	29	

TABLE II.—*Of the Causes which induced the Disease in the Cases admitted during the Year.*

	Males.	Females.	Total.
Child-birth, and other sources of uterine irritation	0	10	10
Intemperance in the use of ardent spirits,	6	2	8
Domestic disquietude of different kinds	1	6	7
Overstrained mental exertion	2	0	2
Perverted and overstrained devotion	0	4	4
Pecuniary difficulties	1	1	2
From unknown causes	5	5	10
			43

N.B.—In twenty-two of these cases, predisposition to insanity from hereditary taint, or from the patient having had a former attack, was ascertained to have co-operated with the causes specified above.

TABLE III.—*Of the Forms assumed by the Disease in the Cases admitted during the Year.*

	Males.	Females.	Total.
Mania	8	6	14
Monomania, *with excitement*	5	8	3
Monomania, *with depression*	3	11	14
Dementia, with fatuity,	1	4	5
Moral insanity	0	2	2
			43

TABLE IV.—*Of Results.*

Dismissed between 1st May, 1837, and 1st May 1838, cured	16
Ditto ditto, ditto, improved	8
Ditto ditto, ditto, unimproved	1
Dead	8
	33

Total number removed from the Asylum during the year	33
„ remaining in the Asylum, 1st May, 1838.....	116
	149

From the foregoing tables it will be seen that the returns for the past year coincide in many respects with those of former years.

Causes of the Disease.

And, first, I would particularly invite attention to the fact that an hereditary predisposition existed in a large proportion of those admitted—corresponding to the experience of former years—and that in some of them it prevailed to such a degree as to suffice, apparently, to induce the attack, independently of any direct or exciting cause, although more frequently it could be traced to these two sets of causes combined.

Intemperance has been found to be one of the most common of the *exciting* causes among the male patients; and amongst the females, uterine irritation, arising as well from the natural as the diseased state of the organ, such as pregnancy—parturition, and its sequelæ—the first efforts to establish menstruation—the suppression of the catamenia—and their cessation at the critical period of life.

While females are thus rendered very liable to insanity, from the manner in which the whole body, and especially the nervous system, sympathizes with the generative functions, recovery in them is more hopeful, inasmuch as the brain is for the most part but secondarily affected, and, if not injured by the continuance of the disease, generally recovers itself when the irritation of the distant organ is removed. In young females—especially if treated early—insanity very generally ends in recovery; and in such the proportion of cures is fully as high as in any other malady whatever. I have referred more particularly to intemperance and uterine irritation, because of their frequency, but there are many other causes which have a powerful influence in rousing the latent disease into action; and, indeed, it very often happens that several causes, moral and physical, concur towards its production—such, for instance, as disappointments in life, family quarrels, grief, poverty, and the like.

The insanity of females, then, from being more frequently sympathetic than idiopathic, is also more curable than the insanity of males, where it too often happens that palsy, associated with organic changes of an irremediable nature, have affected the brain or its membranes before they are committed to the care of the Asylum. In the Reports of the two foregoing years, allusion was particularly made to the fact of palsy being much more common amongst male than female lunatics, and this is further confirmed by the extensive observations of the French physicians, in their lately-published Reports on the Parisian Asylums.

Forms of the Disease.

A table has been given, exhibiting at one view the forms assumed by the disease in the cases admitted during the year, which may prove interesting in the statistics of insanity. From this it will be observed that mania, or active derangement of all the faculties, occurred in a much greater proportion in the males than in the females, taking into account that the number of the former was to the latter as 15 to 28. Again, it will be seen that monomania, with depression—more generally recognized by the term melancholia—was more common among the females.

In 23 of the cases the disease had gone on previous to admission for a period varying from three months to several years, and in 5 even to complete fatuity. Yet, notwithstanding many unfavourable circumstances, of 29—the number considered to be curable—16, or more than a half, fully recovered, and 8 improved; that is, 24 out of the 29 patients who were submitted to a regular course of treatment have been sent back to their friends, more or less benefited by their residence in the Asylum.

It is deeply to be regretted that so many individuals should be, year after year, allowed to lapse into a state of hopeless insanity, by reason of the procrastinating disposition of friends. Although I have urged this point at some length on former occasions, I cannot too often recur to it, and would repeat, that insanity being subject to the same laws as other diseases, is like them only amenable to the arts of the physician in its early stages; for, by continued excitement, the brain, which constitutes the seat of the disease in question, becomes changed in its structure, and consequently permanently injured, so that, after a certain time, it matters not what methods of treatment are employed, as all will necessarily prove ineffectual, just as would be the case with confirmed consumption, or ossification of the heart.

A form of insanity has been referred to in the tables, under the title of "moral insanity," about which a few remarks may be offered, as possessing great interest in itself, and bearing on the subject of moral responsibility. It has long been remarked that before insanity has so far appeared in an individual as to be shewn by any positive delusion or hallucination, a change of temper and disposition takes

place, evinced in suspicion and distrust of those whom the person was wont to regard with affection, or in some other symptom of moral perversity. Generally this state ends in the individual betraying delusions which proclaim his insanity to all. Not so, however, in every instance, for in some the moral perversity goes on increasing, until it ends, perhaps, in the commission of crime, without any derangement of the intellect being observed; and then, the individual being brought to trial, unless the plea of insanity be sustained, may be condemned to pay the same penalty as another who has committed crime from the usual motives. Much caution must obviously be used in admitting such a plea; but the details of cases which have occurred in this and in other countries leave no doubt of the existence of such a form of the disease. A father, for instance, murders all his children at once, although formerly he regarded them with even unusual affection; he takes no steps to avoid the legal consequences of the deed, but, on the contrary, thinks that he has removed them from the evils of life, or gives some equally strange reason for his conduct, and, at the same time, shews those physical signs of disordered health which attend on mental derangement in other cases, and may also be known to inherit predisposition to insanity; yet, because he evinces no intellectual hallucination, in the common sense of the term, he is regarded in the light of a criminal, and not considered as insane. It seems natural to expect that the moral faculties should be liable to derangement as well as the intellectual, forming, as they do, an equally important part of the mental constitution. And in every asylum there will be found patients who, while they betray hallucinations, are distinguished much more by their propensity to mischief, and their loss of all sense of decency and propriety of conduct, and other symptoms of moral perversity, than by any aberration of their reasoning powers.

Pathology of the Disease, as deduced from the Post-mortem Examinations.

The number of deaths during the year has been under the average. To detail the morbid appearances in each of these cases would be out of place here; yet a few remarks may be offered, arising out of a general survey of the whole, viewed in connexion with the pathological results deduced from the examination of the fatal cases of former years—amounting to between 60 and 70. That state of the brain which accompanies insanity does not appear to be necessarily one of inflammation. In other affections of the same organ, we see many morbid symptoms, of a very marked character, produced by mere determination of blood, or other irregularity of the circulation within the head. And such we believe to be the state of the brain in the *first* stages of insanity; for, on examination of the bodies of those dying early, none of the peculiar products of inflammation will, in general, be found. The arachnoid may be transparent throughout, and the brain itself only slightly congested. Where, however, the disease has lasted for a longer period, it seldom fails to betray appearances indicating *chronic* inflammation, especially a thickened and opaque state of the arachnoid, and a plentiful effusion of serum—a state of the parts which is associated with fatuity, and often also with palsy. In a very few instances only are there any marks of *acute* inflammation having existed during life; and it is of importance to keep this in mind, lest the practitioner should be tempted, from the violence of the delirium and great muscular energy, so often displayed by lunatics, together with the other symptoms of general excitement, to carry depletion to an extent which would induce fatuity, or otherwise permanently injure the patient. The opposite opinions which have been held in regard to the efficacy of blood-letting in this disease may be reconciled in accordance with such a view of the disease; for while benefit will in general be derived from the *moderate* employment of it in the *earlier* stages, profusedly used, either then or afterwards, it aggravates all the symptoms.

In one of the cases, a female, where paralysis was present during life, and where the insanity had existed for a long time, the morbid appearances, as might be expected, were of a very marked character. All the investing membranes appeared to have been the seat of inflammation, the dura mater had contracted morbid adhesions to the skull cap, the arachnoid was opaque throughout, and the pia mater minutely injected; while, on the surface of the brain, and in the ventricles, a copious effusion of serum was found. In this, as in most of our other cases of palsy supervening on insanity, the "*paralysie générale*" of Esquirol, the patient displayed great variety in the intensity of the symptoms, at one time being able to walk pretty steadily, and answering questions with considerable intelligence and clearness of utterance, while, at another, the gait was staggering like that of a drunkard, the speech inarticulate, and the ideas confused. As I imagine that the serous effusion in these cases takes place very gradually—probably some months elapsing before the quantity has accumulated to any great amount—so is the paralysis slow and imperceptible in its approaches. But,

in order fully to account for the *varying intensity* in the symptoms to which allusion has just been made, we must look to an intercurrent pathological condition, which I conceive to consist in an obstructed, or, in other instances, in an excited state of the circulation of the blood through the brain, induced by some of the ordinary causes of disease—such as intemperance in eating or drinking, constipated or otherwise disordered bowels, &c. This condition of the cerebral circulation being often speedily removed for a time, by the action of a brisk purgative, or a moderate bleeding or cupping, the patient returns once more to his usual state, exhibiting only the comparatively slighter traces of palsy; and continues so until, in the course of a few days, weeks, or months, the congested or subacute inflammatory condition of the brain recurs, whereby he is once more reduced to a state of nearly complete palsy, or is quickly carried off in a state of coma, should the inflammation or congestion extend to a large portion of the cerebral substance, or its membranes. In more than one of the fatal cases the close connexion between insanity and a scrofulous constitution was seen; the latter displayed in the existence of sores in various parts of the body, or in the formation of tubercles in the lungs. Insanity occasionally masks the symptoms of other diseases; an instance of which occurred in a patient who died of consumption, with total disorganization of one lung, and yet presented, during life, neither cough, expectoration, nor any of the other symptoms of the disease.

Freedom of Patients from Epidemic Diseases.

It may be recollected that the patients escaped in a remarkable manner from the influenza, which prevailed during the former year; and during the present there has been another, though less striking instance, of the comparative immunity from epidemic diseases enjoyed by the insane; for, while typhus has prevailed to a great extent in the town, no case of it has occurred in the Asylum. Yet it may be here remarked, that certain patients, of weak constitution, who have laboured long under very high excitement, have fallen into a state simulating typhus so closely, that, without possessing a very perfect history of the case from the beginning, it would have been impossible to distinguish it from one of the stages of the former disease—since the low delirium, the subsultus tendinum, the very frequent pulse, and the dry, black, and glazed tongue, with excoriations on the parts of the body exposed to pressure, were present in a marked degree.

The occurrence of such symptoms in maniacal patients confirms, were it necessary, the opinion that many of the peculiar symptoms of typhus are owing to the nervous system being more involved in the morbid action than in other forms of fever. When the state alluded to continues long, the patient sinks, as in fever, from the exhaustion of the vital powers; of which we had an instance in a female patient. When such a fatal termination does not occur, the patient generally recovers the perfect possession of reason, in proportion as the violence of the physical symptoms abate, as was exemplified in three other patients similarly affected.

Employment of the Patients.

At the date of the last report very confident expectations were entertained in regard to the advantages which would follow the acquisition of the new ground, in affording additional employment to the patients in the Asylum. These, fortunately, have been fully realized. Patients who required formerly constant restraint, and were thought incapable of any liberty, have been occupied in the fields; and, in the society of others similarly engaged, have lost that state of mind which rendered them unhappy to themselves and dangerous to others. The utmost order among those employed has been observed, and, although in general associated to the number of thirty, and entrusted with all the common implements of husbandry, no accident has occurred. The patients themselves, instead of viewing the labour as a task, have regarded it as an agreeable occupation. The advantages of this system of treatment are not confined to patients who may be expected to recover their mental powers, but are also conspicuous in those in whom a perfect recovery is not to be expected, but who are thus trained to such habits of regularity and discipline, as to be capable of being restored to their friends, and even, in some instances, usefully employed at home.

In the *large extent* of our ground a principal advantage lies, as there is at all times sufficient employment for the patients, and that not of a uniform kind, but varying with the nature of the crops and the change of the seasons, and thus calculated to *interest* the patients in what is going on. Had it been otherwise, the good derived from active employment at one time would have been neutralized by inactivity at another, or some uninteresting occupation been had recourse to, which the patients would soon perceive tended to no *useful* end. Without regarding the prospect of recovery which this system holds out, every friend of humanity must view it with pleasure, as affording, in the meantime,

some degree of happiness to a class of beings so deserving of sympathy.

But, in order to render this branch of our treatment complete, we require that a course of in-door occupation should be organized for the winter months. Hitherto it has been impossible to effect this fully, from the limited accommodation of the house; but, in the buildings now erecting, ample means will be afforded in the work-shops and sheds which form part of the plan. It is not easy to fix on those employments which are adapted to a considerable number of the inmates of an asylum, drawn as they are from very different ranks of society, and accustomed formerly to very opposite trades; seeing, too, that we must always keep in view that as little risk as possible attend the exercise of them, and that they are such as can be readily taught, if necessary. In accomplishing this object, the suggestions and co-operation of the managers, especially of such of them as are engaged in business, are desirable; and, judging from the interest which they have always taken in promoting the ends of the institution, there can be no doubt of every assistance being readily granted.

It is less difficult to employ the *female* patients, very few of whom are so excited, or so stupid, as to be unable to knit, to spin, to wash, or do some other household work. Of fifty-three, the average number of females in the house during the year, forty-three have been able to join in some of the domestic duties, and in them the benefit of such a system is sooner displayed, as regards recovery.

Amusement of the Patients.

I have spoken above of the advantages of employment to the patients: it is now proper to say a few words in regard to the means of amusing them. The walks which are now being made through the grounds will afford an agreeable promenade to those who, from their rank or sex, cannot join in the field labour; and, by commanding a view of the country around, will tend to divest their minds of those ideas of confinement and restraint which residence in an Asylum naturally excites, and which, if indulged in, are apt to induce a state of irritation very prejudicial to recovery. To the same end it is desirable that the greenhouse should be retained, as it would afford, at all times, an agreeable termination to a walk, and, in winter, when other means of amusement are wanting, to give a very interesting employment, especially to the females of the better class, for whom, in the meantime, it is difficult to find any suitable out-door occupation.

Amusement in the way of reading is also adapted for very many patients. A considerable number of periodicals, therefore, are distributed amongst them, and are perused with avidity. It is to be regretted, however, that the books belonging to the Asylum are so few and of such indifferent quality, and it would be well if, by donation or purchase, they were increased, so as to form a suitable library, and afford another resource against the tedium of winter. With few exceptions, the patients take great care of any books given them, and in this instance, as in every other, seldom abuse confidence when reposed in them.

Religious Instruction.

The advantages of divine service to the insane, both in improving their minds and diverting their attention from subjects connected with their delusions, have been acknowledged in all the Asylums where it has been introduced, and continue to be conspicuously observed in this institution. With some patients no more effectual motive to good behaviour has been found than permission to attend the chapel on Sunday, according to their conduct during the week. It may seem strange to those who are not brought much in contact with the insane, how divine service can be adapted to such individuals. This will be better understood when it is stated that, of the inmates of an Asylum many are subject to a recurrent form of insanity, which leaves them in the intervals of their excitement in a state of mind little different from what they formerly enjoyed. Some again are insane on a single subject, and have their minds free on every other; others are in a subacute state of excitement, with faculties rather sharpened than impaired by their malady; and, lastly, others are approaching convalescence. It must be obvious that those in the states enumerated may derive much benefit from whatever occupies their attention strongly in a beneficial way, and tends to soothe their feelings. Such is the effect of divine service, and whatever tends to increase that effect must be deserving of attention. In this light is the proposal to be regarded of having a building erected, apart from the house, of a church-like exterior, placed in a pleasant situation in the grounds, and thus calculated to call up in the minds of the patients devout and religious feelings. The present chapel has long been too small for the existing number of inmates, and will be out of all proportion to the extended establishment. It would, moreover, as has been before suggested, form an excellent sick ward, being isolated from the other apartments of the house, and well ventilated.

In conclusion, it may be stated, gene-

rally, that in the management of the patients, harsh measures continue to be found quite unnecessary, even if allowable. Where so many individuals are congregated together, a system of order and discipline is required, but to this the patients soon become accustomed, and can then be treated with the utmost kindness. As a proof that such is the impression of the patients themselves, it may be mentioned that no instance of ill-will towards the attendants has been observed in those discharged. On the contrary, in many instances, they have been in the habit of visiting those connected with the institution; and when they have felt symptoms indicating a return of their complaint, have applied for advice, and been not unfrequently relieved, without confinement in the Asylum being again resorted to.

J. Macrobin, M.D.

ON UTERINE HÆMORRHAGE,

WITH PARTICULAR REFERENCE TO A CASE OF PARTIAL PRESENTATION OF THE PLACENTA.

To the Editor of the Medical Gazette.

Sir,

If you consider the accompanying case of sufficient practical importance, I will be obliged to you to insert it in your valuable publication at your earliest convenience.—I am, sir,

Your obedient servant,

John Renton.

Penicuik, August 7th, 1838.

The following case of child-labour seems to me to be deserving of record, not less on account of its favourable termination under most unpromising circumstances, than from the complication and urgency of the symptoms with which it was accompanied. If it tend to confirm or elucidate any of those important points of practice which are at present undetermined and involved in obscurity, and which it is of the greatest consequence should be established by rule, my object in requesting a place for it in the London Medical Gazette will be fully attained. A faithful narrative, and well-selected collection of authenticated cases, afford the best, because they are the only legitimate, grounds upon which the establishment of sound principles and correct practice can be founded. I am well aware of the impropriety of forming precedents, and deducing practical inferences from isolated cases; but the history of all important deviations from the ordinary course of nature, especially in those labours which are attended with immediate danger to the patient, and which involve the practitioner in fearful responsibility, is instructive, as illustrating the utility or inutility of the particular mode of management pursued. On so interesting a subject as that of uterine hæmorrhage it is always desirable to know the peculiar causes on which success, or the want of it, depends — whether existing difficulties have been averted by timely and judicious interference, or whether they have been increased by negligence, inadvertence, or mismanagement. When it is considered that general rules, as laid down in systems of midwifery, may be too refined, and admit only of partial, and therefore of imperfect application, as they are all more or less liable to be controlled and modified by adventitious circumstances, which it is impossible to describe, foresee, or guard against, it seems the more necessary that a fuller and more precise account of every complex case should be published, in order that the method of treatment pursued (especially when conflicting opinions are entertained on the subject), may be presented under every possible contingency of symptoms and data in this way established, by which the comparative merits of opposed practices may be tried. "The true foundation," says Dr. Denman, "upon which the rules of practice should be formed, is the result of a number of individual cases."

At six o'clock, P.M. (Sunday) Nov. 19th, 1837, I was requested by my friend, Mr. Gibson, surgeon, West Linton, to visit Mrs. R—, a lady upwards of 30, delicate, and of relaxed constitution, in labour of her third child. The previous labours had been natural, short, and easy. During the whole period of her present pregnancy she had suffered much from frequent attacks of bilious and acid vomiting, attended with gastrodynia and pyrosis. She was at all times liable to complaints arising from indigestion, particularly in the early months of pregnancy, but the dyspeptic symptoms in place as formerly of abating in violence, as gestation proceeded, continued in a very

aggravated form, and had greatly impaired her strength. She was more than usually anxious about her confinement, both on account of the general debility under which she laboured, and because the uterine action had been preceded, and was ushered in by hæmorrhage. The first appearance of the discharge, which she attributed to lifting up * her youngest child, a very stout boy, occurred about three weeks before. Mr. Gibson was called in early on Friday morning (the 17th) in consequence of another return of hæmorrhage without pain, which was at first mistaken for the escape of the liquor amnii. The quantity of blood lost might be, as nearly as Mr. G. could guess, four or five ounces. In the course of the day she lost nearly as much more. On vaginal examination, he found the os uteri very rigid and slightly dilated, and within it the edge of the placenta. He confined her to bed, applied the plug, and administered an opiate. She continued in this state until Sunday evening, when I was summoned. I found her very much exhausted, her features sunk without paleness of face, and her strength completely worn out with violent paroxysms of sickness and retching. I was informed that her stomach had been so irritable as instantly to have rejected every article of food she had swallowed for the last two days. She had had little or no sleep during that period, being almost constantly distressed with gnawing pains in the abdominal region, proceeding from spasmodic contractions of the uterus.

At my request Mr. G. removed the plug before I examined. His account was substantially correct. The upper part of the vagina contained a number of small clots. The os uteri was opened scarcely to admit two fingers, and its neck was firm and undilatable. Its condition afforded an excellent instance and proof of that particular cause retarding the progress of the labour, by the resistance it occasions to dilatation, which Dr. Hamilton† has described under the name of an undeveloped band of the cervix uteri. The membranes were entire, and the presenting portion contained a barely perceptible quantity of the liquor amnii. Although the fœtal head had not entered the pelvis, it was felt distinctly enough to indicate a natural presentation. About an inch above the rigid cervix a detached portion of placenta was discovered on the anterior side of the uterus, extending in a slanting direction upwards to the right. I kept my finger within this circular band for a considerable time, for the purpose of ascertaining the strength of the uterine action, and the nature of the hæmorrhage. The uterine contractions, which were very irregular, I perceived were confined to the anterior and upper half of the body of the uterus, and scarcely affected the os uteri. As I manually assisted the dilatation of the latter, I found that a number of small firm clots were discharged by this partial action; and what was much more important to know, that there was at the same time an insidious oozing of warm fluid blood.

The pulse was quick and small. Its state, however, could not be relied on to determine that the loss of blood had been either very great or dangerous, as that had been withdrawn gradually and not rapidly from the body; and its weakness admitted of being explained by the presence of other symptoms. These symptoms (the sickness, excessive retching, and faintness without syncope) rendered the impression made on the system by the hæmorrhage more difficult to be ascertained.

In our patient's state of general debility every source of possible danger was to be inquired into. It was not only necessary to examine the state of the pulse and countenance, but, as Mr. Ingleby has remarked, although it imposes on the practitioner "the office of nurse as well as that of accoucheur," it is of the greatest importance that the condition of the clothes and bedding should also be inspected. It was possible, in the recumbent posture to which the body had been so long confined, that a deceptive hæmorrhage might have occurred without any marked or apparent indications. Mr. Ingleby* mentions that he was once called to a case in which "the practitioner, who had been in attendance the whole of the night, was aware of a progressive exhaustion, but unconscious of its cause.

* Dr. Merriman records a case similar in some respects.—Vide Synopsis of Difficult Parturition, Case 2nd, page 274.

† Vide Hamilton's Practical Observations, part i. page 216.

* Vide Ingleby's Treatise on Uterine Hæmorrhage, 1832, page 144.

When I saw the patient, about nine o'cloek in the morning, I was told there had been no hæmorrhage; but on moving her from the spot on which she lay, the bedding was found saturated with blood." The same fearful exhibition was presented on raising our patient from the bed to the erect posture; and I discovered what has been generally remarked, that more clots and fluid blood were discharged in that state, and to such an extent that I soon found it necessary, on the ground of safety, that she should be replaced on her side.

It was obvious that a decided line of practice should be immediately adopted —that the time for watching the symptoms had passed away, and the period for acting had arrived.

I endeavoured, unsuccessfully, to allay abdominal uneasiness by friction over the uterine parietes; the sickness at stomach by an ipecacuanha emetic; and the acidity afterwards by the carbonate of soda, diffusible stimulants, and brandy and water. But the stomach was in too irritable a state to retain them long enough for any benefit to be derived. I regretted this the more, as repeated doses of the infusion of the ergot, the effects of which I was most anxious to try, were also rejected, almost so soon as they were swallowed.

Here was a case of labour, where uterine action was irregular and inefficient, and there was no tendency to its establishment; where there existed, along with constant nausea, a rigid condition of the cervix uteri; where there was partial separation of the placenta with presentation of its edge, accompanied with considerable hæmorrhage, insidiously going on and occurring in a delicate person, worn out by pain and want of food and sleep, and whose constitution could not stand the loss of much blood.

Under such circumstances what was the most proper course to pursue? The liquor amnii was collected in such small quantity, I have mentioned, that it scarcely could be felt through the membranes, which lay in close contact over the head. It occurred to me, that in consequence of the contraction of the cervix uteri upon the head, the descent of the waters from the upper to the lower part of the uterus might be intercepted. I have not unfrequently observed a similar effect to be produced in natural labours, when the head rapidly and easily enters and fills up the superior aperture of the pelvis. When the upper part of the uterus is either distended by the liquor amnii being detained in the way I have described, or when its general volume is enlarged beyond its usual extent, by superabundance of that fluid, the practical fact is undoubted, not only that the pains at the commencement of such labours are unusually fatiguing, and productive of little effect upon the os uteri, and the female is desponding, but that its discharge establishes and accelerates uterine action. In order to ascertain how far my opinion was correct, I introduced my hand into the vagina, and with my two forefingers, which I retained within the os uteri, I raised up the head of the child. The fits of vomiting and paroxysms of pain soon dislodged the liquor amnii from above, and when I found the membranes considerably distended by it, I withdrew my hand. This good effect, however, was counterbalanced by a slight increase of the draining and considerable protrusion of the placenta. After this, however, she began to feel (as in the commencement of the former labours) pains in the back; but as these had little or no effect upon the cervix uteri, and as both more clots and fluid blood were expelled, the only alternative left, in order to prevent the formation of the former and the further discharge of the latter, was to rupture the membranes. I accordingly did this, and was careful that the liquor amnii should be wholly discharged. I effected this by raising the fœtal head above the brim of the pelvis before the accession of each pain, and keeping it in that position during the paroxysms. This plan was the more necessary, that the waters seemed superabundant in quantity. I was convinced that the dilatation of the cervix and os uteri was to be a very tedious process, and one which could not be safely trusted to the unaided efforts of nature. But I was persuaded, in order to accomplish that object, manual assistance could be as efficaciously afforded after as before the liquor amnii was discharged. By removing that fluid there were the chances either that the hæmorrhage would be removed or greatly abated by general uterine contraction, or by the pressure of the child's head on the particular portion of the uterine surface from which

the placenta was separated, and which there were many reasons to suppose was in the vicinity of the cervix. In this expectation I was not disappointed. About twenty minutes elapsed before the uterus began to act so as sensibly to affect the cervix. Its efforts in overcoming the resistance to dilatation, were, I perceived, feeble and altogether unavailing without manual assistance. My whole attention and time were therefore engaged in attaining that object. As "the impotency of labour" proceeded, in a great measure, from the constricted condition of the circular fibres of the cervix uteri, my first endeavour was chiefly directed to overcome that retarding cause. I found, however, in consequence of the slow and irregular return of the pains, that the dilatation went tardily on, notwithstanding I continued assisting every pain. I had no alternative but to excite and renew uterine action from time to time, whenever it flagged, by gentle but persevering efforts in dilating the cervix. During the time the first stage of labour was about being half completed, and which occupied nearly four hours, another part of my operations was directed to the support of the placental margin, (which was gradually descending), by keeping it above the pubes. I need scarcely add, that I had many painful apprehensions about the issue of the case, before the child's head entered and occupied the superior strait, from the increasing discharge of blood. When the head had descended so far, all hæmorrhage ceased; the first stage of labour was completed in less than an hour, and a safe delivery was soon afterwards accomplished. The child was still-born, as generally happens in such cases.

The only other circumstance worthy of notice occurred in the extraction of the placenta. I cannot describe that circumstance better than by quoting the following extract from Mr. Ingleby's excellent treatise, already referred to *. "There is a very peculiar form of retention of the placenta, alluded to by Dr. Ramsbotham, which has several times occurred under my own observation, but is, I find, by no means well understood. I mean the attachment, by morbid organization, of a circumscribed portion of the placenta, usually the upper surface, whilst another portion, the opposite edge and side, is detached and lies over the os uteri, or perhaps projects in an elongated form into the vagina. This will be regulated in some measure by the site to which the placenta has been attached; particularly when it happens to be affixed to the inferior part of the womb, since, on the delivery of the child, the detached portion falls the more directly into the vagina, and the practitioner, so far from thinking of morbid adhesion, actually considers the whole separation as very nearly, if not wholly accomplished. This state, therefore, is really a source of much deception and embarrassment; for as the lower part of the uterus, and sometimes the vagina, is occupied by a detached portion of the placenta, the practitioner most unceremoniously urges its detrusion by means of the funis, and reduces the patient to a state of imminent peril. In consequence of these efforts the bulk of the placenta is torn away from the part which still remains in firm attachment; the os uteri closes upon it, and, unless its removal can be promptly effected, the patient is necessarily exposed to dangers of the most formidable kind."

Mr. Gibson undertook to manage the removal of the placenta, while I endeavoured, by external compression, to effect a good contraction of the uterus. From the diminution in the size of the uterine tumor, I imagined I had succeeded in this; but Mr. G. mentioned, although he felt the placenta at the upper part of the vagina, it was so firmly retained, he was afraid, if he used much more force than he was exerting in the extraction, that a portion might be disrupted. I took his place, and made an effort with the funis to withdraw it. Disappointed in this, I introduced my hand into the uterus, and found a considerable portion of the placental mass adhering to its anterior parietes, about three inches above the cervix; but by far the greater portion was detached, and lying in the way which Mr. Ingleby has described, partly within and partly without, but not retained by the os uteri. I withdrew the whole *en masse*, and administered a large dose of solid opium. We watched the case for two hours, and neither hæmorrhage nor any untoward symptom supervened. The progress of her recovery was very much retarded by a severe attack, on the third

* Vide Treatise on Uterine Hæmorrhage, p. 200.

day after delivery, of pneumonia, and subsequently of phlegmasia dolens. From her extreme exhaustion, Mr. G. despaired of her being able to stand the antiphlogistic treatment he found necessary to adopt for the removal of these complaints. Under his management, however, she was restored to her usual state of health in the course of three or four months.

I have been induced to allude to the particular cause of the detention of the placenta, because it is of rare occurrence, and corroborates the remarks which Mr. Ingleby has made on the dangerous rule laid down by Dr. Gooch, in regard to the force which he recommends for the extraction of the placenta, when it is felt in the upper part of the vagina *.

I have had occasion too often to have recourse to manipulation in the removal of those preternatural attachments by which the placenta is sometimes connected by small portions of its mass, and at other times throughout its whole bulk, to the uterine surface, to be possibly mistaken in this case, as to the nature of the adhesion. Uterine contraction had detached a much larger portion of placenta than had been removed before delivery, as the very different appearances on its surface afterwards indicated; but the remaining portion, which I gradually separated with my fingers, was too intimately connected to be withdrawn by any other method than the one I adopted. Had the cause of detention been misunderstood, and forcible attempts at extraction been made by pulling the funis, or that portion of the placenta which was without the os uteri, a considerable portion must have been left within the womb, and would probably, in the almost exsanguined condition of our patient, have produced fatal effects.

Dr. Burns observes †, that irregular action of the uterine fibres may be spasmodic; a state in which the circular fibres of the cervix are generally affected. On this condition we can explain the "colic or cramp pains," as they have been expressly termed, with which Mrs. R. was so much distressed before the commencement of labour.

But the chief and particular cause which retarded its progress consisted in the undeveloped band of the cervix uteri. At the same time, while such a primary agency is admitted in the production of the inefficient state of uterine action, it would be improper, while we take into account all the circumstances of the case, to overlook the co-operations of those accessory causes which I have described as arising from the superabundance of the liquor amnii*, its confinement above the fœtal head in the upper part of the uterus, and the disordered state of the digestive organs †.

It would be, perhaps, refining too much on causes to assign to each its particular share of power in retarding or counteracting labour; but if I might be permitted doing so, I would say in this case, comparing the pains with the resistance made, that the imperfection and insufficiency of the labour originated in the constriction of the cervix uteri and over-distension of the uterine parietes, and its irregularity in the fits of nausea and sickness.

[To be continued]

* Vide Ingleby's Treatise on Uterine Hæmorrhage, p. 202.

† Vide Burns' Principles of Midwifery, 8th edit. p. 419.

* Dr. Denman has an excellent section on the too great distension of the uterus. Vide Introduction, &c. 7th Edit. p. 223.

† In Burns' Principles, p. 418, an attempt is made to account for this, "by changes in the action or condition of the origin of the nerves supplying the uterus."

‡ Vide Medico-Chir. Trans. vol. xviii. p. 189.

OBSERVATIONS ON NÆVI, THEIR STRUCTURE AND TREATMENT.

By T. B. Curling, Esq.

Assistant-Surgeon London Hospital, Surgeon to the London Dispensary, and Lecturer on Morbid Anatomy.

[Concluded from page 797.]

The treatment of nævus, like that of most diseases not clearly understood, has been very various; and the number of modes recommended for its removal affords the best evidence of the unsettled nature of the practice relating to it, if not of the imperfections of the means employed. A principal object in this paper is to call attention to the treatment by seton, a plan originally suggested and practised by Mr. Fawdington, and recommended a few years back by Mr. Macilwain, in a paper ‡ detailing two cases of deep-seated nævus successfully treated in this way, which does not appear to be so fre-

quently resorted to, and to have obtained that preference which, in my judgment, its merits and efficiency justly entitle it to. The following is an account of the first case in which I was induced to adopt this practice.

CASE III.—*Subcutaneous Congenital Nævus occupying the lower part of the leg, and rapidly extending, treated successfully with setons.*

August 10th, 1836.—An infant was brought to the London Hospital within twenty-four hours after birth, with a nævus situated at the posterior and lower part of the right leg. It was in size somewhat larger than a shilling, and was soft, compressible, and of a slightly livid colour. I requested to see the child again in three days. The nævus had then increased to more than double its former size, and encircled full two-thirds of the limb. Seven setons, each consisting of a double thread of silk, were passed by means of a straight sewing needle through the tumor in various places, in the direction of the axis of the leg. On the fourth day after the operation there was but slight inflammation, and the nævus was very little altered in appearance. On the seventh day there appeared to be sufficient inflammation at the outer part of the nævus, and I therefore withdrew one of the ligatures. The child did not appear to suffer in health. On the eleventh day the remainder were removed, when a few drops of pus escaped at the openings made by the needle. From that time the inflammation slowly subsided, suppuration ceased, and in about three weeks more the tumefaction was removed, and every trace of the nævus had completely disappeared.

The detail of many cases in which setons were employed is unnecessary, as they resemble each other in all points of practical importance. I shall therefore only relate one other case, in which, the nævus being of large size, great thickness, and at parts much consolidated, was one apparently ill adapted for any plan of treatment except excision.

CASE IV.—*A Tumor below the Elbow, formed by a large Subcutaneous Nævus, cured with Setons.*

Mary Chipchace, a little girl about nine years of age, of a spare habit, was brought to me, February 10th, on account of a large subcutaneous nævus situated over the ulna, just below the olecranon. It was about the size and thickness of half a large orange, and of so firm a texture that the nature of the tumor was only rendered apparent by a slight blue discoloration occupying part of the skin on its surface. Its bulk was very little reduced by pressure. An elder sister, who came with this patient, informed me that a mark had existed from birth, but that at first it was not larger than a fourpenny-piece. Within the last two or three years it had continued slowly, but uninterruptedly, to increase. It was tender on pressure, but was frequently rendered painful and became inflamed, from the effects of slight blows, to which, from its situation, it was constantly exposed.

February 14th.—I passed, through the base of the tumor, three long needles, armed with two silk ligatures doubled, and having cut off the needles, left the ligatures to act as setons. The first needle employed was one of the largest-sized darning needles I could obtain. In consequence, however, of the consolidated state of part of the vascular tissue that it had to penetrate, I experienced very considerable difficulty in introducing it. For the two other setons, therefore, I used the ordinary three-edged needles for sewing up the dead body, which passed with great facility. No hæmorrhage attended the operation.

17th.—I found that the seton had excited a good deal of inflammation. The tumor was evidently enlarged, the skin around it was red and hot, and the girl was feverish. I did not, however, remove the setons until the following day, when, being of opinion that sufficient inflammation was excited, they were withdrawn. On drawing out the middle one, a few drops of pus escaped. A poultice was applied, and on the next day I thought it prudent, as the inflammation did not appear to be subsiding, and was near the elbow-joint, to apply a few leeches to the arm, and to substitute an evaporating lotion for the poultice. There was likewise a good deal of constitutional disturbance. By appropriate treatment, however, all this subsided in the course of a few days. Suppuration occurred in the centre of the tumor, and, in order to secure a free exit for the matter, I slightly enlarged one of the openings made by the needle.

March 3d.—The size of the tumor was much diminished, and there was only a slight discharge from the interior. The child had lost all febrile symptoms, and was regaining her health. I ordered the sulphate of quinine, and, as the skin was excoriated, a lotion of the nitrate of silver to be applied. From this period the tumor continued slowly to decrease, the discharge ceased, and on the 20th of April the part was completely healed; scarcely any tumefaction, discoloration, or scar, remaining, to indicate either the previous existence of the morbid growth, or the curative efforts of the surgeon.

The introduction of the seton in the treatment of nævus operates by stirring up inflammation, which, being attended with the effusion of lymph, or pus, into the interior of the vessels, occasions the obliteration of the reticular tissue; and this, as the inflammation subsides, is followed by the gradual and slow absorption of the thickened parts. There are several other methods which act in the same way; but I believe that none of them, in an equal degree with the seton, combine the three important objects—certainty in their result, safety in their employment, and freedom from subsequent deformity. Excision is certain in its result, and is a sure and effectual mode of getting rid of the disease; but unless the growth be small it cannot be resorted to with impunity. The experience of Mr. Wardrop, of the dexterous French operator, Roux, and of others, furnishes fatal cases of hæmorrhage from the operation; and when situated on an exposed part of the body, there is the additional objection that it is succeeded by a scar. Cutting off the circulation from the tumor by means of ligatures applied to its base, is likewise a very sure mode of destroying the nævus. It is, however, a most painful and irritating mode of treatment. I recollect well the case of a child with a large nævus on the side of the head, when I was attending as a pupil at the London Hospital, in which this practice was adopted, but so much constitutional disturbance was produced that the patient died in about a week; a result not very surprising, when we reflect that the ligature was applied to the sound and sensitive skin surrounding the nævus, which, from the size of the tumor, was necessarily a considerable circle. The ligature is also followed by the formation of a cicatrix, after the separation of the morbid growth. Tying the carotid artery, to arrest the growth of nævi about the face and head, has so frequently failed, that I conceive the operation is not likely to be soon repeated. In the first case in which Mr. Fawdington had recourse to the seton, the nævus was situated behind the angle of the jaw, and the carotid artery had been previously tied without any satisfactory result. The injection of nævi with stimulating fluids, though a practice which, in the hands of its proposer, Mr. Lloyd, and of others, has been often successful, cannot be viewed as unattended with danger. An instance of sudden death under the operation has recently been recorded * by two surgeons in the country, whose honesty and candour in giving publicity to the case fully merit the thanks of the profession. The fluid injected was dilute liquor ammoniæ, two previous attempts with less stimulating fluids having failed. It might, perhaps, be questioned whether death was really caused by the action of the liquid injected, and the proof would be difficult. The case must, nevertheless, serve as a warning; and considering the free and ready communication between the plexuses of this tissue and large veins, and the rapidity with which poisons act upon their lining membrane, I cannot regard the forcible injection of a nævus with a highly stimulating fluid as altogether free from the risk of the injection reaching some of the larger vessels, and producing a suddenly fatal impression, or subsequently exciting dangerous inflammation of their internal coat. We should scarcely be satisfied in trusting to pressure made around the nævus, to prevent such serious consequences. That injection is uncertain in its result, and often requires to be repeated, is fully admitted. The application of escharotics, as the potassa fusa or strong nitric acid, is only adapted for slight superficial nævi, for which they are excellent remedies. Subcutaneous and large nævi require repeated applications, in order to effect their destruction; and after the separation of the sloughs, granulation takes place, and the part is disfigured by a scar. Inoculation with the vaccine virus, and free acupuncturation, are sufficiently mild in their effects, and free from dan-

* Medical Gazette, vol. xxi. p. 529.

ger; but they very often fail, and cannot be depended on in the treatment of a nævus rapidly extending. When the disease is stationary the milder methods are preferable, and therefore these plans may be fairly tried.

I have thus briefly reviewed the chief and more important of the various plans of treating this disease, in order to place in a clearer light the peculiar advantages of the seton in the treatment of growing nævi. In a tissue so little prone to take on diseased action, it is essential that the treatment adopted should be sufficiently decided to ensure the excitement of inflammatory action, without, at the same time, endangering the life of the patient; and the success of the seton in effecting this object constitutes its chief merit. In Case 3, it has been seen that the progress of a nævus increasing with remarkable rapidity, was at once arrested, and subsequently cured, by the introduction of seven setons. In Case 4, an indolent and formidable tumor yielded to this plan, whereby a cutting operation was avoided, and the cure accomplished with scarcely any scar or deformity. In this instance more local inflammation and constitutional disturbance were produced than I have witnessed upon any other occasion, but the state of the tumor required pretty active inflammation for its obliteration. In infants I have been surprised at the slight irritation which the seton generally gives rise to. In order to excite a requisite degree of inflammation, Mr. Fawdington considers it sometimes necessary, in the course of the treatment, to moisten the silk with some irritating liquid, as a solution of the nitrate of silver or sulphate of copper. I have never had occasion to do this; and in large nævi, the chief point is to take care that a sufficient number of setons are passed. I believe that want of attention to this circumstance is the reason that this mode of treatment has sometimes failed in the hands of other surgeons. The needle should be introduced through the sound skin at the side of the nævus, and, after traversing the morbid growth, should be brought out through the sound skin at the opposite side. In some situations it may be inconvenient, or even impossible, to make use of a straight needle; in such cases the common curved needle may be employed. The operation is never followed by any hæmorrhage that cannot be instantly arrested by slight pressure. The time required for leaving the setons in of course varies according to their effects; but I have seldom found it necessary to allow them to remain longer than a fortnight*.

The introduction of setons is also a proceeding well adapted for the cure of varicose veins of the leg, and is preferable to the various other operative methods which have been proposed. The unfortunate results which have in some instances attended the attempts to procure obliteration of the vena saphena major, have created a feeling unfavourable to operations in these cases: but whatever danger there may be in interfering with the venous trunk, the same does not attend operations on the smaller vessels; and the complete impunity with which we excise and tie diseased hæmorrhoidal veins, and, I may add, the reticular tissue in nævus, is sufficient to shew the little risk incurred in operating on plexuses of varicose veins. After the removal of the setons the obliteration of the venous plexus will be facilitated by the application of pressure†.

OF THE

PATHOLOGY AND MORBID ANATOMY ON VEINS.

By James Copland, M.D. F.R.S. &c.

[Continued from page 800.]

The middle membrane of veins is often softened, generally at the same time with the inner. When this is the case the vessel is torn with great ease. This coat is also subject to atrophy: when this exists the vessel presents an extraordinary degree of tenuity.

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* The mode of curing nævi, practised by M. Lallemand, of Montpelier, is a combination of the seton and pressure. It consists in passing pins into the tumor in various directions, and connecting them with numerous twistings of waxed threads. They are left in a sufficient time to produce the inflammation required. In one instance of a large nævus over the scapula, 120 pins were employed. He sometimes makes an incision into the tumor before introducing the pins, thus making the operation very nearly the same as that for hare-lip. This appears to be a very effectual plan of treating the disease, but I have not had occasion to resort to it.—Vide *Archives Générales de Médecine*, tom. viii. serie 2.

† Errata in preceding portion—p. 794, l. 45, *after* "congestion" *insert* "consequent;" same page, notes must be transposed, *i. e.* "† Works, &c." stand first; p. 795, l. 30, *for* "organs" *read* "growths;" p. 796, l. 5, *before* "20th" *insert* "April;" p. 797, l. 45, *for* "their" *read* "its," l. 47, *for* "them" *read* "it," and l. 48, *for* "they are" *read* "it is."

Hypertrophy of the middle coat has been minutely described by M. Andral. When this change exists, the coat is more distinctly visible, and its longitudinal fibres more evident. When the hypertrophy is considerable it loses its transparency, acquires a yellowish colour, and becomes to a certain degree elastic, so that when cut across, the vein remains open as an artery, to which it closely approaches in appearance. On minute dissection, however, of the middle coat of the hypertrophied vein, it never presents any trace of circular fibres, nor does it possess the same degree of elasticity as the middle coat of arteries. M. Andral states that he once detected, as he believes, muscular fibres in the parietes of the vena cava inferior, near the heart; the cava was greatly hypertrophied. In the horse, the structure of the vena cava, near the right auricle, is evidently muscular. The hypertrophy, in this case, developed a structure similar to the natural condition of the vessel in some of the lower animals.

The middle coat, however, may be much thickened without actual hypertrophy or development of its fibres. This may arise from a deposition of fibrinous lymph in its texture, which becomes solidified by the absorption of its serous portion. M. Andral accounts for this change by supposing that a quantity of blood accumulates in its tissue; that its colouring particles are absorbed; and that the white fibrine remains behind in a solid form, combined, molecule to molecule, with the coats of the vein, presenting the lardaceous appearance described by authors as a particular tissue.

The external coat of the veins is subject to the same changes as have been observed in the external coat of arteries, and which I have described in the article on the latter order of vessels, in my Dictionary.

Ulceration and *perforation* of the several membranes composing the veins are sometimes met with. M. Andral thinks that the latter is more frequently observed than the former. Perforation has been detected in the superior cava, both within and without the pericardium; in the inferior vena cava; in the vena portæ, both within and without the liver; in the splenic vein; in the jugular vein; in the subclavians; in the mesenteric veins, and in the veins of the extremities. The perforation is sometimes met with without any appearance of disease in the vicinity of the coats of the vessel, rupture taking place from external violence or muscular exertion. This form of perforation, or rather rupture, is most frequently observed in the vena cava and veins of the internal viscera. Instances of rupture of large veins are numerous. Schenck, Morgagni, De Haen, Doubleday, Groquier, Lovadina, &c. have recorded cases of rupture of the vena cava. Morgagni also found the pulmonary vein ruptured. In the majority of those cases the rupture was occasioned by exertion or external injury. It is probable, however, that the walls of the ruptured vessel had been previously diseased. In cases of perforation, the vessel is commonly more or less inflamed, ulcerated, softened, atrophied, &c.; and here, as well as in the case of rupture from exertion or violence, the perforation occurs from within outwards. But the perforation may also proceed in a different direction: viz. from without inwards, as when the vein is seated in diseased parts, as in the case of carcinomatous ulcers of the stomach.

The calibre of the veins may likewise be changed, particularly when the veins are altered in the manner described, the change of diameter occurring in the part the parietes of which are diseased.

Dilated or varicose veins present a variety of appearances:—1st, the veins may be simply dilated, either in respect of a whole vein, or of portions of it, without any affection of the capillaries which nourish it, but more frequently with a state of chronic inflammation of its coats: most probably the dilatation is the consequence of inflamatory action, this state disappearing, but the dilatation continuing. 2nd, They may be dilated, either uniformly or at intervals, with thinning of the parietes. 3d, They may be dilated and their coats thickened, either uniformly or at intervals, the vessel being generally also lengthened, and consequently tortuous. 4th, They may be dilated and divided into compartments by the interposition of septa or partitions, between which the blood stagnates or even coagulates. 5th, They may be dilated, divided into compartments, and the dilated portions perforated, so as to allow the blood to pass into the cellular tissue surrounding

the vein. M. Andral thinks that the tumors described by authors under the name of *erectile* are in reality nothing more than a cluster of small veins communicating with one another and with the surrounding cellular texture by the perforations situated as now described. "The cellular tissue," he adds, "situated between the veins thus affected, is liable to various alterations as respects its nutrition and secretion, whence arise the great variety of appearances which these tumors present, the numerous morbid productions that are found in them, the different degenerations which they are said to undergo, their transformation into cancer, &c."

The structure of every variety of hæmorrhoidal tumor may be referred to one or other of the foregoing varieties of dilatation or varix; but these lesions are not confined to the veins in the vicinity of the anus. M. Andral once found the external jugular altered in the manner described in the last variety. In some cases the tumors occasioned by dilated veins disappear spontaneously. When this occurs, the veins are generally obliterated. Some of the above kinds of varix, according to this pathologist, depend on increased activity of the nutritive process; others on diminished activity of this process. More frequently, however, they are the result of pressure exercised on the venous trunks into which the varicose veins pour their blood, or of some obstruction to its return through the former vessels. In this case the varicose veins are not only greatly dilated and elongated, but their parietes are also hypertrophied.

Contraction and *obliteration* of veins are, according to M. Andral, much more common than their dilatation. These lesions are occasioned—1st, by obstructions in the interior of the veins; 2nd, by causes seated in their parietes; and 3rd, by mechanical compression external to them, generally from tumors, &c. Amongst the most frequent sources of the obliteration of the canals of veins is the spontaneous coagulation of the blood within the vein. That the blood may coagulate during the life of the patient, within a vein or portion of a vein, and even within the sinuses of the dura mater, has been proved by the researches of numerous pathologists, particularly by Meckel, Gendrin, Bouillaud, Ribes, Andral, Cruveilhier, Dance, Arnott, Lee, Tonnellé, Reynaud, and others. I have been satisfied of its having occurred in several cases which have come before me in practice. It is often difficult to assign the particular cause of the coagulation. In some cases it is apparently owing to the state of the vital energies of the system, and their effect on the blood. In other cases it is evidently occasioned by the morbid state of the valves already noticed, and to a lesion of the internal parietes of the vessel. Most commonly, however, it proceeds from coagulable lymph or pus secreted from the internal surface of the vessel; the morbid secretion adhering to the inflamed surface producing it entangling the fibrinous and colouring parts of the blood, and thus forming a coagulum which either partially or entirely obstructs the passage. This coagulum generally varies in its colour, density, and firmness of adhesion to the parietes of the vessel; and it generally increases in bulk until the passage is entirely obstructed by it. Occasionally the coagulum becomes in some respects organized. Some of the pathologists whose names I have here adduced, particularly M. Andral, suppose that these coagula live precisely as the veins in which they are formed, and, like every other part endowed with life, keep up a constant process of nutrition and secretion, and are also liable to become diseased.

The canal or cavity of a vein may be altogether obliterated, and the vessel reduced to a fibro-cellular cord. This seems to be effected in a similar manner to that observed in arteries. Obliteration of large venous trunks gives rise, as observed in arteries, to a collateral circulation, kept up by means either of small veins, or of one vessel that has acquired an unusual size. The most remarkable instance of obliteration was observed by M. Reynaud, in the superior vena cava. The veins on the lateral surface of the chest were remarkably enlarged, and anastamosed freely with the epigastric veins, which were also greatly enlarged. The circulation was carried on chiefly by the vena azygos and inferior cava *.

The consequences of obstruction and obliteration of veins, especially their influence in producing dropsy and œdema, are well known. Several instruc-

* Journ. Hebdom., t. ii. p. 110.

tive cases of obliteration of the cavity of the vena cava, iliac veins, and sinuses of the dura mater, are recorded by Bartholinus, Rhodius, Mantissa, Bontius, Albinus, Haller, Morgagni, Hodgson, Breschet, Wilson, Tonnellé, Reynaud, and Andral.

Ossific or *calcareous formations* are rarely met with in the coats of the veins. They have, however, been observed by Morgagni (*De sed. et caus. Morb.* Ep. lxiv. Art. 9), Walter (*Anat. Mus.* i. p. 172), Murray (*Act. Med. Suecic.* i. p. 3), Baillie (*Morb. Anat.*), and Tilorier (*Journ. de Med. Cont.* t. xi. p. 224, Ap.); and more recently by Macartney, Beclard, and Andral, in the femoral and saphena veins. Sometimes these coneretions protrude into the cavity of the vessel, either bursting its internal membrane or carrying this membrane before them. In either case they may at last escape into the cavity, and lodge there, without any attachment to the sides of the vessel; for in the latter case the membrane may contract behind the coneretion, forming at first a peduncle attaching it to the vessel, which at last is ruptured or absorbed. These coneretions have received the name of *phlebolithes*, and vary from the size of a millet-seed to that of a pea. They have sometimes been found in the centres of coagula which had evidently formed around them. They have been observed by Columbus*, Walter†, Soemmering, John‡, Langstaff§, and Fred. Tiedemann‖. The last-named pathologist has described them minutely. Those which he has observed, as well as those noticed by Columbus, John, Langstaff, and Walter, had been formed in the uterine and hæmorrhoidal veins. M. Tiedemann says that they are formed of concentric layers of the phosphate and carbonate of lime, united by albuminous matter, around a minute albuminous clot or coagulum; and denies that they are formed in the way which I have just stated. He contends that they have been produced by a sort of crystallization, or deposition, of the earthy particles contained in the blood, around a nucleus which had formed in the nearly stagnant blood in the veins. M. Andral also considers it possible that they may have been formed in the blood itself. It is difficult to assign limits to the range of possibilities, without relation to the faith of those for whose belief they are adduced; but the question is, whether or no these coneretions have ever once been ascertained to have been formed in this manner. I believe that no such fact has been ever adduced.

Fatty matter, or adipose substance, has been found in the parietes of veins. Bontius met with it in large masses in the vena cava, obstructing its canal. Andral found it between the coats of the vena portæ.

Entozoa have been frequently found in the veins of the lower animals. The Strongylus and Filaria have been detected in the veins of the horse. M. Andral met with individuals of the class Nematoidæa in the vessels of a porpoise; and in one only case he found Acephalocysts (hydatids) in the pulmonary veins.

Gaseous fluids have occasionally been remarked in the veins. In some cases they have proceeded from incipient putrefaction, but in others this could not have been the case. In cases of surgical operations, when large veins are divided, air not unfrequently enters the veins; and if it rush in suddenly, and in considerable quantity, it is soon followed by death, as several fatal operations, in which this has occurred, have shewn. The matter, moreover, has been put to rest by the experiments of MM. Magendie and Piedagnal. From these it appears that death is instantaneously produced by the air, when it is suddenly introduced; but the mode by which it produces this effect is not so manifest. The air seems to act in deranging or destroying the contraction of the heart, and injuring the actions of the lungs.

* De re Anatomica, l. xv.; Franc. 1593.
† Observat. Anat. p. 44.
‡ Sweigger's Journ. &c. t. xii. p. 80.
§ Med.-Chirurg. Trans. vol. viii. p. 272.
‖ Journ. Complem. t. iii. p. 38.

CASE OF CYSTICERCUS CELLULOSÆ BETWEEN THE CONJUNCTIVA AND SCLEROTICA.

To the Editor of the Medical Gazette.

Sir,

As the entozoa of the human subject are engaging the attention of some professional men, the following case may be considered interesting.

A little girl, of six years of age, was brought to the dispensary for the cure of complaints in the eyes, during the last month, with a vesicular tumor, about the size of a moderate pea, beneath the conjunctiva, upon the sclerotica of the right eye, so near to the inner canthus that it was entirely concealed from view, excepting where the eye was turned outwards. It was accompanied by no inflammation, nor productive of any inconvenience; neither mother nor child knew how long it had existed, as it was discovered only a day or two before they came to me.

I opened the tumor freely with a cataract knife, and a thin serous fluid escaped, the distended conjunctiva immediately becoming flaccid. Besides the clear fluid which was let out, I observed between the lids a white flat substance, which I supposed to be a flake of coagulable lymph, or some caseous matter which the tumor had contained; it was, however, of a firm membranous character, and on being immersed in clean water, expanded, and was seen to be a little bag, opened, and having attached to it a white, rather solid body, a line and a half in length, and half a line in breadth: the bag itself might be compared, in point of size, with the skin of a very small currant. The more solid portion appeared to me within the cavity of the bag, but it might have been upon its external surface, the bag being inverted.

On examining the substance with a lens of an inch focus, it was obvious that it was the cyst of an hydatid, the projecting part exactly resembling the head and neck of the cysticercus telæ cellulosæ, as figured in the interesting article upon the Entoza, by Mr. Owen, in the Encyclopædia of Anatomy and Physiology.

While viewing it in water, four discs were observable at the extremity of the head; but when gently compressed between the glasses of an aquatic box, and viewed with a higher power, three of the suctorious discs, as they have been denominated, and another surrounded with hooks, presented themselves to observation.

The rugæ in the neck of the parasite

Description of the Drawing of the Cysticercus Cellulosæ.

1. Natural size of the hydatid.
2. Appearance when viewed with a lens of an inch focus.
3. Head, with a small portion of the neck, seen under a compound microscope, with achromatic object-glass.
4. The disc having the hooks, under a high power.
5. A separate hook.

were very conspicuous. It differed, however, from the figure referred to, in having the head and neck covered with minute oval bodies, probably gemmules. On the head they were not very numerous, but the neck was studded with them; they were not only upon the surface, but within the substance, of the matter forming the neck and head, different ones being brought into the focus as the distance of the object-glass was varied. The size of these gemmules is about the 1,500th of an inch. Not one is to be found upon the cyst. The thick neck, covered with these minute oval bodies, is strongly contrasted with the fine smooth membrane forming the cyst.

An accidental degree of pressure upon the glasses between which the hydatid was placed brought them into such close contact, that the object was completely flattened, and a minute fissure was produced in the head, from which a small quantity of a thickish fluid exuded, and five discs were brought most favourably into view, allowing of the employment of a high power object-glass. Some of the hooks were removed by the pressure to a little distance from their original situation on the glass; but one complete circle of them, and a part of a second circle, remained *in situ* on one of the discs.

I have been able to make a tolerably good preparation of the hydatid by drying it between two glasses. A doublet of one-tenth of an inch shews the hooks and the granules very distinctly.

An interesting case of the Cysticercus cellulosæ in the anterior chamber of the eye, referred to by Mr. Owen, is detailed in Dr. Mackenzie's work on the Diseases of the Eye, as having occurred at the Glasgow Eye Infirmary in 1833. It appears that this hydatid is not very often found in the human subject; and I am not aware of its having been met with before, forming a tumor upon the sclerotica.

After opening the cyst I applied, as I usually do in these cases, a solution of the nitrate of silver to the wound, with the view of promoting adhesive inflammation of the secreting surfaces. On examining the child's eye after evacuating the tumor of its contents, it appeared to me that there was a portion of white membrane, similar to the piece which escaped, remaining beneath the conjunctiva; and such still is apparently the case, though several days have elapsed since the operation. No disposition, however, to a return of the tumor is yet discoverable. It is probable that another hydatid existed, the cyst of which being divided, allowed its contents to escape.—I am, sir,

Your obedient servant,

J. B. Estlin.

Bristol, August 1838.

APPARATUS FOR FRACTURES.

To the Editor of the Medical Gazette.

Sir,

Having, upon the various occasions on which I have been called upon to apply Mr. Amesbury's thigh-splint, observed that it was not so perfect as it might have been, I am induced to publish an addition I have made to that really valuable instrument, which, in my opinion, will render it much more beneficial in its effects and a source of greater comfort to the patient.

The object of the surgeon, in fractures of the thigh, is to place the limb upon the splint in such a position that the fractured extremities of the bone may remain in exact apposition, not by active extension or pressure, but by retaining the muscles in a perfect state of quiescence, taking care that the limb is completely at rest, not pressing more on one point than another; in a word, that the instrument shall completely support the limb, without the slightest effort on the part of the patient; that it shall be so adjusted as to prevent shortening of the limb by passive opposition, and not by active extension. These objects have not hitherto been effected so completely by Mr. Amesbury's splint as I think they will be by the assistance of my addition, and for the following reasons: — The points of extension and counter-extension in fractures of the thigh are the tuberosity of the ischium and the bend of the knee; but as, when the patient is placed upon his back, the tuber ischii is raised from an inch and a half to three inches from the mattrass, according to the size of the patient, it is evident that in the original splint extension cannot be maintained, unless the splint is either supported by pads, which from their softness are always yielding, or by the fractured limb itself,

fig. 1

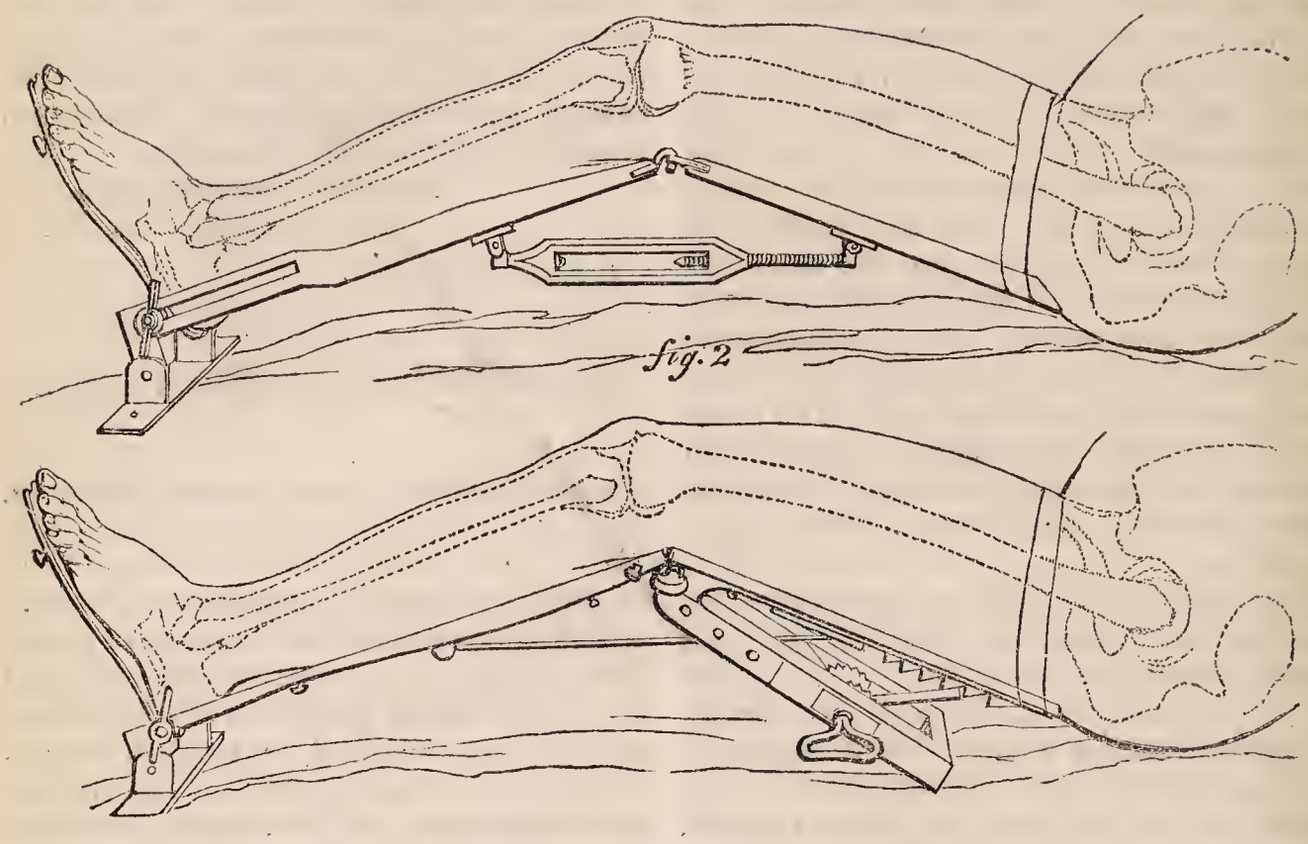


fig. 2

by means of the pelvis strap; thus, the limb is rendered a support to the apparatus, instead of being sustained by it, without any exertion, as ought to be the case. To obviate this defect, I have added a frame beneath that portion of the splint destined to support the thigh, which, with the assistance of a lever worked by a simple cogged wheel, adjusts the splint, and obviates any alteration in the depth between the tuberosity of the ischium and the mattrass, and maintains it in constant apposition, preventing any fatigue to the limb, which it supports without the slightest exertion on the part of the patient.

The benefit to be derived from my alteration, will, I trust, be made evident from the annexed wood-cut. In fig. 1 the original splint is given as applied at the North London Hospital—in fact, it was taken from a case of fracture under treatment. The objections are evident. The termination of the splint will be observed raised from the bed to a considerable distance, and wholly supported against the tuber ischii by the pelvis strap, making it very apparent that the limb cannot be in so quiescent or passive a condition as is necessary for the well-doing of the patient. Fig. 2 represents the splint with my addition supporting the limb without the slightest effort on the part of the patient. In consequence of its firmness, it is not liable to slip or yield, and thus when once properly adjusted it will obviate all chance of those changes of position which are difficult to avoid with the common apparatus.

In conclusion, I beg to observe that I shall be most happy to shew and further explain the instrument to any of my professional brethren, should they deem it worthy of their notice.

I have the honour to be, sir,
Your obedient servant,
HENRY HANCOCK,
Lecturer on Anatomy and Physiology,
Charing Cross Hospital, &c. &c. &c.

59, Harley Street, 16th August, 1838.

MESMERISM.

REPORT OF A VISIT TO UNIVERSITY COLLEGE HOSPITAL;

With Remarks on the Animal Magnetism there exhibited.

BY WM. COOKE,
Secretary to the Hunterian Society.

THROUGH the politeness of Dr. Elliotson I was present with my two sons on Saturday last, when the Doctor went through many of his former experiments on the two girls whose cases

have so often been brought under public notice. Dr. Elliotson knew nothing of me personally, and was equally a stranger to the sentiments I had adopted with respect to the published details of his experiments, yet he most frankly and promptly complied with my request to be a spectator when his experiments should be repeated. With much pleasure, therefore, I express my conviction, that if there is deception practised, it is not practised by this distinguished physician. In this allusion to what transpired on that occasion, and in publishing my own impressions with respect to the experiments performed, I do not feel that I am violating any of the obligations of courtesy, because the Doctor himself stated that he did not wish to limit any persons in their remarks on his proceedings.

It is but fair to state that, having read most of the reports on animal magnetism, I had felt entire incredulity as to its power, except so far as mental influence was concerned. The advocate of the *science*, therefore, will feel at liberty to say that I was a prejudiced observer.

The first patient I saw operated upon was a countryman, who was thrown into a fit of hysteria, and whom we left rolling on the floor, the operator not being able to arrest the progress of the paroxysm.

On entering the room where the two girls were, apart from other patients, I could not but look at them with a special feeling of interest. Their ages, I think, are 13 and 17. The elder lay on the bed, looked very pale and languishing, and expressed herself as having headache. The younger was sitting up, looked robust, simple-minded, and playful. They were addressed by Dr. Elliotson in a tone of great familiarity, and in language which assured them of his admiration of their exemplariness. In them there was not, at this period, the slightest indication of undue forwardness. Dr. Lardner was present, and acted as time-keeper.

As the experiments performed on this occasion have been already detailed, I shall only allude to them cursorily. They were first made on the younger girl. A farthing, a shilling, and a sovereign, were successively magnetised, by being held during fifteen seconds in the hand of Dr. Elliotson; and on being transferred to the hand of the girl, violent contraction of the hand ensued, at about the lapse of three minutes. When these coins were carefully wiped before being placed in the girl's hand, no contraction followed. This patient was soon dismissed; for it will be evident to every spectator that she is a far less interesting object for experiment than her sister.

A similar series of experiments was made on Miss Okey, and she quickly roused from her apparent languor to high and interesting excitement. She became voluble, and manifested no small degree of shrewdness; and her brogue, real or assumed, added much to the vivacity of her manner and expression. She made very familiar with the experimenting and noting doctors, but not more so than was agreeable to them. This state, however, was repeatedly interrupted by the magnetic influence. She was fixed statue-like, or she was knocked down by an overcharge, or she was laid into a profound and stertorous slumber; roused again by the counteracting stroke of the operator's hand, or the counter influence of metals not possessing the magnetic power.

But the most extraordinary power of this magnetic influence remains to be mentioned. The hand held over the coin at the distance of some inches, and for about half a minute, has the power of imparting to it the quality of acting on the nervous system through the hand into which it is received. Some reader may say that this is scarcely credible; and if so I fear he will withhold from me the credit of veracity when I mention a yet higher experiment. Dr. Elliotson, standing at the distance of about a yard and a half from the mantel-piece, fixed his eyes steadily on the end of it. He then placed a sovereign on the spot to which he had directed his vision, and after allowing it to remain some seconds, he requested Miss Okey to take it up. The coin had become magnetized, and the girl was fixed, and it required some of the common means resorted to by the magnetizer to bring her to herself.

These were some of the many facts we were permitted to observe; but to me and to my companions the mode of conducting the experiments, as well as the experiments themselves, was any thing but conclusive. It was not an occasion for the performance of any cross experiments, but I learnt from Dr.

Elliotson that the effects I saw were invariable, and that there was no doubt of their validity. I believe that in Dr. Elliotson's mind there exists no doubt on that point, but as the subject is fairly before the profession and the public, I beg to state the ground of my own doubts.

1. It appears that these girls themselves did not exhibit the power of being acted on until magnetism had been practised on them for some time.

2. Although the younger girl was blindfolded during a few of the experiments, yet, from the confidence reposed in her, there was so little caution used that it was not clear that she was not aware of the order in which the magnetized and unmagnetized coins were placed in her hands.

3. During the whole period of the experiments it was taken for granted that the girls were incapable of deception, and therefore Dr. Elliotson and Dr. Lardner talked of the results both before and after an experiment was performed, in the hearing of the girls, so that it would have been extraordinary, under all the circumstances, if the anticipated results had not occurred.

4. It is totally at variance with all analogy that the frequent repetition of shocks capable of producing contraction of limbs, capable of suspending all motion, and fixing the body in a wearisome attitude, or sufficient to produce sudden and deep insensibility, can be undergone without leaving the system disturbed and the person exhausted; and yet at the lapse of an hour, after all apparent changes of condition, the elder girl seemed as lively and gay as at first.

5. I am totally ignorant of any law, in any department of science, by which the act of looking upon a stone can give to that stone any quality it did not previously possess.

6. If coins were susceptible of being magnetized by their retention in the hand, their effects must have been long since ascertained from the frequency of transfer from one person to another, after having been acted upon by perspiration, under all circumstances of temperament and excitability.

Though not placed formally among my reasons for distrust, it appears to me that the possession of a power capable of producing sudden unconsciousness by an agency not detrimental to health, would be so liable to perversion, so easy of application by a debased mind to purposes of wickedness, that it would scarcely have been passed over in the prohibitions or injunctions of the Decalogue.

There is a part of this exhibition of Miss Okey which surely demands some animadversion. Be the case what it may, and let her be considered in a state of somnambulism, and not for the time accountable for her expressions, is it discreet that her younger sister should be present whilst she is using language the most profane, and rendered the more dangerous by the sweetness of her voice and those attractions of manner that make one specially regret that a child at so interesting an age should be familiar with language seldom heard more profane at Billingsgate? But with my belief that Miss Okey well knows what she is about, I regard such an exhibition as that I witnessed as having a tendency to destroy these proprieties which adorn the female character in every condition of life, and as involving her in moral guilt.

It does not necessarily follow, that if my opinion prove true, these two poor girls should incur all the culpability of the deliberate impostor. They have been cut off from a watchful parentage—placed among influential strangers—subjected to measures giving them extraordinary eclat—much caressed by persons of distinction—and is it to be wondered at that they have lost an ascendancy over themselves? Are there not many instances of female deceptions, practised under much less extenuating circumstances, which are quite unaccountable, except by imputing them to a viciousness of mind produced by causes acting on the nervous system, and usually connected with error in functions peculiar to the feminine constitution.

I had sketched out this paper before the Medical Gazette of July 14 fell into my hands. The very important paper of Mr. Blake, in that number, giving an account of the failure of the experiments when so conducted as to withhold from the girls the means of knowing whether things submitted to their touch were magnetized or not, supplies the deficiency of my own paper, and I think fully bears out the inferences I was led to draw from the witnessed experiments.

Dr. Elliotson is a zealous practitioner, and if he have a crotchet on this sub-

ject he possesses so many redeeming acquirements, and occupies a station of such high influence over a numerous class of inquiring students, that his brethren watch his movements with a generous solicitude. Those who are parents, and have sons educating for the practice of medicine or surgery, surrounded as the pursuit is with difficulty, will inquire, whether in the attempt to remove these difficulties any teacher beset the path of inquiry with principles or practices which lower the dignity of the profession; and in the growing regard to the investigations of truth, natural and revealed, there will be a decided preference of the teacher who, in his own language and deportment, and in all his investigations, sets an example of deference to moral and physical truth.

39, Trinity Square,
Aug. 16th, 1838.

EXPERIMENTS AT UNIVERSITY COLLEGE HOSPITAL.

To the Editor of the Medical Gazette.

Sir,

The following remarks have been called forth by Mr. Mayo's letter, in your journal of the 11th instant. It is with some diffidence that I venture to oppose myself to such eminent men as Dr. Elliotson and Mr. Mayo; but the knowledge that I have chiefly to speak of facts gives me courage. Although I shall confine myself more immediately to the consideration of the correctness of Mr. Mayo's statements, perhaps I shall, as the question is of such interest, be excused if I enter in some parts somewhat at large on the subject of mesmerism. When an individual is called upon to examine such extraordinary phenomena as are asserted to be produced through the agency of mesmerism, he ought *à priori to assume* that those in whom such phenomena are exhibited are impostors. How much the more necessary is such an assumption when it is found that the patients are labouring under hysteria or epilepsy, which diseases are, as is known to all, so frequently accompanied by a morbid state of the mind predisposing the patients to deception. This was shewn in Ann Ross, who pretended sleep, somnambulism, &c. from mesmeric processes; when the trick was discovered she could give no reason for having feigned. Even if, at the commencement, all due precautions are taken, and the effects produced by mesmerism are found to be real, yet, as it frequently happens that epileptic or hysterical patients after a time feign certain affections which they at first really laboured under*, the inquiry at each successive stage ought to be carried on with the same assumption, namely, that those operated on are attempting an imposition.

A medical man, previously well acquainted with the attainments of Dr. Elliotson, would, on hearing for the first time of the effects of mesmerism, naturally presuppose that he (Dr. E.) had taken all possible caution in conducting the inquiry. Such, however, has not been the case. Deception is a thing so foreign to *his* character, that it is with the utmost difficulty he can be led even to suspect it to form an ingredient in that of others.

When profound sleep and ecstatic delirium, with loss of sensibility on the surface, were the only effects resulting from mesmeric movements, he believed those effects to be real, although, since the exposure of Ann Ross, who bore the extraction of two double teeth without wincing, he states that he knew that sufferance of pain is no test of the existence of coma, of the reality of epileptic fits, &c., remarking at the same time, "I have shown *that* in my Physiology."

After a time, Dr. Elliotson discovered that mucous surfaces absorbed the mesmeric influence very rapidly and effectually, so that by blowing on Okey's eye, she being in ecstatic delirium, coma was almost instantaneously produced. Now, previous to this discovery, Dr. E., in order to shew Okey's imbecility on some points in this state, pretended he had swallowed Mr. Wood, and Okey used

* There is a remarkable instance of this now in University College Hospital. — Pearsey, æt. 25, was frightened about eight years ago by a person who started up, enveloped in a sheet, from behind a cellar-door, as she was opening it. She fell down in a state of insensibility, which lasted three days; and in the meantime the young man who played the trick upon her had poisoned himself. She was afterwards seized with hysterio-epileptic fits, which continued at intervals till her admittance into the hospital, at which time, also, her right leg was permanently contracted and bent up towards the nates. During a fit, the leg was brought down, and she was made to perform certain other *voluntary* movements by means of *pretended* mesmeric processes. We cannot but believe that the fits were real at first.

to look down Dr. E.'s throat after Mr. Wood; yet, although her eyes were within an inch of the open mouth, and they must necessarily have absorbed the mesmeric influence emanating along with the pulmonary exhalations, not the most distant approach even to drowsiness ever appeared. It was also found, that touching the mucous membrane of the eye or mouth, (Okey being in ecstatic delirium) produced instant coma or catalepsy: nevertheless, Mr. Chapman, at that period the house-apothecary, by pretending to be busied in removing a piece of tea leaf, sticking between J. Okey's teeth, was enabled to rub the mucous membrane of her lip for a minute or more without producing sleep. It has been objected to this experiment that the Okeys frequently have a temporary state of system, during the continuance of which mesmeric manipulations are not followed by any result. To this objection I answer, that both immediately before and after the experiment just noticed, sleep was instantly produced by touching the mucous membrane. I have also seen Okey, when delirious, kiss persons, but under such circumstances no sleepiness occurred.

It was further found that if one patient were in the coma, or somnambulism, a similar state came on in another mesmerizable patient if she held the hand of, or was otherwise placed in contact with, the first patient; but I have seen the same result follow the contact of two persons, when the first was not in a mesmeric state. E. Okey put her arm round the neck of Ann Ross, who was in a state of somnambulism, and gradually but rapidly passed from ecstatic delirium to somnambulism. The very next day Ann Ross confessed that she had never been sent to sleep. If it be objected that the mere contact with Ann Ross would produce the somnambulic state in Okey, I answer that I have frequently seen the Okeys hug Dr. Elliotson, Mr. Wood, and the nurse, most heartily, without any such effect resulting. Besides, if contact with any person would produce such an effect, why did Dr. Elliotson ever call attention to the mesmeric state of the first patient?

I now come to the observations of Mr. Mayo. He states that "gold, silver, water, the moisture of the skin, are capable of receiving and retaining the mesmeric influence, whilst others are not, as copper, pewter, &c." Yet I have seen a mesmerized sovereign produce no effect, and the contrary happen when a copper Victoria medal was employed. On the day the experiments I allude to were performed, I held a pillow before E. Okey's eyes, whilst a fellow-student, with all the precautions suggested by Dr. E., passed the coins across Okey's hand, and after each crossing she was desired to close the hand firmly. After a time, when a trance had been caused by the Victoria medal, Dr. E. observed that the effect probably arose from the contact of my knees with Okey's, as I stood before her. In answer to this objection, I shall now merely state that the "fixation" came on *suddenly*, and not *gradually*, as one would suppose it ought, if owing to the gradual transmission of mesmeric influence from my body to Okey's. Whatever source of fallacy may be pointed out in the last-named experiments, still the fact will remain, that the copper piece frequently produced effects quite as powerful as those brought on by means of the sovereign.

On one occasion a purse containing several sovereigns was taken from a gentleman's pocket, and the coins laid on a table. J. Okey (being in a state of delirium) took them up, one after the other; none produced any effect. The girl was then taken out of the room, and it was suggested that the sovereigns should remain untouched, and that Okey should be recalled, and told to take the sovereigns up again. Dr. E. observed that he would not answer for the results of the experiment, as the coins would most probably become mesmerized in a short time by the mesmeric emanations proceeding from the bodies of so many persons (about twelve) in a small room; and assuredly when Okey was called, and took up the coins successively, several of them produced the "trance." I am at a loss to conceive how it happened that some of the coins had become mesmerized whilst the others had not, all being exposed to the same influence; and I am still more at a loss to understand how so small a quantity of influence as could be applied to Okey's hand through the medium of sovereigns, produced catalepsy, when the emanations with which the air of the room was charged, and by which the coins were mesmerized, produced no

effect, although they must have been directly applied to the bare surface of Okey's face, neck, and hands. But even admitting that the emanation from our bodies *had* mesmerized the sovereigns as they lay on the table, how was it that they produced no effect when Okey took them up, almost immediately after they had been taken out of the gentleman's pocket? They must have been there absorbing mesmeric influence for a considerable length of time *.

Mr. Mayo states that "the effects produced" [by a metal] "are proportionate to the vigour of body and health of the party who may have mesmerized the metal." I was not present when this was first demonstrated by means of the patients in the ward; but on the second occasion I found that Dr. E. spoke, without the least caution or reserve, before J. Okey, of the effects he expected to ensue from the mesmerization of the metal by different patients; and I am told by a person who was present on the first occasion that he did the same then. Supposing the last statement to be incorrect, the experiment is worthless, as the patients were in the same ward as Okey, and she knew perfectly well the relative strength of the patients.

Lately, when I was absent, similar experiments were tried with patients in another ward, the faces being covered. A fellow student states that as much effect was produced on Okey when a sovereign was mesmerized by a patient greatly reduced by peritonitis, as when mesmerized by a younger woman having no other complaint but procidentia uteri.

As a former number of this journal contains Mr. Blake's remarks on "mesmerized water," my observations on it will be few. I may, however, state, that Dr. E. unreservedly talks of the failures, sources of fallacy, &c. before the Okeys, whatever state they may be in. This great want of caution arises from his implicit faith in the honesty of the girls, and his perfect conviction of the truth of mesmerism. I will give an example of this want of caution. One day an effect was produced by water which Dr. E. had *not* mesmerized. He remarked, that doubtless the water had become mesmerized by a person who was sitting near the corner of the table where the glass stood. Okey was then in the cataleptic state; if she were feigning, she learned that on all subsequent occasions she might, when puzzled, go off into a trance, if some one had by chance stood near the glass she was drinking from.

I may here mention, that when no source of fallacy suggested itself to explain a previously unexpected result, Dr. E. or Mr. Wood used frequently to induce that state in Okey in which she holds communion with her "negro," and then tell her to ask him (the negro) what had been the cause of the unexpected result. She did so, and they always appeared satisfied with the explanation afforded by the negro.

We have seen that Dr. E. believes that the mesmeric influence sometimes temporarily ceases to affect the Okeys. Let us suppose that down to this time the phenomena had been real, but that on a sudden, without any apparent reason, the Okeys became permanently insensible to the "influence;" how easy it would be for them, if so inclined, to practise imposition, when experiments are conducted in such an incautious manner.

Mr. Mayo further states, that "one of these (the non-mesmerizable metals) can transmit the mesmeric influence. . . . . E. Okey being in mesmeric delirium, held a pewter porringer in her hand by the handle; half a dozen mesmerized sovereigns were then placed in it. In a few minutes the trance supervened." In the beginning of the next page I find, "when she," [E. Okey] "has been thrown, by holding a mesmerized sovereign or shilling, into coma or delirium, the continued contact of the mesmerized metal prolongs these conditions. It is then difficult to extract the piece of money from her grasp, so firmly are her fingers closed upon it. The means that are on other occasions available to relax the mesmeric cramp of the muscles of the hand do not now succeed. It is in vain that the operator holds his fingers towards her closed hand; the mesmeric cramp is not relaxed. But if he now employs the agency of a non-mesmerizable substance, insinuating, for example, with this object a tin or brass extinguisher

* I have heard Dr. Elliotson state that he did not consider it right in gentlemen to publish remarks on phenomena which were under the course of investigation. This observation cannot be applied to the above remarks, after Mr. Mayo's letter.

into the closed hand, the cramp is found speedily to yield to the ordinary manipulation, and the hand opens as quickly as if the mesmerized metal were not in it." It is difficult to conceive how, in one instanee, "the influenee of the mesmerized metal is thus exactly neutralized by that of the non-mesmerizable substance," whilst in another instance the non-mesmerizable metal "transmits" the influence.

But, admitting that the influence is exactly neutralized in this case, still the sovereign ought to be retained, as more mesmeric influence is passing off from the hand of the person holding the extinguisher, in the same manner as it passed off from my knees, as already mentioned in a former experiment.

In a subsequent paragraph I find, "In this marvellous inquiry, wonder suceeeds wonder." . . . . . "Simply looking upon a mesmerizable substanee placed at the distance of two feet from you, will mesmerize it." If this be the case, *all* the "slight effects" hitherto regarded as a result of mesmerization, from momentary contaet with the operator's finger, may have been produeed by some person looking at the mesmerizable substance. "If you desire her to hold out both her hands, (she being, of course, as for the other experiments, in the state of delirium,) and you look for a minute intently upon one, and desire her to shut that hand, and to keep it so shut, squeezing it hard at the same time, the same result (mesmerie effects) ensues. In this experiment, her closing the hand you have not looked at is followed by no mesmerie effect." I eannot but consider it the height of absurdity in Mr. Mayo, to believe that a "*physical* influenee," as he subsequently states it to be, whieh physical influence "emanates from living beings," (independently of their will, as is proved by the frequent accidental mesmerization of substanees), should require that the *eyes* of the operator be directed towards the part, in order that its effect may be produeed.

With regard to the instance of "prevision," noticed by Mr. Mayo, I may remark, that it is very easy for any female who *pretends* to be without eommon sensibility, to prophecy that she will regain it a month hence, and it is very advisable for her so to do, if she thereby exempts herself from the not very agreeable task of suffering the pinches, pulls, and pricks, spectators may be pleased to bestow upon her, in order to test her insensibility.

Next, and lastly, comes "the marvellous phenomenon of transposition of sensation."

"I have seen her (E. Okey)," writes Mr. Mayo, "on three oceasions assume a new eondition, and be without eye-sight, hearing, or taste, while she has distinctly perceived some objects placed a few inches from her, through a sense more akin to vision than to any other that has been developed in her hand." I have only once seen E. Okey in this state, and then Mr. Chapman, one of my fellow-students, and myself, made the following experiments:—We took a piece of bread, and held it near the back of the left (the seeing) hand; in a very few seconds the hand began to move in a somewhat rotatory manner, at first slowly, then more rapidly; suddenly she clutehed the bread, earried it to her mouth, and began to eat with great eagerness. When she had swallowed the morsel, she placed the baek of her hand near the open mouth, as if to ascertain whether all the food was gone. Now if there were neither the senses of touch or taste remaining, and the muscular sense of resistance regulated the movements of mastication and deglutition, surely it ought also to have enabled her to know whether any of the bread still was or was not retained in her mouth. In the next place, we held a folded eloth before her eyes, and then took another pieee of bread and plaeed it near the right, the non-seeing, hand (we were not at that time aware that the left hand only saw), having previously touehed the left hand with it, in order to make her believe it to be held near that. After a very short time the hand began to move. We whispered to each other, "She begins to see it." Presently she made a clutch, and of course found nothing. It was then pretended that at the moment it was seen she was on the point of seizing the bread; it was removed, and one of us remarked it was a pity thus to tantalize her, as she was evidently very hungry. The experiment was repeated, in the same manner, three times more, and with the same results.

So mueh for that state in whieh "all the common avenues of sensation are occluded—when eye-sight, taste, touch, hearing, are suspended; and when a

sort of vision is sensibly exercised by some part of the common surface of the body!"

I have now concluded my remarks, and although Dr. Elliotson has told some of us, his pupils, that he believed (and a most illiberal belief I consider it) that no experiments, however conclusive, would convince us of the reality of the mesmeric phenomena, I trust that I have shewn that our reasons for disbelief, or at all events for great doubt, are not based on insufficient ground.

I remain, sir,
Your obedient servant,
EDW. WOOLDRIDGE.

1, Jermyn Street, Aug. 20, 1838.

NEW NAME PROPOSED FOR MESMERISM.

To the Editor of the Medical Gazette.

SIR,

IT has been suggested to me that another name should be substituted for those hitherto employed to denote the remarkable class of phenomena of which I have recently sent you some accounts. It has been observed, that the term "animal magnetism" is objectionable, from its suggesting an affinity, which does not exist, between these phenomena and common magnetism; and that both with that term and with "mesmerism" so much imposture and roguery are associated, that on that account the sooner both are got rid of the better.

In their place I therefore propose the term *exoneurism* (from ἔξω and νεῦρον) to express the class of phenomena; and for the adjectival form, the word *exoneural*.

The word exoneurism happens to be appropriate to both kinds of phenomena which the present inquiry embraces; of which one is, the change produced in the nervous system of a living being by a peculiar influence *from without*, or (looking to the cause instead of to the effect) the action of the nervous influence of a living being *beyond* the limits of its frame: the other, and the more questionable, being the action of the mental principle independently of or *without* its usual organs.

The *ordinary* phenomena of the nervous system may be classed as *esoneural*.

I have now completed the fostering offices towards mesmerism, which its forlorn condition appeared to me to claim for it, as soon as I detected beneath its dirt and frippery the marks of an honest extraction. I have viewed it as stolen at its birth by quackery from science. I have endeavoured to restore it to its true rank and place. I have clothed it in a dress in which its proper parent need no longer be ashamed to own it. I have finally contrived for it something like a philosophic appellation.—I am, sir,

Your obedient servant,
HERBERT MAYO.

19, George Street, Hanover Square, August 24th, 1838.

CASE OF LUMBRICI PRODUCING TETANUS.

To the Editor of the Medical Gazette.

SIR,

THE inclosed case happened to me some time back: should you deem it worthy, may I request its insertion in your valuable GAZETTE.—I am, sir,

Your obedient servant,
W. CLARK,
Surgeon.

Long Street, Devizes, August 10, 1838.

George Maslen, æt. 45, a labourer, living at Potterne, a village about two miles from this town, was taken ill in November last, but his indisposition appeared so slight, that it was not at that time thought neeessary to eall for medieal advice. He remained for a day or two with the usual symptoms of cold, accompanied with a general stiffness of the body and limbs. This stiffness increasing, alarmed his wife, who requested me to see him. This was on the fifth day from the commencement of the attaek. The peeuliarly arched position of the body as he lay in bed, without reference to other symptoms, was a sufficient indieation of the disease under which he laboured. The head was drawn back; the ehest prominent and fixed; the muscles of the face eontracted; and all lateral motion of the lower jaw lost. The teeth could only be separated sufficiently to allow the apex of the tongue to be thrust between them; respiration laborious and abdominal. A profuse perspiration bedewed his body

and face, but his feet and legs were cold, and in a bent position; the muscles of the back were of a board-like hardness, as were also those of the lower extremities. Pulse 85, natural as to strength; bowels costive; breath offensive; urine scanty, and of a pinkish hue; vision of both eyes impaired, the pupils being almost insensible to light; his mind calm, and even cheerful. The most minute inquiries were made, with the hope of gaining some clue to the cause of this attack, but none could be found save the cold he had taken from exposure to damp. From the costive state of the bowels, &c., it was thought advisable to adopt a purgative plan of treatment; calomel and scammony were therefore prescribed; they acted powerfully, and the evacuations were dark and offensive. On visiting him the next day, further questions were asked, and amongst others, whether he had ever been subject to worms. This was answered repeatedly in the negative. The patient's wife, however, gave clear proof that great intestinal irritation existed previous to this attack, inasmuch as she had remarked that he frequently started in his sleep, that he "ground his teeth," and had convulsive twitchings of his arms, and other symptoms which usually denote the existence of an irritating cause. It was consequently determined to persevere in the purgative treatment.

After a large quantity of offensive feculent matter had been evacuated, three large lumbrici were dislodged, and from that time the spasm relaxed; the back lost its arched appearance; the head could be raised from the pillow; the mouth could be opened nearly to its full extent; deglutition improved; lateral motion of the lower jaw became natural, and the features less contracted; in short, it is needless to describe the gradual but sure relaxation of the spasm. Upon one occasion only was there any tendency to a relapse, and that was produced apparently by frequent attempts, during the night, to turn himself in bed. Since that time he has had no return of the tetanic spasm. In a fortnight he left his house, and in a month was able to resume his work.

It has been said, that when worms are found to co-exist with tetanus, they are to be regarded rather as an accidental circumstance than as a cause of the spasm. With this doctrine I cannot agree; for how frequently do we see in children chorea and other spasmodic diseases produced by the presence of worms in the intestinal canal. In the early part of last June I was requested, in the unavoidable absence of the surgeon (Mr. Trinder) to visit a patient at the gaol, whom I was informed was dying. On examination, I found the lower extremities in a complete state of spasm; the muscles rigid and contracted. He suffered severely from cramp; his features were collapsed; pulse scarcely perceptible; hands and feet cold; in fact, the complaint under which he laboured appeared to be cholera. I ordered his feet and legs to be put into hot water with mustard, and brandy and water to be given him frequently. A short time after my visit (to use the expression of the nurse,) two large worms (*lumbrici*) crawled from his mouth; the effect was, that the spasm immediately began to relax, and he rapidly recovered.

These cases, I think, shew the power of worms in producing spasm; but whilst, on the one hand, I cannot agree to the doctrine of worms being merely accidental, on the other my experience will not enable me to assert their all-sufficiency in producing tetanus. We are told that the most frequent cause of true idiopathic tetanus is exposure to cold and damp, and that there are very few cases induced by any other cause. It is true that cold and damp may, in the case of Maslen, be regarded as the exciting cause, but it appears to me that a predisposition to nervous irritation occasioned by the presence of worms must previously have existed; and we know where a predisposition to any disease exists, how easily the latent spark is fanned into a flame.

Since the above was written, I have found two cases recorded in the 1st vol. of the Edinburgh Medical and Surgical Journal, in the years 1809 and 1810: the first, reported by Dr. Briggs, of Liverpool, is "a case of tetanus, cured by purgatives;" the other, by a naval surgeon, translated from Dr. Stoll, and giving the appearances on dissection. Lumbrici, in this case, were found in the jejunum. Dr. Stoll asks, "Were the worms the cause of the disease? or was it some other acrid matter lodged in the intestines, or in some other part of the body?"

PATHOLOGY OF ABDOMINAL TUMORS.

To the Editor of the Medical Gazette.

Sir,

The following case may perhaps serve as a trifling contribution to the pathology of abdominal tumors:—

W. Robinson, 28 years of age, a waiter in a gin-shop, was admitted into this hospital, complaining of tension and pain in the abdomen. He was not able to sit; his face was sallow, sharp, and somewhat emaciated, having the expression of one affected with organic disease of some abdominal viscus, or of a female suffering from ovarian disease. His limbs were tolerably plump; breathing frequent; appetite impaired; and he vomits frequently, but especially after taking liquids. His urine was natural, but dribbled constantly, and his stools were loose and somewhat frequent. The abdomen was enormously distended, and tympanitic over its whole surface, except above the pubes. Here there was a tumor extending to near the umbilicus, where it had a defined margin; it was elastic and resistant to the touch, and gave an obscure sense of fluctuation. In the epigastric and right hypochondriac region there was a large rounded tumor, distinctly tympanitic. On examining per anum I found a large tumor pressing from within the pelvis, and affording an elastic feeling, like a bladder much distended by fluid. The spines corresponding to the twelfth dorsal, and first and second lumbar vertebræ, were tender to the touch.

From his childhood the patient had been subject to occasional incontinence of urine, especially after taking spirituous liquors. Four months ago this symptom became constant; he was compelled to pass his urine frequently; and a swelling appeared above the pubes. This gradually increased in size; the abdomen became distended; and in three months he was attacked by sudden paroxysms of most acute pain, commencing in the loins, and extending round the abdomen. The case appeared very obscure, but it was supposed that an hydatid tumor completely occupied the pelvis, and projected above the pubes; and by compressing the rectum, bladder, and urethra, originated the dribbling of urine, tympanitic distension, and other symptoms enumerated. It was proposed to thrust a curved trocar into the tumor through the rectum, and evacuate its contents.

After his admission opiates were administered, and attempts made to administer an assafœtida enema, and to introduce the catheter, but without success. He suffered from paroxysms of pain, during which he uttered very loud cries, and vomited frequently.

On the third day after his admission the vomiting became more urgent, and the fluid ejected stained his linen with a bright yellow colour. When I requested him to preserve me some for examination, he replied, "I'll throw up some directly, sir." He then sipped a little tea, and with very little effort immediately vomited six or seven ounces of a yellowish red fluid, which communicated to linen the yellow stain already mentioned, became of a dark green colour on the addition of hydrochloric acid, and threw down a copious precipitate with a solution of nitrate of silver. On the sixth day he became suddenly worse, and died.

His friends removed the body to his home, where, through the kindness of Mr. Anderson, his previous medical attendant, I was enabled to assist at the post-mortem examination, thirty-four hours after death. As the corpse was in the coffin, it was not examined so carefully as otherwise it would have been. There was a thick layer of fat beneath the integuments of the abdomen, and the abdominal muscles were remarkably thin: the omentum had undergone that carneous degeneration which has frequently been found connected with the vomiting of green or yellow fluid. The liver was pushed high into the thorax, and the stomach was compressed against the vertebral column by the colon, which was enormously distended with air, and constituted the rounded tumor observed in the epigastric region. The small intestines were equally distended by an enormous quantity of liquid fæces, and their peritoneal coat was injected. The bladder was found to form the hypogastric tumor; its parietes were much thickened, and it was distended with urine, of which it contained about a pint; there were a few flakes of puriform matter on its peritoneal coat; its inner surface was not diseased. The true pelvis was completely occupied by

a tumor, which pressed closely against the whole of its walls, but particularly against the symphysis of the pubes and the promontory of the sacrum; at these points the finger could, with great difficulty, be introduced between them and the tumor: the latter being punctured, several ounces of a limpid and almost colourless fluid gushed out. The tumor was now found to be formed by this fluid contained in a serous cyst, having parietes of a cartilaginous structure, and lined by a smooth membrane: the latter was easily and completely removed from the outer structure, with which it appeared to have little or no organic connexion. It was about two lines in thickness, opaque, of a milk-white colour, and very lacerable. Its inner surface was dotted with a number of blackish-grey points, about the size of mustard or lint seeds: they consisted of a soft mucous-like matter, beneath which was a very small black point, surrounded by an areola.

This tumor was plainly of the kind termed *aqueous encysted* by Mr. Cæsar Hawkins, and *acephalo-cyst hydatid* by Andral; corresponding in every respect with the latter author's description of that entozoon*. The early age at which the urinary functions were disturbed, would lead to the suspicion that it had existed many years in a quiescent state. The degeneration of the omentum might be supposed to follow the impeded circulation through the hæmorrhoidal veins.

No operation was admissible in this case; but the question how far puncture of such a tumor would be proper under more favourable circumstances seemed to me of some importance. From the following data I came to the conclusion that the operation would be, although not free from danger, yet justifiable as a last resource.

Baron Dupuytren, in his clinical lectures on Hydatid Tumors, mentions a case in which a swelling of this character followed a blow with a whip on the temple; he opened it, and a large white sac escaped with a quantity of serous fluid. He also operated on a painter, who had a similar acephalocyst at the bend of the arm. His description of these cysts in general corresponds very closely with that I have given; and he distinguishes them from serous cysts. He quotes Professor Lassus, as relating several cases in which incision of the cysts accelerated the death of the patients, but thinks the operation favourable if the hydatid be external to the visceral cavities.

Sir B. Brodie recommends the cyst to be punctured so soon as it becomes inconvenient from its magnitude, but not before*.

Mr. Park has recorded six cases of tumors within the pelvis impeding parturition: three only of these bear upon the question of operation: the result of all is in its favour†.

Mr. C. Hawkins, in an essay on Aqueous and Hydatid Encysted Tumors‡, has quoted or referred to 31 cases; of these the results of 11 bear upon the question under consideration; in 9 they are favourable, and in 2 only unfavourable to the operation.

Mr. Stanley had a case in St. Bartholomew's Hospital of hydatid abscess of the liver. Mr. Stanley punctured the tumor, and evacuated more than a gallon of pus and hydatids. The case went on favourably§. Another case, somewhat similar, in which the liver was cut into to the depth of an inch and a half, occurred in one of the hospitals in Paris, with apparently a favourable result.

A case still more in point occurred in a female, aged 38, who was in La Charité Hospital, under the care of Roux. Eight years previously she had a difficult parturition from a tumor situated in the pelvis. In five years after it had increased so much as to render the evacuation of the rectum and bladder difficult; and at the time of admission this was almost impossible. A resistent tumor could be felt in the vagina, with which, however, it was unconnected. Roux made an incision into the tumor from the vagina, and evacuated a quantity of a diaphanous, straw-coloured fluid: the finger could then be introduced into a capacious cavity, from which membraneous fragments of a pearly-white colour, and gelatinous consistence, were discharged. A pair of polypus forceps were introduced, and a large body of a similar appearance removed; the cavity was then filled with

* Pathological Anatomy, translated by Drs. West and Townsend, vol. i. p. 626.

* Clin. Lecture in Med. Gaz. vol. xv. p. 25.
† Medico-Chir. Trans. vol. ii. p. 296.
‡ Ibid. vol. xviii. p. 98.
§ Lancet, 1833-4, vol. i. p. 189.
‖ London Med. Gaz. vol. xii. p. 752.

charpie, to which a string was attached. Suppuration followed, and after febrile symptoms, and an accidental hæmorrhage from the wound, the patient recovered so far that there was no doubt of her doing well.

The operation in a case such as I have related would be very simple. A small trocar or grooved needle ought first to be introduced through the rectum, so as to establish the diagnosis by inspection of the fluid. A lithotome or bistoire cachée would enlarge the opening sufficiently to admit a pair of forceps, by which the inner lining membrane of the cyst might be easily removed. Every English surgeon would of course avoid inducing suppuration by the introduction of charpie or other foreign body; and adhesion of the walls of the cyst would probably speedily follow from the regular pressure the pelvic viscera would make upon them.

I am, sir,
Your obedient servant,
THOMAS LAYCOCK,
House-Surgeon to the York County Hospital.

August 1838.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Palmer's Edition of Hunter's Works. Vol. IV.

THE fourth volume contains several detached essays—the best parts of all Hunter's writings—the *membra disjecta* of the giant. Hunter himself does not seem to have been aware of their excellence, for he allowed some of them to lie scattered throughout the current numbers of the Philosophical Transactions; and even those which appeared under the title of the "Animal Œconomy," were not collected with the same spirit and interest as he bestowed on a much inferior work—the Treatise on Syphilis. He little knew that in these papers he stood on the very verge of some of the richest and happiest discoveries of a later day, Had his intellectual sight (to use the poet's figure of speech) been cleared with euphrasy and rue, he might have obtained a distant view of his promised land, and foreseen the conquests to which his labours were already leading the way for future squadrons of inquirers. But his vision was short; the veil of futurity is impervious even to the eye of genius.

Hunter's merit in these papers is great. His researches were pushed to the very borders of the desert. The map of our anatomical knowledge will shew where those borders then terminated; and the discoveries of Sir C. Bell, Sir B. Brodie, Magendie, Mayo, and others, will prove how great have been the territories acquired from those unknown regions. But Hunter stopped at a point beyond which others have since advanced; for he stopped to make sure the data of anatomical facts, without which no subsequent advance could have been successfully carried forward. Cuvier followed upon Hunter, enlarged and established the number of data, and pioneered the ground for the march of British, but more especially of German, commentators, expositors, and physiologists. Harvey had seen a little, and Haller had conjectured more; but it remained for Hunter to mark out the line where the knowledge of his day ended, and where the ignorance began.

In this review we intend to regard Hunter chiefly as an inquirer into the nervous system—the chief points of novelty, research, and success, among later anatomists. On the subject of digestion and the circulation of the blood, he stands the foremost; and the actual result of his experiments on these two points have left little else for those who followed him, except to correct, enlarge, illustrate, and confirm. But on the subject of the nervous system, Hunter did only what ought always first to be done—namely, establish the data upon which to argue and experimentalize. His life ought to have been prolonged beyond the term of ordinary existence, in order to have advanced, at his steady pace, from the circulating system, with the same precision and copiousness of detail, to the nervous centres and their multitudinous ramifications.

In our notice of the third volume, we said that "Hunter had not time to look up to the brain as the nervous centre, nor even to glance at the wide relations of the ganglionic system." In the present volume Mr. Owen expresses his opinion that Hunter was better acquainted with the functions of the brain, both in general and particular, than is usually admitted. But we hold to our first

opinion; for though Hunter enlarges upon the nervous system in several of these papers, and even dilates upon particular vital endowments of parts of that system, yet his attention was evidently engaged upon the subject as an anatomist rather than a physiologist. This was no fault on the part of Hunter, but rather a great merit; for it shews that he was too wise to draw any physiological conclusions before he had established his anatomical premises. Nevertheless, he unavoidably stands lower on this subject than he does on that of the functions of organic and vegetative life—the formation of blood, and its distribution throughout the body.

The comparative physiology of the nervous system, as it is now worked out and demonstrated, from the cerebral ring through which the œsophagus passes in the molluscæ, up to the large and globular mass of brain in the mammalia, has almost reached the extent of anatomical skill, and is fast approaching to completion in a thorough knowledge of its functions. In the papers now before us it is interesting, and, above all, novel, to observe how Hunter had pitched instinctively upon several steps in this great scale, and had investigated and almost understood some of the most remarkable of them individually. For instance, he had already classed the elemental form of brain in the molluscæ, and had moreover (with his wonted genius), aimed at a generalization in comparative anatomy, according to the shape, the size, and the position of the cerebral mass. His generalization is, indeed, too wide, but it included the more determinate and fixed generalization of the present day, without being erroneous as to the particulars which it included. He had also arrived at a special truth as to the functions of the fifth pair: but suddenly, and without any evident reason, he turned aside from the full inquiry, into the very subject which has redounded, and still redounds, so much honour to Sir C. Bell on the motor and sensitive functions of nerves. It is curious to see Hunter pondering on the nervus vagus, and to learn from Mr. Owen (in his Preface) that he had demonstrated the fibrous structure of the brain in 1335 and 1336 of preparations in the College of Surgeons. These insulated discoveries are exceedingly interesting; and if they remained insulated in Hunter's mind, as they evidently did, it is because his view was directed principally to the functions of the capillary system.

Were we asked in what manner we would decide on the real character of any one, we would answer, that it is in moments of action and in efforts of unpremeditated exertion that we discern the native worth and strength of an individual. These detached papers of Hunter are to be viewed in this light, for they shew to what points and to what objects his thoughts were at all times always directed. The paper on the electric property of the torpedo has led to this remark. In this paper, while endeavouring from the testimony of others as well as from his own experiments to ascertain the mode in which the animal discharges its electric shock, his mind was, as usual, bent in search of some higher moving power; but, contrary to the latitude of conjecture and lofty aspiration with which he indulges himself in his other works, especially in that on the blood, he here discreetly curbs his imagination, and halts before "that veil of nature which man (he says) cannot remove." Laying aside conjecture, therefore, he proceeds to the more substantial means of knowledge, and prosecutes the dissection of the torpedo with accuracy and minuteness. The whole subject is still one of deep importance; it yet remains open to fresh investigation, for the similarity between nervous power and electricity is palpable enough, though their identity cannot be proved.

Again, if we refer to "the description of the nerves which supply the organ of smelling," we find the following remarkable words:—"We may reasonably conclude that to every organ is allotted its particular branch, and that however complicated the distribution may be, the complication is always regular"—a conclusion amounting almost to prophecy, which has received its fulfilment in the discoveries of modern science. The statement of the latest writer on this subject is, "that the sensation of a single point evidently depends on the impression being conveyed by means of a single fibre to a single point of the sensorium."—(Müller's Phys. Dr. Baly, p. 601). We have only referred to this paper and quoted from it, to shew how clearly his mind was tracking out the true path which has since been so ably trodden by his successors.

These are a few of Hunter's claims to originality, and, as an original, his claims are ample enough, since he holds several of the chief starting points of modern science. All his detached essays are more than commonly interesting, for in them we find the materials of a great thinker, and they tell the history of the inward man better than the best biography. Accurate, thoughtful, and full of matter of fact, they were too short to satisfy the understanding of the great master, and too small to attract and fix the attention of the multitude.

The editing of this volume has been confided to Mr. Owen, the conservator of the museum of the College of Surgeons, under whose eye the preparations not merely repose but live. In conjunction with the papers on the Animal Economy, several others have been collected from various sources, and now published for the first time. The whole has been arranged according to the subjects of which they treat:—1. Those on Generation. 2. Those on Digestion, Animal Heat, &c. and 3. Those on Descriptive Anatomy; occupying nearly 500 pages of letter-press, and presented for the first time in a collected form. In this manner, Mr. Palmer's edition of Hunter opens and closes with novelty. The Surgical Lectures in the 1st vol. have been brought into light after having lain concealed in the dust and privacy of forty-nine years; and in the 4th vol. the detached essays of the same author have been brought under one focus, instead of their rays being allowed to remain any longer scattered, broken, and rendered useless, in the most opposite directions.

We have thus gone through the entire writings of John Hunter, as they now stand collected and published. Within the narrow space of a weekly journal our movements are limited; but we have bestowed every care in condensing and exhibiting the total amount of his intellectual powers. Without allowing ourselves to indulge in the eulogy of a friendship from early attachment, we have equally claimed what is due to him as a master-builder in his science, and rejected the praise which is never becoming when it is not appropriate. The boundaries within which his genius moved, it has been our endeavour to define; and with a free and rapid pencil to sketch out a marked and characteristic portrait of his works: we have reviewed him as an anatomist, surveyed him as a man, and praised or corrected him as a philosopher; we have extricated him from the embraces of too close a familiarity, and represented him fairly before his fellows, with the virtues and the imperfections common to our mortality.

MEDICAL GAZETTE.

Saturday, August 25, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE LONDON UNIVERSITY.

THE letter of C. L., which was published in a recent number, has put the public in full possession of the chief cause of the disputes which have arisen in the Senate of the London University, and of the plan which the better, because the more independent, portion of its members intend to pursue. Our correspondent charges against us, that the strictures which we have made from time to time on the proposed plans of management, and of the education to be adopted for the medical degree, have occasionally gone too far. We are glad to find that we differ from him only as to the degree, and not the kind of opinion which we expressed; but we do not regret that our remarks were severe, when we find reason to believe that they assisted in some measure to "rouse up a spirit of resistance" which has put the University in such a position that the intellectual members of the Senate can exercise their judgments without fear of being overwhelmed by the political. It would give us sincere pleasure could we venture to hope that the result of the present change will be such an alteration in the system that our future remarks may be those of commendation rather than of censure: we shall express either, and that strongly, though we trust without overstating them, according to the

course which the Senate and the Medical Board pursue. We have always urged the propriety of a University in London, provided it were managed in such a manner as to insure the sound education of those who were virtually excluded from Oxford and Cambridge, and to promote the advance of general science, without inflicting any injustice on existing institutions. We believe that the constitution of the present University will not accomplish this, and therefore we have opposed it. Nevertheless, we readily comply with our correspondent's general invitation for "those who wish well to the projected institution to come forward, to suggest improvements, and to give their best assistance to the good work."

Imprimis, then, it is impossible that any University should work well which is so completely under political influence as the present one is, and we regret to learn is still to continue. It is evident that among a body of gentlemen like those composing the Senate of a University, there will not be more than one or two who are anxious for influence with the minister of the day, or who are placed in a situation to make much use of it; but one such will be sufficient to mar all the plans of the rest, because his activity will secure him attention in official places, where he can make himself useful, and while he holds a monopoly of the benefits of such influence, it answers his purpose to stand any amount of disapprobation on the part of his colleagues rather than resign it. (Many will see that this is drawn from the life). If, on the contrary, there be no further inducement to act on political motives, than the general desire which every one feels to gratify those who are of his own politics, this would have so slight an influence, that each member would allow it to give way to propriety and fairness, and all, in the case before us, would elect a man into their company who would confer honour on them by his talent and reputation, though he might differ from them in religious or political views. However, we do not deny that there is sometimes mischief in a system of self-election, though it is far oftener in theory than in practice, and always less than there is in giving the power into the hands of a minister of the crown, even when he acts upon his own judgment. There is a medium plan which avoids the dangers of both. After the first appointment of the Senate, let the vacancies be filled up by the crown, on the recommendation of the remainder of the members: such recommendations will generally be complied with, and if not, there will be a fair cause and an open field for inquiry into the reasons for the refusal, which, in these days of public vigilance, will not fail to end in justice.

Thus much for the constitution of the Senate; as to the scheme of medical education which they should determine on, we are not disposed to alter the opinion which we have already expressed, and the sum of which is, that the compulsory part of the curriculum should be limited to those subjects which are evidently and eminently practical—that these should be taught in the most practical manner possible, and that the examinations should, in these comparatively few subjects, be severe and scrutinizing.

A more important subject at present is the previous education of graduates in medicine. We agree with C. L. that this should be higher than is generally demanded by our medical boards, and that it should include other subjects than classics, with regard to which we long since pointed out the inutility of that slender knowledge which has usually passed current. Nearly twelve months since we suggested that it would be advisable to require a knowledge of French or German, and of the principles

of the more practical parts of natural philosophy, rather than a useless amount of Latin or Greek; and we hope that some, or all, of these are included in what our correspondent alludes to as the "several branches of general science and literature, beyond what has usually been required at Oxford and Cambridge." We do not know exactly what or how many *several* means; but care had better be taken not to require any that are not generally studied in good modern schools; for it may be at present questionable whether the advantages offered by the London University will be ever sufficient to induce any to whom the choice is open, to prefer it to Oxford or Cambridge, if there is a barrier at its entrance which it may cost them much of either money or labour to surmount.

Indeed, with respect to money, we must say that our correspondent's letter has placed the whole scheme in a different light to any in which we had hitherto been led by its founders to regard it, though we had always suspected that it would take its present direction. It was certainly not expressly declared in the charter that the University was to be established for the benefit of those whose slender means did not permit them to seek the advantages of Cambridge or Oxford, though their talents would have secured them the highest honours in either; but it was a general understanding that one of the main objects of the new University was to open the higher walks of the secular professions to persons of all degrees and of all sects in religion. But it is now evident that the design is that it should fulfil only the latter intention; for the course of education (if our correspondent speaks, as we believe he does, the opinions of the majority of the Council) is to be such as "the sons of persons moving in a higher class of society will alone go through." The question of the expediency of opening all the departments of the profession to persons of an inferior grade in society, involves a great and important principle, which it is not necessary now to discuss; but if the London University do not aim at accomplishing this, we think its utility will be very limited, and we can scarcely see a probability of its obtaining a sufficient number of graduates for its support. The present scheme of education for the degree of doctor of medicine will be scarcely less expensive than that required for the same degree at Cambridge or Oxford; or at least it will be scarcely less to those to whom expense is a subject of principal importance, and who would avoid the extravagant modes of living by which far more money is consumed in the older Universities than by the education which they require. It should be remembered, too, that they (Cambridge and Oxford) are able to hold out to their poorer graduates high rewards for their talents, and for the exertions which their limited means will induce them especially to make: besides exhibitions from the best public schools, there are scholarships, for instance, of from 20*l.* to 100*l.* a year during gradution, and fellowships of from 100*l.* to 300*l.* a year for life, or till marriage. These are something more than merely honourable; they both decrease the outlay for education, and afford a source of maintenance in the weary years of unprofitable inactivity which a physician must spend while he is a junior. Indeed, their value is so great, that unless another University can hold out the same advantages, or compensate for their absence by an equivalent deduction from the expense of education, those to whom money is an important object will never be induced to forego the chance of securing it by these means. In short, *cæteris paribus*, those

Universities will always be most resorted to which offer the best scholarships and fellowships.

Now as yet the London University has not a shilling to give away, nor in these times is there much probability of its ever being largely endowed: consequently, unless the education it requires be much cheaper than at Oxford or Cambridge, those who calculate their expense accurately will not resort to it; and unless its degrees should acquire a much more honourable reputation than theirs (which is not in the slightest degree probable), it will hold out no advantages whatever to any but those whose religious opinions prevent their entrance at the more exclusive universities, and it will thus fall entirely into the hands of dissenters. We would at present only suggest that this would be highly impolitic in a pecuniary point of view, because, as dissenters form only a very small portion of the "persons moving in the higher class of society," it is not probable that a sufficient number of their sons would become physicians at the London University to meet its necessary expenses, more especially as the College of Physicians is open to them without any such preliminary. The case is simply this: those who wish for the most aristocratic diploma will go to Oxford or Cambridge; those who wish for the cheapest diploma, with the greatest respectability, will do the same, in the hope (though it may be an unfounded one) of obtaining a fellowship; those who desire cheapness at the expense of dignity, will go, as they do now, to the German Universities; many more will continue to go to the Scotch; and a part only of the Dissenters will remain for the University of London. Will these be sufficient to maintain either its respectability or its pecuniary expenditure? We suspect not.

We believe, therefore, that its best prospect of success would be to endeavour to create an aristocracy of talent; to make its education at least as cheap as that now required for the license of the Apothecaries' Hall, and to make it so practical and so strict in all useful subjects, that those who obtain the diploma might gain the fair reputation of being the most competent and skilful practitioners of their day; and as the Society of Apothecaries is not very popular, the London University might prove a formidable rival to that body, to which, indeed, both in its constitution, its curriculum, and all other arrangements, it bears a much greater resemblance than to any other of our professional institutions.

There are some other points in our correspondent's letter which we intended to have alluded to, but our present space will not permit us to do so. We believe we can shew that the deductions he has drawn on the subject of the number and length of the courses of lectures are incorrect; and we feel convinced that the best way to secure a reputation for knowledge in the students, is rather to teach them a little well, than a vast amount—ill. We will now only say, that we are not inclined to change the opinions we have often expressed on this subject, and some of which immediately preceded C. L.'s letter, in our number for August 11th.

ANIMAL MAGNETISM.

We beg to direct attention to two letters in our present number, on the subject of Mesmerism; taken in conjunction with Mr. Blake's, published some weeks ago, (see p. 644) they can scarcely fail to convince any unprejudiced person of the mixture of incaution, credulity, and imposture, which characterises these exhibitions. As it has been generally supposed that a committee, having among its members Dr. Roget, Mr. Lawrence, and other gentlemen not pledged by the previous expression of any opinion, have undertaken to examine the subject, we take the opportunity of stating that no such independent committee has been formed.

GLASGOW EYE INFIRMARY.

Clinical Lectures by Dr. Mackenzie.

August 18th, 1838.

Lecture III.

We have admitted on the list, since 1st August, 42 cases—viz., ophthalmia tarsi, 4; ophthalmia porriginosa, 1; dacryocystitis, 2; wounds of eye, 2; ophthalmia traumatica, 2; chronic catarrhal ophthalmia, 2; ophthalmia neonatorum, 1; serofulous ophthalmia, 9; catarrho-serofulous ophthalmia, 2; variolous ophthalmia, 2; rheumatic ophthalmia, 1; catarrho-rheumatic ophthalmia, 1; corneitis, 1; corneitis and iritis, 1; corneitis and retinitis, 1; syphilitic iritis, 1; specks of the cornea, 3; granular conjunctiva, 2; photopsia, 1; amblyopia, 1; amaurosis 2 = 42.

Of all these 42 cases, you will find tolerably ample details in the case-book, and I need scarcely say, that as the integrity of vision is more or less involved in each of these cases, they are all of them of interest. To make even a few remarks on each of them would much exceed the limits we have prescribed to ourselves, and I shall select, therefore, one or two only for our present consideration.

Syphilitic Iritis.

E. W. (No. 8696), a married woman, aged 25 years, admitted 4th August, 1838. Has been labouring under iritis of left eye for five weeks, preceded by an eruption on the head, trunk, and extremities. It seems to have been a papular eruption, and has left small dark blanes. Her throat has been inflamed for about two months, but is not ulcerated. Considerable zonular sclerotitis, discoloration of the iris, irregularity of the pupil, haziness of the cornea, and great dimness of sight. The iris, near the pupil, is of a rusty colour. The same symptoms exist, but in a much less degree, in the right eye. Severe nocturnal pain, preventing sleep, in left eye and left side of head; pulse 108; profuse perspirations in the night. Was bled two weeks ago; applied four leeches to right temple, and used purgatives; but has derived little benefit, except from the leeches. Is nursing a child ten months old, which, she says, is affected with an eruption; as was also her husband.

We had all the ordinary symptoms of iritis well marked in this case. It was more from the eruption than from any peculiar local appearances, that we concluded the case to be syphilitic. The rusty colour of the iris near the pupil, indeed, was pretty distinct; but there were no pustules or tubercles on the iris, such as we often see in syphilitic iritis. The patient denied having had any primary symptoms. These are often so slight in females, that the first thing to alarm them is the breaking out of a syphilitic eruption, and under such circumstances (I mean, when the primary symptoms are so slight as not to attract attention) the eruption is generally papular—not scaly, nor tubercular.

Our patient was bled at the arm on the 4th. You must not trust to leeches, or, as the London practitioners seem to do, to cupping, in iritis. The plan of trusting to cupping appears to be followed to some extent, also, in Edinburgh, whence I have seen several patients who owed, I think, the loss, or at least the irreparable abridgment, of their sight, to having been cupped merely for iritis, when the momentum of the circulation should have been moderated by the much more effective plan of opening a vein. Twelve or fifteen ounces of blood, taken by a pretty large opening from the bend of the arm, has much more effect in checking inflammation of the iris than twice that quantity taken by cupping. You always find the blood buffy in iritis, and more so in syphilitic cases than in any other.

Our patient was ordered to take 4 grains of calomel, with 1 grain of opium, thrice a day. At the same time she was put upon low diet—a most important part of the treatment. Secondary syphilis is an inflammatory disease, and is not to be cured by mercury alone. Indeed, both in primary and in secondary syphilis, it is of the utmost importance to regulate the diet of your patients. Fermented liquors and spirits should be entirely abstained from, and little or no animal food should be allowed. The reverse of this is too often the case. Many patients under a course of mercury are allowed to eat and drink as they like; the practitioner seems too often to think, that he has done all that he ought to do when he has put the patient on mercury; and the consequence is, that very often the disorder marches on from bad to worse, instead of yielding to that medicine. This want of success is attributed to the mercury not agreeing, whereas, the whole cause, in general is, that the antiphlogistic regimen has not been followed. Let a patient with syphilitic iritis eat and drink fully, and there is a great chance that his sight will be permanently deteriorated, or even destroyed.

5th Aug.—Blood buffy; bowels bound. She was ordered to wear a double shade, and had an ounce of castor oil. The purgative was proper, as well on account of the confined state of the bowels, as to aid the action of the mercury; for we find that mercury has always a better effect in syphilitic iritis, when it purges, than when the bowels are confined. You are obliged to regulate the state of the bowels very carefully during a course of calomel.

This medicine is not to be allowed to run off entirely by stool, and therefore we combine it with opium; but, at the same time, experience shows that if the first doses purge freely, the symptoms are moderated in a greater degree than if the mouth merely became affected without any purgative effects being produced.

6th.—Six leeches to the left eyelids.

7th.—Right eye more distinctly affected with iritis; severe supra-orbital pain on left side; mouth affected; frequent stools, with tenesmus. She was ordered to omit the calomel with opium; to take one grain of opium immediately, and repeat it at bed-time if necessary; and to rub the head with a liniment composed of equal parts of tincture of opium and wine of belladonna*.

8th.—Severe supra-orbital pain abated. She attributed this relief to the liniment—a means from which we have often witnessed benefit. Bowels much easier; mouth scarcely so bad. Extract of belladonna had been smeared daily on the eyelids and eyebrows since her admission, but had produced little dilating effect upon the pupils till now that the inflammation had yielded and allowed the pupils to expand. Both pupils were dilated irregularly, lymph having been effused, and having become the medium of adhesion between the pupillary edge of each iris and the capsule of the lens. This was especially the case in the left eye, in which six distinct tags between the edge of the pupil and the capsule were now visible. These were not to be seen before, when the pupil was of its natural size, but were now visible, when the pupil was dilated. Our great object now was to get the medium of these tags (the effused lymph) absorbed, and the tags broken. This is often accomplished if the treatment is commenced early enough; if too late, the adhesions are permanent. The extract of belladonna was continued, as well as the liniment; and next day we had the pleasure of finding the right pupil somewhat more dilated and quite regular, and the left pupil presenting only two tags, the other four having broken across within twenty-four hours. This effect is not to be ascribed to the belladonna alone, but partly to the sorbefacient influence of the mercury. It is a dangerous practice to use belladonna in iritis, without bleeding and mercury. It is apt to make the proper (muscular?) texture of the iris contract, and of course the pupil to expand, so far as that texture is concerned; while the uvea is left adherent to the capsule, and scarcely ever separates from it, whatever further treatment be adopted. The consequence is, that you have a wreath of pigmentum nigrum within the verge of the pupil, forming a variety of cataracta pigmentosa, and permanently impeding vision. Now you scarcely ever see this if you commence the treatment with V. S. and mercury at the same time that you apply the belladonna.

The report of the 9th states, also, that the redness of both eyes was much diminished, and that the eruption was fading. She still complained of tenesmus, and the opium was repeated.

10th.—Left pupil widely dilated, but still the two tags remain.

12th.—The two tags of left pupil form the only remaining objective symptom. Besides continuing the extract and the liniment, she was ordered to drop upon the left eye an aqueous solution of belladonna thrice a day.

17th.—The two tags appear very slender.

Rheumatic Ophthalmia.

As analogous in its character, and in the necessary treatment, to iritis, we may next direct our attention to the case of rheumatic ophthalmia, No. 8709.

John Barnes, a tailor, aged 40 years, was admitted on the 8th August, with rheumatic ophthalmia of the right eye, of a fortnight's standing. In this inflammatory affection of the eye-ball there is no increased secretion from the conjunctiva, no glueing of the eye-lids in the morning, no feeling of sand in the eye, no chemosis; but there is injection of the deep-seated conjunctival network, dimness of vision, and severe nocturnal pain in the region of the eye. These latter symptoms existed in Barnes; the pulse was 80; the pain was situated immediately under the eyebrow; the right pupil was larger than the left. He attributed his complaint to sitting at his work in a draught from a chimney. He had already been bled at the arm, an indispensable remedy in rheumatic ophthalmia; and his mouth was touched with calomel and opium. He had found considerable relief from the application also of a few leeches. In rheumatic ophthalmia the iris is often involved, but it was not so in this case; on the contrary, the pupil of the inflamed eye was dilated; there was an approach, in fact, to the state termed acute glaucoma.

Six leeches were applied to the right eyelids, and he had an ounce of castor oil next morning.

9th.—Vision scarcely so dim; pain and redness less.

A blister behind right ear.

10th.—Greatly better; mouth very sore, as he has continued taking four grains of

* The Vinum belladonnæ is prepared by macerating one ounce of extract of belladonna in sixteen ounces of Sherry wine for a week, and then filtrating through paper.

calomel, with one grain of opium, each night, except last night.

Castor oil repeated.

11th.—Appearance of right eye still more healthy, but vision still misty, and darkened with muscæ volitantes.

Alum gargle.

16th.—Fewer muscæ volitantes; is still unable to read with right eye; right pupil less than left, but regular.

Extract of belladonna to right eye-lids.

17th.—Right pupil considerably dilated, and quite regular.

In rheumatic ophthalmia, as well as in iritis, we observe a zone of vessels running in radii towards the edge of the cornea. This symptom is sometimes termed zonular sclerotitis, but not very correctly. No doubt the radiating vessels lie on the sclerotica, but they belong not so much to the sclerotica as to the conjunctiva.

The conjunctiva is a very vascular membrane: its vessels are chiefly capillary; that is, the greater part of them do not continue to diminish in diameter as they proceed, but maintain the same diameter throughout. In inflammation they fill with red blood, so as to become manifest; but even in health they carry red blood. In fact, the existence of vessels in any part of the body, traversed only by colourless blood, is merely a conjecture, and has not been demonstrated.

The conjunctiva receives its arteries chiefly from the lacrymal and oculo-muscular branches of the ophthalmic, on the one hand; and, on the other, from the superior and inferior palpebrals. The branches from these several sources anastomose, and form over the sclerotica two networks—a superficial and a deep-seated.

The superficial conjunctival network results from the arteries furnished by the palpebrals and the lacrymal, and divides into ramifications, which advance in a serpentine direction towards the edge of the cornea, where they form anastomotic arches, and communicate with the deep-seated network.

The deep-seated conjunctival network is formed by much smaller vessels, arising partly from the oculo-muscular, and partly from the anterior ciliary arteries, before these penetrate into the sclerotica.

The ramifications of the two net-works form, close around the cornea, a vascular wreath, from every part of which numerous very delicate ramifications arise, and direct their course towards the centre of the cornea. These ramifications we see enlarged in corneitis.

The vessels of the conjunctiva, although they carry red blood, are not visible in health, from their small size (perhaps the 2000dth or 3000dth part of an inch in diameter), and from the globules of blood which are coursing through them, and which measure about 1-4000dth part of an inch in diameter, running probably in a single series. It is only when the circulation is impeded and the red globules accumulated, that the conjunctival vessels become evident; the superficial conjunctival net-work presenting a reticulated, and the deep-seated a radiated or zonular arrangement, the one evidently lying over the other.

In most ophthalmiæ both net-works are more or less injected. In catarrhal ophthalmia it is chiefly the superficial; in rheumatic, chiefly the deep-seated.

It is remarkable, that out of the forty-two cases taken on the list since 1st August, there have been no cases of catarrhal ophthalmia. This shews that the disease is not at present epidemic. We have had one case, however, and that a pretty severe one, of catarrho-rheumatic ophthalmia—a disease in which there is a union of puro-mucous conjunctivitis with inflammation of the deeper textures, the sclerotica, cornea, and iris.

Catarrho-rheumatic Ophthalmia.

Daniel Macphee (No. 8715), a labourer, aged 43 years, was admitted 13th August, with catarrho-rheumatic ophthalmia of left eye, of a fortnight's standing. Conjunctiva much inflamed, and a considerable ulcer near lower edge of cornea. Severe nocturnal pain between eye and brow. Pulse 96. Skin hot in the night; tongue foul; bad taste in the mouth. He was ordered to be bled at the arm; to take 6 grains of calomel, with 1 grain of opium, at bed-time; and 1 ounce of sulphate of magnesia next morning. A solution of 4 grains of nitrate of silver, in 1 ounce of distilled water, was applied to his eye; and he was directed to bathe his eye thrice a-day with a solution of 1 grain of muriate of mercury in 6 ounces of water, with the addition of 1 drachm of belladonna wine: this collyrium to be used tepid, and after being employed for fomenting the eye externally, to be allowed to run in upon the conjunctiva.

14th.—Fainted after a cup of blood was taken away. Pain was very bad during the night. Eye less vascular.

Calomel with opium and salts, to be repeated. A blister behind left ear.

15th.—A better night.

Three leeches over nasal vein. Red precipitate salve (12 grains to the ounce of simple ointment) for smearing the edges of the lids at bed-time.

16th.—Ulcer healing. Eye easier. Conjunctiva still much inflamed, especially where it lines lower eyelid.

17th.—Conjunctiva less inflamed.

In this variety of ophthalmia, ulcera-

tion of the cornea is a common and a dangerous symptom. We employ V. S. for the deeper part of the disease, for this is a remedy which, in general, is not necessary in mere catarrhal ophthalmia; and the same with the calomel and opium. The solution of nitrate of silver we use once daily, for the conjunctival part and for the ulcer of the cornea. You have seen how well this combination of remedies has answered in this patient, whose eye we may now regard as out of all danger.

A *FEUILLETON* FOR FORENSIC MEDICINE.

APPEARANCES PRESENTED BY A BODY SUPPOSED TO HAVE BEEN BURIED ABOUT 200 YEARS.

A "Constant Reader" has called our attention to some curious and interesting particulars, elicited at an inquest on the body of a man supposed to have been buried about 200 years, and which are contained in a late number of an Irish journal (Cork Reporter), dated Bandon, Aug. 10th; and "though the medical details," as he observes, "it is to be regretted are somewhat meagre, yet they possess sufficient interest to be made available to the student of forensic medicine and the medical jurist, and deserve to be reseued from the ephemeral productions of the press." In accordance to his wishes, we subjoin the following extracts, which he has sent us:—

This inquest took place on Monday, the 30th ult., before Mr. Francis Baldwin, Coroner, upon view of the body of a man unknown, which was found in the bog of Munnifeugh, parish of Clondrohid, barony of West Muskerry. It was stated in evidence that, on Wednesday, 25th of July, while some labourers were employed in cutting a fresh trench, and had gone about four and a half sods deep, two of them discovered about nine or ten sticks irregularly placed upright in the bog. On digging a sod deeper, they came to a large stone, which was about half a hundred weight. They immediately displaced it, and underneath was another turf, on the removal of which the body of a man, placed on its back, at full length, presented itself. It was in a perfect state of preservation; with a cap, the witness stated, on the head, but which most probably was the scalp. The length of the body he thought measured about six feet. The place had no appearance different from the surrounding portion of the bog, and there were no signs of its having been recently disturbed: this land had the name of *Knuckpagree-a-Denaha Looney*, or "Looney's Mount," and a rumour existed in the neighbourhood that it was so called from the fact that a man, named Denis Looney, who had hanged himself there, was buried on the spot.

Another farmer swore that he was about 60 years of age, and had lived in the vicinity ever since he was born, and had been in the habit of cutting down grass on the particular spot in question for years back, but that he had never observed any difference between its surface and that of the contiguous portion. He also deposed that he had often heard his father say, that he had heard from *his* father that there was buried about that spot a person named Denis Looney, who had hanged himself; and further, that he had often been told by a neighbour, who was then very old, that she had heard her father say, that her grandfather had acted as one of the jurors on the inquest upon the body of the above, who had been interred where the deed took place. This woman was said to be 90 years of age.

The medical gentleman who was examined stated that he found the body with the face much disfigured; the bones of the skull separated; the chest laid open; and the integuments and muscles of the back in some degree presenting their natural condition. The bones, when handled, were converted into pulverizable matter. He also deposed that, from the appearances presented, he believed the body must have lain in the bog for a series of years.

From the statement of Mr. Baldwin it appears that the disfigured condition of the body was caused by its having been carried about with spades, as the country people would not put their hands to it, it being to them an object of superstitious terror; and that every part uninjured was in high state of preservation. Mr. B. stood upon the abdomen, when not the slightest impression was produced on it; and when he struck the thighs with considerable force with a stout cane, it rebounded, as if from some hard substance. On a portion of the integuments being raised from the upper part of the chest, the bones and muscles appeared as if the body had only been a few days buried, and the entire felt hard and firm.

The coroner likewise made an accurate admeasurement of the body, which was as follows:—

	Ft.	in.
From the top of the shoulder to the sole of the foot.....	5	2
Girth of body across the navel ...	3	2
„ „ upper part of the thigh.	2	0
Distance between the great trochanters posteriorly	2	0

It may here be remarked that it is a well-ascertained fact that bogs contain a large portion of tannin, and are said to have been formerly used for the purpose of tanning,—a circumstance which, if correct, would account for the high state of preservation in which the body was found.

OF

DRUGS ON SALE IN THE ENGLISH MARKET,

With their Prices and several Duties.

(From the Official Returns, August 21, 1838.)

	Price.		Duty.		Duty Paid.	
					In 1838 to last week	Same time last year.
	£ s. d.	£ s. d.		s. d.		
Aloes, Barbadoes, D.P. c	12 0 0	to 30 0 0	B P. lb	0 2	64,493	55,224
Hepatic (dry) BD..... c	5 0 0	14 0 0	F. lb	0 8		
Cape, BD. c	1 16 0	2 1 0				
Anise, Oil of, German, D.P.... lb	0 9 6	0 9 6	F. lb	1 4	133	251
E. I. ..... lb	0 6 0	0 6 0	E. I.	1 4	653	1,104
Asafœtida, B.D. c	2 10 0	5 0 0	c	6 0	38	22
Balsam, Canada, D.P. ..... lb	0 1 3	0 1 4	lb	0 1	5,896	1,350
Copaiba, BD. ..... lb	0 3 6	0 3 6	c	4 0	84	144
Peru, BD. ..... lb	0 4 3	——	lb	1 0	1,798	507
Benzoin (best) BD..... c	25 0 0	50 0 0	c	4 0	72	94
Camphor, unrefined, BD..... c	11 0 0	——	c	1 0	380	292
Cantharides, D.P. ..... lb	0 4 6	——	lb	1 0	9,628	17,302
Carraway, Oil of, D.P. ..... lb	0 8 0	8 6	lb	4 0	580	1,370
Cascarilla or Eleutheria Bark, D.P.C.	3 10 0	——	lb	0 1	4,579	2,858
Cassia, Oil of, BD. ..... lb	0 6 6	0 6 6	lb	1 4	2,908	2,283
Castor Oil, East India, BD ..... lb	0 0 6	0 0 10	c	1 3	4,023	4,783
West I. (bottle) D.P. 1½ lb	——	——		——		
Castoreum, American ..... lb	0 17 0	0 18 0	lb	0 6	782	203
D.P. Hudson's Bay ..... lb	0 18 0	1 0 0				
Russian ..... lb	none					
Catechu, BD. Pale c	1 2 0	——	c	1 0	22,378	22,664
Dark	3 0 0	——				
Cinchona Bark, Pale (Crown).... lb	0 2 0	0 3 6	lb	0 1	84,535	100,309
BD. Red ..... lb	0 2 0	0 4 0				
Yellow ..... lb	0 3 0	0 3 3				
Colocynth, Turkey ..... lb	0 2 6	0 4 0	lb	0 2	7,649	6,213
D.P. Mogadore ..... lb	0 3 0	——	lb	0 2	17,939	8,812
Calumba Root, BD. c	0 12 0	1 15 0	lb	0 6	16,863	22,356
Cubebs, BD. c	3 10 0	3 15 0	c	4 0	74	62
Gamboge, BD. c	5 0 0	15 0 0	c	4 0	326	368
Gentian, D.P. c	1 4 0	——	c	6 0	39	52
Guaiacum, D.P..... lb	0 1 0	0 1 8				
Gum Arabic, Turkey, fine, D.P... c	8 0 0	9 0 0	c	6 0	4,201	1,585
Do. seconds, D.P. .. c	5 0 0	7 0 0				
Barbary, brown, BD. c	2 6 0	2 8 0				
Do. white, D.P..... c	4 15 0	——				
E. I. fine yellow, BD. c	2 14 0	3 0 0	c	6 0	3,535	1,585
Do. dark brown, B.D. c	1 15 0	2 5 0				
—— Senegal garblings, D.P. c	3 15 0	4 0 0	c	6 0	11,924	1,300
—— Tragacanth, D.P. c	8 0 0	12 0 0	c	6 0	388	285
Iceland Moss (Lichen), D.P. .... lb	0 0 2½	0 0 3	lb	0 1	5,179	6,690
Ipecacuanha Root, B.D. ..... lb	0 1 9	0 2 0	lb	1 0	9,923	7,747
Jalap, BD. ..... lb	0 1 7	0 1 10	lb	0 6	22,552	39,391
Manna, flaky, BD. ..... lb	0 4 0	0 5 6	lb	0 3	4,662	14,947
Sicilian, BD. ..... lb	0 1 7	——				
Musk, China, BD. ..... oz	1 0 0	1 8 0	oz	6 0	1,251	1,352
Myrrh, East India, BD. c	5 0 0	14 0 0	c	6 0	110	92
Turkey, BD..... c	2 0 0	11 10 0				
Nux Vomica, BD..... lb	0 8 0	0 9 0	lb	2 6	740	1,120
Opium, Turkey, BD. ..... lb	0 15 0	——	lb	1 0	17,432	24,226
Peppermint, Oil of, F. BD..... lb	1 1 0	——	lb	4 0	603	809
Quicksilver, BD. ..... lb	0 3 6	——	lb	0 1	215,038	155,022
Rhubarb, East India, BD..... lb	0 2 6	0 3 3	lb	1 0	22,230	29,025
Dutch, trimmed, D.P. lb	0 3 6	0 4 0	F. lb	1 0	4,389	4,319
Russian, BD. ..... lb	0 8 3	——				
Saffron, French, BD. ..... lb	0 18 0	——	lb	1 0	3,492	3,450
Spanish ..... lb	0 19 0	1 0 0				
Sarsaparilla, Honduras, BD..... lb	0 1 0	0 1 9	lb	0 6	77,197	75,283
Lisbon, BD. ..... lb	0 2 0	——				
Scammony, Smyrna, D.P..... lb	——	——	lb	2 6	5,419	5,676
Aleppo ..... lb	0 12 0	0 15 0				
Senna, East India, BD. ..... lb	0 0 3	0 0 4	E.I. lb	0 6	50,279	63,019
Alexandria, D.P..... lb	0 1 9	0 1 10				
Smyrna, D.P. ..... lb	0 1 0	0 1 3	Other sorts	0 6	42,777	063
Tripoli, D.P. ..... lb	0 1 0	0 1 3				

‡§‡ BD. In Bond.—c. Cwt.—B. P. British Possessions.—F. Foreign.—D. P. Duty paid.

SOUNDS OF THE HEART.

To the Editor of the Medical Gazette.

Sir,

Permit me to reply to a statement contained in our journal of 28th July ult. (page 696, note); an opinion being there attributed to me which not only is not mine, but which I consider erroneous—viz. "that the second sound (of the heart) is caused by the flapping of the semilunar valves." I could have wished that the writer had quoted the words of the thesis which he refers to, as, although *printed*, it is not in the hands of the public, nor easily obtained. My opinion, which has been published in different periodicals, besides in the Preface of my First Principles of Medicine (so favourably noticed by you, in your number of 21st July ult.), is, that the healthy sounds, both first and second, of the heart, are produced by the tension of the valves. Now as flapping is the result of looseness, and tension is the contrary, I assert that my opinion was misstated. The first sound has been attributed to flapping by another author, but not by me.—I remain, sir,

Your obedient servant,

A. Billing.

Bedford Place, Aug. 16, 1838.

DEATH OF A CHILD FROM A HERRING BONE.

An inquest was held on Tuesday, the 21st instant, at Ipswich, on the body of a male child, aged 1½ years, whose parents are named Brook, and reside at Tunstall, its decease being occasioned in a doubtful manner. It appeared that on the Friday before, while the family were at breakfast, the child took up the tail of a herring, and, in sucking it, swallowed a portion of the vertebræ, which, getting into the larynx, occasioned suffocation the following night. The real cause of the poor little fellow's death, however, was not unravelled, until the jury suggested that a post-mortem examination should be made, which was accordingly done by Mr. Barker, when a piece of the back-bone of a herring was found adhering as above described.

COLLEGE OF SURGEONS.

ADJUDICATION OF THE JACKSONIAN PRIZE.

The Jacksonian prize for the year 1837 has been adjudged to Mr. Samuel Gaskell, of the Royal Infirmary, Manchester, for a "Dissertation on the Nature and Processes of Suppuration and Ulceration."

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, August 9th.

Thomas Glascott Vandrey, Camborne, Cornwall.—Henry Charles Stewart, London.—Francis Wakefield.—John Duncan Stuart.—Richard Elworthy Tanner, South Molton, Devon.—John Coales, St. Alban's.—Theodore Parrett, Newport, Monmouthshire.—Henry Jon. Bullen, Bampton.—Edward Monks, Oxon.

WEEKLY ACCOUNT OF BURIALS.

From Bills of Mortality, *Aug.* 21, 1838.

Abscess	1	Hæmorrhage	1
Age and Debility	18	Heart, diseased	1
Apoplexy	1	Hooping Cough	1
Asthma	1	Inflammation	7
Childbirth	1	Bowels & Stomach	2
Cholera	1	Brain	2
Consumption	32	Lungs and Pleura	3
Convulsions	24	Influenza	2
Croup	1	Jaundice	1
Dentition or Teething	2	Liver, diseased	1
Diabetes	1	Mortification	1
Dropsy	11	Small-pox	19
Dropsy in the Brain	5	Spasms	1
Dropsy in the Chest	3	Tumor	2
Fever	14	Unknown Causes	48
Fever, Scarlet	1		
Fever, Typhus	2	Casualties	5
Gout	1		

Decrease of Burials, as compared with the preceding week . . . 7

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

August.	Thermometer.		Barometer.	
Thursday . 16	from 40 to 70		30·06 to 30 00	
Friday . . 17	53	67	30·07	30·10
Saturday . 18	52	71	30·14	30·12
Sunday . . 19	52	74	29·96	29·80
Monday . . 20	48	67	29·77	29·55
Tuesday . . 21	56	69	29·34	29·33
Wednesday 22	52	65	29 34	29 22

Wind, S.W.

Except the 18th and afternoon of the 20th, generally cloudy, with frequent and heavy showers of rains.

Rain fallen, ·275 of an inch.

Charles Henry Adams.

NOTICES.

If our correspondent near Norwich will favour us with a specimen of the proposed set of papers, we shall send him an immediate answer.

Mr. Whitehouse. We shall be most happy to receive such a paper. The casts have been sent to the printer's, where his messenger will find them. The present notice was accidentally omitted this day fortnight.

Mr. Terry's letter next week.

Wilson & Son, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 1, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXVII.

Diseases of the Heart (continued).—Inflammation of the Pericardium.—Anatomical Characters of Pericarditis.—General Symptoms; their Uncertainty.—Connexion of Inflammations of the Heart with Rheumatism.—Physical Signs of Pericarditis.—Friction Sounds.—Signs of Effusion, &c.—Varieties of the Disease.—Adhesions of the Pericardium; their different Kinds and Effects; their Signs.—Inflammation in them.—Inflammation of the Endocardium.—Anatomical Characters of Endocarditis.—General Symptoms.—Pathology and Physical Signs.—Frequent Occurrence of Endocarditis.—Its constitutional Effects.—Inflammation of the Substance of the Heart.—Symptoms and Signs.

Inflammation of the heart produces different symptoms and consequences, according to the structure which it chiefly affects. It commonly attacks the serous and fibrous membranes either covering the exterior of the organ, or lining its interior, and by their folds forming the valves; more rarely it affects the muscular fibres, or the cellular texture which unites them. Hence the division of inflammation of the heart into the species, *pericarditis*, *endocarditis*, and *carditis*. The two first are commonly combined, but not constantly; and we may well consider them separately.

Inflammation of the pericardium, as of the pleura, tends to produce an effusion of serum and coagulable lymph on the free surfaces of the membrane; by these products, as well as by other changes of the vital and physical properties of the heart, pericarditis develops various symptoms and signs, by which we may ascertain its existence.

I do not consider it necessary to describe to you at length the various morbid appearances which are found after death in the inflamed pericardium. They vary like those of pleurisy, and I refer you to the descriptions which I have given of them. There is this difference, that the motion of the heart being constant, and not like that of the lung interrupted by the effusion, it in various ways modifies the deposition of lymph, according to the quantity of accompanying liquid; and thus are produced those transverse ridges and furrows, honeycomb or reticulated, and shaggy or villous appearances, which are seen in the lymph of an inflamed pericardium, and which Laennec imagined to be a peculiar kind of new structure. The liquid may be serous, sero-purulent, or purulent; with all the varieties that are found in the liquid of pleurisy.

The general symptoms are often very obscure. There may be pain below the left breast, shooting to the sternum, back, and left arm; perhaps with tenderness on pressure on or between the cartilages of the fourth, fifth, and sixth ribs, or on the epigastrium; but all such pain or tenderness is sometimes entirely absent, and is more commonly produced by pleurisy than by pericarditis. Neither is dyspnœa nor palpitation always present, although they are sometimes extremely distressing, and give the patient's countenance an expression of anxiety, uneasiness, or restlessness, which is very characteristic, and may be perceived even when the patient does not acknowledge the existence of the sensations. I have often known pericarditis to exist without producing in the chest any unusual sensation whatever; and even the

pulse in the first stage has been below its usual frequency. Besides this, these symptoms are occasionally disguised by delirium, or a state of incoherent stupor, which draws all the attention to the brain, although it is only secondarily affected. Under these circumstances it is not wonderful that the existence of the disease should have been often overlooked, and known only through an examination after death, or by the production of a permanent organic disease, which is more unequivocal in its symptoms.

But pericarditis cannot exist for any length of time, and certainly not to a dangerous extent, without producing physical signs that are quite characteristic. That these have often escaped notice, even of auscultators, is because they have not been sought for. It is of great importance, therefore, to examine the region of the heart in all cases in which there is the probability of its being affected, whether any symptoms have manifested themselves or not. The cases in which you should especially be on the look out for the signs of pericarditis are those of rheumatism in all its forms, but particularly the acute kind, which is generally called rheumatic fever; the subjects of mechanical injuries, or of pleurisy or pneumonia affecting the left side; and all doubtful instances of difficult breathing, especially when accompanied by œdema, general or partial. Of the causes of pericarditis, rheumatism is by far the most common; but it still more frequently produces endocarditis. In fact, although I do not agree with Bouillaud's opinion that these inflammations are essentially a part of rheumatism, I can confidently state that I have found signs of endocarditis or pericarditis, or both, to a greater or less extent, in fully three-fourths of the cases of rheumatism which I have examined in the last three years, although in little more than half that proportion was there any complaint of pain in the chest, palpitation, or dyspnœa. That the membranes of the heart are frequently affected in cases of rheumatism has been long acknowledged by English practitioners. In the first volume of the Medico-Chirurgical Transactions, Sir David Dundas published a paper expressly pointing out this fact; and Dr. Wells has given many illustrations in the third volume of the Transactions of a Society for the improvement of Medical and Surgical Knowledge, published in 1812. Yet M. Bouillaud, not familiar with English medical literature, or having consulted only recent works, which were imperfect in their information, has imagined himself to be the first to point out the connexion between rheumatism and diseases of the heart. We are, however, indebted to him for much valuable knowledge with regard to diseases of the heart in general, and for better information than we possessed before the publication of his work, as to the kind and extent of share which rheumatism has in producing diseases of the heart. I must refer you to his work, which is the most complete on the subject, although it is vitiated by a spirit of hasty and excessive generalization, which too much prevails in the Broussaian school of medicine. I have great pleasure in referring you also, for the most complete digest of modern information on diseases of the heart, to the fourth part of Dr. Copland's admirable Dictionary, which has just appeared (May 1837.) Each subject that this enterprising writer undertakes he masters so completely, as to outdo even those who have made it the chief object of their study. The observations which I shall have to offer you are chiefly the results of my own experience and study; for a complete or historical view of the subject I refer you to Dr. Copland.

The first physical sign of inflammation of the membranes of the heart is an increased loudness of the natural sounds, with an impulse stronger and more abrupt than usual. The pulsations are generally but not always accelerated; sometimes they are slower than usual, and then they are stronger; and the first sound, although loud, is prolonged even when there is no murmur. Sometimes there is now and then a reduplicated pulse, or some other slight irregularity; but the beats are for the most part regular, and they owe their loudness to their increased vigour. In this state of the signs the inflammation may be in the pericardium, endocardium, or both, without exhibiting any thing distinctive of its seat for from twenty-four to forty-eight hours, and may sometimes be removed by general and local blood-letting, without proceeding further. But in that time, and perhaps in less, new signs commence, which announce changes which distinguish the seat of the inflammation. Those of endocarditis are various murmurs, which are commonly the first to shew themselves; but we must consider them afterwards. Those of pericarditis are various sounds of superficial friction, which are quite characteristic. At first this sound is soft and rustling, like the rubbing together of two pieces of paper or silk stuff; and it may accompany only part of the natural sounds, from which, however, it is obviously distinct, in being much more superficial. It is generally heard first about the middle of the sternum, or to the left of it, corresponding with the base of the heart, or the attachment of the auricles. It afterwards in-

creases in loudness and duration, being heard beyond the immediate region of the heart, and accompanying not only the periods of the natural sounds, which it disguises, but also the interval between them. It thus gets a sort of continuous jogging rhythm, corresponding with the movements of the heart, which is like that of the saddle when one rides on horseback; and when, as it generally happens, the friction sound becomes harder, and more like the creaking of leather, its resemblance to the noise of a new saddle is quite ridiculous. In some cases the noise is crackling, like that of crumpling dried membrane or parchment. We owe the discovery of this valuable diagnostic sign to M. Collin; and it has more recently been described by Reynaud, Drs. Stokes and Watson, and others. I have had many opportunities of observing it in its different modifications, and of verifying its accuracy by examination after death, during the last five or six years. I only wonder why Laennec and Bouillaud did not recognise it, even after M. Collin had described it in 1824. They must have mistaken it for the double grating murmur of diseased aortic valves, from which, however, its more superficial, diffused, and less regular character, would easily distinguish it. The confirmed leather creak sound is certainly caused by the rubbing of the rough coating of lymph on the pericardium proper, and on its sac; but I have some doubts whether effusion of lymph is necessary to produce the rustling sound, which occurs earlier, and whether it may not be caused by a mere turgescence of the vessels, and deficiency of the natural serous lubrication, which might be supposed to take place in the early stage of inflammation*. The natural sounds of the heart are completely disguised by the friction sound; but they may sometimes be heard at the top of the sternum, and in the carotid arteries, and they will be often attended with a murmur; this depends on the simultaneous existence of endocarditis, and will be noticed presently.

The pericarditic friction sound does not commonly continue for many days in succession. Either the lymph is absorbed, or it is converted into false membrane, which forms a bond of adhesion more or less perfect between the heart and the sac, in which case the sound gradually ceases; or if the inflammation continue in a low degree, serum is poured out in such abundance, that the heart moves free in the distended sac, and no longer rubs against it. In either case the friction sound becomes less constant, accompanying only the stronger pulses, and gradually ceases. In case of adhesions it is commonly heard longest below the left breast; and when it is terminated by liquid effusion, it generally continues latest near the sternum, corresponding with the base of the heart, which is less easily separated from the sac than the apex. In the latter case change of posture, as by leaning forward, will also sometimes reproduce it when it has ceased. I have known it in a few cases to continue for more than a fortnight in conjunction with the signs of liquid effusion; for this effusion does not always proceed to an extent sufficient to separate the heart from its sac, particularly at its anterior surface.

You now know enough of the principles of acoustic signs to perceive that the effusion of serum in the pericardium must produce dulness on percussion in the region of the heart; whilst, if copious, by removing the heart from the walls of the chest, it generally renders the sounds more distant, and more or less impairs the impulse. The dulness may be limited to an area of two or three inches at the lower part and to the left of the sternum; or in case of extensive effusion it may occupy the whole front of the left side of the chest, even as high as the second rib, and extend even to the right of the sternum. There being no sound of respiration, and sometimes a prominence of the ribs in these parts, the case might be taken for pleuritic effusion, but that the sounds of respiration and percussion are still good in the back and in the axilla, and are not materially varied by the change of posture. There does not seem to be any fixed rule which regulates the displacement of the heart by liquid in the pericardium; for although, as I have just told you, this organ is commonly displaced in proportion to the quantity of the effusion, so that the friction sound is stopped, and the impulse and natural sounds are rendered distant, yet I have met with cases in which extensive dulness on percussion indicated copious effusion, yet the friction sound and impulse still continued strong at the sternum. We may discover when the sounds are rendered weak merely by displacement, on listening to them at the top of the sternum and in the carotid or subclavian arteries, where they may be heard of their usual intensity. This enables us to dis-

* Some experiments have been recently performed for the British Association, by Drs. Clendinning, Todd, and myself, with a view to determine the physical cause of the friction sound. They have led us to the conclusion that inflammatory injection of the pericardium is insufficient to produce it; but the smallest effusion of lymph, and even slight ecchymoses, or effusions of blood under the pericardium proper, from acupunctures, may cause distinct rustling or rubbing sounds. A very good example of the leather sound was presented by a heart which was found to be covered by a thin film of quite soft lym

tinguish this case from that of a greatly enlarged heart acting very feebly: in this instance there would be extensive dulness on percussion with weak sounds and impulse, but the sounds would also be weak in the course of the arteries. When the liquid in the pericardium is scanty, it may only partially intercept the impulse, and modify rather than obscure the sounds. Thus in the standing or sitting posture the impulse at the apex is diminished, and the sounds assume that more drum-like character that I have before described as the effect of an intervening layer of liquid; whilst at the sternum both sounds and impulse may be as usual. In the supine posture similar differences may be presented in an altered relation; the drum-like sounds and diminished impulse corresponding with the dulness on percussion.

With all the signs of extensive disease that I have been describing, the general symptoms may be combined in most varying and uncertain proportions; being sometimes well marked, and necessarily drawing the attention of the practitioner to the seat of the disease; in other cases being so trivial or equivocal as scarcely to be acknowledged, even by the patient. Some feeling of dyspnœa or faintness, especially on moving, is the most constant symptom; and this is generally accompanied by irregularity of the pulse. The contractility of the heart, which was in the first instance exalted by the inflammation of its membranes, ultimately becomes impaired; the action loses its rhythm, becomes sometimes palpitating, sometimes defective, and verges on syncope; syncope, in fact, sometimes does occur, and may suddenly terminate the patient's life. But this is by no means a common case. I have met with but few cases of pericarditis which first attacks a sound heart, proving fatal in its acute or chronic form; but I have known several partially or apparently recover from a first attack, after some months or even years exhibit signs of organic disease of the heart, and ultimately sink under a subsequent attack of pericarditis supervening on this. A large proportion of cases, even where the disease was very extensive, recovered entirely, as far as their subsequent history was ascertained. I do not, therefore, look on pericarditis as so formidable a disease as it has been generally considered to be. In fact, its existence has hitherto been known chiefly by those instances in which it has either proved immediately fatal, or has become conjoined with serious organic disease, which constitute but a small part of the cases which we can detect by physical diagnosis.

The course of pericarditis is, like that of pleurisy, very much determined by the nature of the effusion. When this is moderate in quantity, and composed equally of serum and healthy lymph, it may all be absorbed, and the cure completed in a short time. Such we may conclude to be the case when the friction sound and dulness on percussion last only two or three days, and no sign of disease of the heart remains. When the lymph is more abundant, adhesions will form between the heart and pericardium; and the character and effects of these adhesions will vary mainly according to the quantity of the liquid effusion, and the manner in which this protracts the period of their formation. If the liquid be moderate in quantity, or soon diminish so as to bring the lymph early into adhering contact before its plastic qualities are impaired, and whilst the heart is still vigorous and free in its action, the adhesions formed will then be loose and mobile, composed of yielding cellular membrane, which may very little, or not at all, interfere with the motions of the organ. Such adhesions have often been found after death in cases where their existence had not been suspected during life, not having in any notable degree impeded the heart's action. But if the liquid effusion be abundant, and persist long, there is great fear that the lymph effused will not be formed into adhering contact until its vitality is impaired, when it is capable of forming only denser and more fibrous adhesions, disposed to contract further, and when, the action of the heart being feeble and irregular, it cannot adapt them to its healthier and freer motions; in such cases the adhesions formed, whether partial or universal, will for ever shackle the motions of the heart, and constitute a serious form of organic disease. Such adhesions are most injurious when they confine the apex, naturally the most moving part: they do less injury when they attach the base only; but in this case they are often combined with internal organic disease, arising from endocarditis. Not only the quantity of the liquid effusion, and the consequent slow formation of the adhesions, may conduce to their injurious character and effects, but probably in some measure the nature of the inflammation also. I think that I have seen worse results from pericarditis occurring in subjects affected with fascial rheumatism, than from that supervening in those with the more acute disease confined to the joints. Weakly and aged persons are likewise more likely to suffer from the longer duration of the inflammation in them, and the less assimilable character of its products.

From what I have just said, you may infer that *adhesions of the pericardium* according to their character may or may

not produce signs by which they may be distinguished. When they are mobile, and composed of yielding cellular texture, they do not alter the positions, motions, or sounds of the heart, and therefore yield no signs. When, in consequence of their closeness and rigidity, they do interfere with the heart's motions, and thereby irritate it into inordinate action, it may still be difficult to distinguish this inordinate action from that from other causes, so long as the heart is nearly covered by the lung. I have sought in vain for the "jogging or tumbling" motion which has been stated to be characteristic of an adherent heart. Such motion results from irregular action, often exists without any adhesions, and in some measure depends on the motions of the lung differently affecting successive irregular pulsations. But it often happens in case of close adhesions, that prior to their formation the pericardial sac has adhered in its distended state to the walls of the chest at the left of the sternum, so that when the heart also adheres to the sac, it constantly pulsates in close contact with these walls. This combination of circumstances gives us very appreciable signs. In the first place the motions of the heart may be seen and felt much more plainly and widely than usual, drawing in the intercostal spaces at each systole. Then these motions, instead of being as usual intercepted by the expansion of the lung in a full inspiration, are always close to the walls of the chest; for these walls, instead of, as usual, rising from the heart upwards and outwards at each inspiration, carry the heart with them in all their movements. Under these circumstances, therefore, there will be, proportioned to the adhesion and the size of the heart, a space in which the pulsations are always felt, and the sound on percussion is always dull in every stage of respiration and in every posture of the body. Compare this with what I told you of the shifting, varying force and position of the pulsations and the sound of percussion in the region of the heart in health, and you will understand the difference; and the distinction extends also in part to other diseases, which enlarge the heart, but do not fix it to the walls. When the heart is thus generally adherent to the pericardium, and this to the diaphragm and walls of the chest, the enlargement of the organ which very commonly ensues cannot readily take place as usual, downwards and to the left, but it proceeds upwards and outwards, carrying with it the walls of the chest, to which it gives a remarkable projection about the ends and cartilages of the middle ribs. I have seen this accompanied by a retraction or hollow at the epigastrium. In other cases again, from the adhesions being more partial, the enlargement may take place laterally, and bring the pulsating apex of the heart far to the left side. Various other changes of position may arise from different circumstances, especially when the pericarditis has been conjoined with pleurisy.

It is a common notion among pathologists, that an adherent pericardium is in some degree secure from attacks of inflammation; but this is a great mistake. I have frequently found in both layers of the pericardium, and in the false membrane which unites them, traces of inflammation, such as punctuated and striated redness, softening of the membrane, and an effusion of lymph and a little serum, or a sero-purulent liquid, into their interstices, and into the adjoining cellular membrane in the mediastinum. Of course there can be no quantity of liquid effusion into an adherent pericardium; but under these circumstances this very commonly takes place into one of the pleural sacs instead. The exacerbations to which those affected with old rheumatic disease of the heart are occasionally subject, often originate in inflammation of the pericardium, or the internal lining membrane. An adherent pericardium has no immunity from these attacks; in fact, from its being so constantly fretted and strained by the inordinate motions of the heart, it may be conceived to be particularly liable to them; and if they occasion death, the appearances found are such as I have described. You may perceive that these inflammations of an adherent pericardium cannot give the signs that are usually distinctive—the friction sound, or the dulness and deficient impulse of effusion. Hence, unless their presence is indicated by local pain, tenderness, or soreness, it can only be suspected, on the occurrence of more than usual irregularity and excitement in the action of the heart, unaccompanied by other sufficient cause.

Inflammation of the internal membrane of the heart — *endocarditis*, as Bouillaud has very well named it, is a frequent but not a constant concomitant of pericarditis. Nor is it wonderful that inflammation should readily spread from one surface of the heart to the other, when we recollect how near they approach at the auriculo-ventricular and arterial orifices, where they are separated only by a fibrous structure of more strength than thickness. Now this very fibrous structure is especially obnoxious to rheumatic inflammation, and it is chiefly by inflammation of this kind that both surfaces are simultaneously attacked. Pericarditis, from other causes, may exist without any inflammation of the lining membrane; and endocarditis, when it exists independently of

rheumatism, may continue long without affecting the pericardium; the inflammations in these cases being chiefly confined to the serous membranes, and neither affecting nor traversing the fibrous tissue between them.

The endocardium is placed in very peculiar circumstances with regard to the effects of inflammation upon it. It is a serous membrane, and the products of its inflammation might be expected to be like those of the pleura, or any other serous membrane, lymph and serum. But as we found that the continual motions of the heart give a peculiar form and character to such products in the pericardium, so these motions, and those of the blood, must be continually acting on those of the endocardium; in fact, they must be continually swept away; the serum entirely, and the lymph also, except in circumstances very favourable for its concretion and adhesion to the secreting surface. It is no wonder, then, that endocarditis should not have been readily recognised; since, in many instances, the traces by which inflammation is known to have existed could not remain to indicate its seat. So you may be prepared to expect that the anatomical characters of endocarditis are far less obvious than those of most other inflammations. We do, however, not unfrequently find in subjects dead of rheumatic and other inflammations of the heart, or of older organic disease, indubitable traces of inflammation of the lining membrane. Thus, especially at the left auriculo-ventricular ring, and in the membrane lining the auricle near it, as well covering the laminæ of the mitral valve as in the aortic valves, especially at their attachments and *corpora arantii* in their margins, we meet with striated or punctuated patches of vascular redness, sometimes accompanied by an inequality, roughness, or softness of the membrane; occasionally, with soft thickening or distinct films of soft lymph upon it. In some cases we find apparent abrasions or distinct ulcerations of the membrane; and to the edges of these, or to any roughness or irregularity which the valves or lining membrane may present, are attached little soft fibrinous bodies, generally of a raggid conical shape, and more or less tinged with blood, which have obtained the name of *vegetations*. These all are probably effects of the more acute form of inflammation. But the same parts often present changes which, in other structures, we are accustomed to refer to chronic inflammation, such as tough thickening, with partial puckering, induration, and even ossification, of the lining membrane, or of those folds of it which form the valves, and especially of the fibrous tissue that is a strengthening network in them. The tissues thus changed are liable to rupture, irregular distension, and other accidents which we shall have to notice under the head of Diseases of the Valves.

The general symptoms of endocarditis are still more obscure than those of inflammation of the pericardium. There may be fever with increased action of the heart, sometimes with irregularity, more or less of the feelings of palpitation or faintness, oppression or agitation, and pain, heat or soreness, referred to the sternum or epigastrium; but most of these may be absent, and associated as they often are with general rheumatic fever and pains, and occasionally disguised by sensorial disorder, or gastric and hepatic derangement, the secondary effects of the deranged action of the heart, the symptoms which should fix attention on this organ in particular, are very frequently overlooked, or referred to the convenient and satisfactory category of *general irritation*.

If we examine the pathological effects of inflammation of the lining of the heart on the tissues of the organ, we see that it must do more than produce general irritation. If it be equally diffused over every part of the interior of the heart, it might, in the first stage, cause a general increase of irritability and action; but we have seen that it commonly prevails most in particular parts, especially at the aortic and mitral orifices; and we must suppose that it will modify the vital properties of these parts more than of others; there will be a spasmodic abruptness in the action of the muscular fibres there, or a permanent contraction of those fibres at the arterial orifices which are endowed with tonicity*; besides more or less thickening from the effusion of serum and soft lymph into the looser parts, as between the layers of the valves. These changes of properties vary with the progress of the disease. If the inflammation have continued for some length of time, the over-excitement of the contractile fibres leads to their exhaustion, weakness, relaxation, and ultimately to a change in their structure; and the softening and effusions may lead to stretching, ulceration, rupture, or in time to harder thickening, contraction, and adhesion together of the looser and more membranous parts.

This brief sketch of the pathological effects of endocarditis, on which, had we

* Arteries gradually contract on the application of a mechanical or chemical irritant to them. It is most probable, therefore, that the fibres, in which this tonic contractility resides, would be more irritable and contract more under the first influence of inflammation. Our Committee have been making experiments on this point; the results are interesting, but not yet complete.

time, we might enlarge with advantage, may suffice to render intelligible to you its physical signs, which I think I may describe to you with some confidence, having narrowly examined them in a good many cases, and traced them in a few to the conditions disclosed after death. They are often combined with those of pericarditis, which may come on after or before them, and always very much mask them. Like in pericarditis, the pulsations are not always more frequent in the first instance, but the sounds are louder and the impulse is stronger. In a few examples this has been all, and the loudness and strength have in the course of a few days subsided. But more generally the first sound becomes prolonged and double, or attended with a slight roughness, and eventually lengthened out into a blowing or grating murmur. This announces a modification in the current of blood in some part of the heart, and if you attend to the character of the sound and the situation where it may be best heard, you may with tolerable precision determine in which part of the interior of the heart it is produced. If it is heard best an inch or two below the left breast, where the apex beats, and is heard but little or not at all above the upper half of the sternum and in the carotid arteries, it is produced by regurgitation through the mitral valve, the closure of which the irregular spasmodic action of the fleshy columns renders imperfect. I before drew your attention to the nicely-adjusted mechanism of this valve; how, for its closure, there must be an equal drawing of all its cords, to spread the membranous laminæ against which the blood presses; and you can now see how either the excessive or the defective drawing of some by the spasmodic, or by the weakened contraction of their fleshy columns, would throw these laminæ into wrinkles or loops, forming little chinks, through which the blood may be squeezed in a sonorous jet at each contraction of the ventricle.

If, again, the murmur is distinct along the upper half of the sternum and in the carotid arteries, where it commonly has a rougher more grating character, you may conclude that it is produced in the aortic orifice. It implies the presence of a partial obstruction to the current, formed by swelling or deposit of lymph on the valves, or by the tonic contraction of certain fibres, all results of the partial endocarditis. In these cases, the clear flap, which constitutes the second sound, is often also impaired, being dull or double; and is sometimes accompanied, or even replaced, by a short grating sound, which is heard best about the middle of the sternum, and may sometimes also be heard in the carotid arteries. These altered sounds arise from changes in the arterial valves, which, being thickened by deposit on them, or by infiltration between their laminæ, cannot flap clearly, and may not perfectly close, but leave a little chink between their edges, through which the blood regurgitates with a slight grating noise on the diastole of the ventricles.

There are few cases of acute rheumatism which do not, during their course, present some of these signs, which sometimes are transient, and cease with the fever, but more commonly continue for a long time after, and often become permanent. As far as my observation goes, I should state that those indicating the affection of the aortic orifice are less enduring than those of the mitral orifice; but, whilst they last, they are commonly attended with more constitutional disturbance, and give to the pulse a sharp jerking character. It probably requires a more considerable lesion in this orifice to produce sounds than in the mitral orifice. In the latter situation, the properties regulating the action of the valve are so readily deranged, both by inflammation and by its effects, that the blowing murmur which is heard there often becomes more or less constant, or is produced whenever the circulation is accelerated. The degree in which the regurgitation may prove injurious, will chiefly depend on its extent and on the state of the general circulation. If the heart's propulsive power be weak, or the regurgitation considerable, which may often be known by the lower or deeper tone of the murmur, there will generally be more or less dyspnœa, especially on lying down or on lying on the left side, sometimes with feeling of faintness or palpitation, and perhaps cough; palpitation always on exertion; sometimes an unequal or irregular pulse; and occasionally more or less pain in the left side. But if the action of the heart is pretty effective, and the regurgitation slight, which may be known by the more whiffing or whistling character of the murmur below the breast, there may be little or none of any of these symptoms. But almost in all cases the regurgitation will induce secondary effects, by slightly but constantly *backening* the venous circulation, and causing gradual congestion in many viscera, which may more or less derange their functions, according to their proneness to disorder. Hence occasional gastric and hepatic derangements, or "bilious attacks," from accumulating congestions in the portal system; these are very common: attacks of asthma and pituitous catarrh, from pulmonary congestion; and attacks of headache, drowsiness, or giddiness, from stagnation of blood in the sinuses of the brain.

Thus you have in endocarditis many of the signs and pathological effects of valvular disease, and for this obvious reason, that this inflammation constitutes or induces valvular disease in its several varieties, and either in an acute or in a chronic form is its principal cause. So also endocarditis is often excited during the course of valvular disease or other organic lesions of the heart, causing considerable aggravation of their symptoms, generally increasing them, and hastening their fatal termination. We shall have to notice these effects under the head of diseases of the valves.

I shall say but little on the subject of *carditis* proper, or inflammation of the substance of the heart; for I am not sure that I have seen it, at least distinct from the membranous affections. In cases of pericarditis and endocarditis with disease of the valves, I have not unfrequently found portions of the muscular tissue of a darker colour and more or less softened, which probably is the effect of inflammation; but I am by no means confident of this. In one case in which I saw this livid red softening it occupied a considerable part of the posterior wall of the left ventricle: there was disease of the mitral valve, and thickening and redness of the endocardium near it. The patient had long suffered from palpitation, which had become aggravated of late, and his death was caused by spontaneous gangrene of one of the legs. A few instances are on record by Corvisart, Latham, and others, of pus being found between the muscular fibres; and Bouillaud describes other conditions which he supposes to be the effect of inflammation, especially a grey and a yellow softening. It appears to me probable that inflammation may lead to these conditions in some instances; but Dr. Copland has seen them (and so have I) in various cachectic states of the body, where there had been no signs of inflammation, but where all the tissues were more or less discoloured, the blood being apparently in a diseased state. To judge that the tissue of the heart is especially diseased, we must see that it differs much in appearance from the other muscles of the same subject. You will find, on comparing the same muscles in different subjects, a remarkable variety of colour; and in some there is no freshness in any of the muscles, but all are pale, and verging on a pinkish drab or dingy brick-colour. Where this is not the case, a pallid yellowish appearance of the substance of the heart is not at all an uncommon accompaniment of other lesions of the organ, especially adhesions of the pericardium and accumulations of fat; but I should be inclined to refer this to an altered state of the nutrition of the organ, owing perhaps to partial obstructions in the coronary vessels, rather than to the immediate influence of inflammation.

I can say nothing from experience on the symptoms and signs of carditis: in the cases on record there have been described, pain in the region of the heart, with fever, dyspnœa, extreme anxiety, agitation, and perhaps delirium; rigors, palpitation violent at first, passing afterwards into fluttering, fainting, and irregular movements; and in a very few days death ensues. In simple and general carditis I presume the sounds would be altered in intensity rather than in character; being louder at first, and soon becoming weak: but it is probable that generally some parts would be more affected than others, or the apparatus of the valves would be also involved; in which case there might be produced murmurs of various kinds. It is scarcely possible that acute inflammation can continue in a considerable part of the substance of the heart without interrupting its function, and rapidly destroying life; but it is conceivable that a small portion might exist, and even form an abscess, or, in a more chronic form, lead to a change of structure. Whether the yellow fibrous transformation of the muscular substance that is occasionally met with be the result of a process of this kind, I have not had a sufficient number of facts to enable me to determine; but I have seen, in cases of long-continued disease of the heart, among other changes in which an inflammatory process could be recognized, a development of whitish fibres, and even laminæ, in the fleshy substance, which had much the appearance of the commencement of fibrous degeneration.

I shall return to this subject under the head of Hypertrophy.

LECTURES ON BLOOD-LETTING;

Delivered from time to time,
At the General Dispensary, Aldersgate Street.

By Henry Clutterbuck, M.D.

Formerly one of the Physicians to that Institution.

Lecture XII.

Of the Effects of Blood-letting in Diseases of the Nervous System.

Under the term *nervous system* are included, 1st, the encephalon, or general contents of the cranium; 2dly, the spinal cord; and 3dly, the different nerves proceeding from the one and the other; with the ganglions attached to and connected with them: and, as appendages to the brain, may be mentioned also the external

organs of sense, and the muscles of voluntary motion. But, in reality, there is no part of the body that is not, more or less, under the influence of the nervous system. In pointing out the effects of blood-letting on this class of organs, I shall, however, confine my remarks principally to the encephalon or brain; for of the other parts of this system (the spinal cord and nerves), in relation to this remedy, but little is known from any direct experience; though we may presume they are, equally with other parts, under the influence of this agent.

The pathology of the brain, like that of other organs, can only be properly based on its physiology. The brain, then, considered as a whole, has many peculiarities, both physical and vital, by which it is distinguished from other organs. These peculiarities require to be pointed out, as they modify materially the symptoms and consequences of disease, and, not less so, the operation and effect of remedies. I am aware that much that I shall have to observe on the present occasion has been the subject of former remark; yet I would fain hope its importance, practically considered, will justify the repetition, as enabling you the better to understand, and apply to use, the powerful agent that forms the particular object of our inquiry.

In the first place, then, you will observe that the brain altogether, including the cerebrum, cerebellum, and medulla oblongata, with their membranes and vessels, and the blood contained in these, are confined in an unyielding bony case, that has no direct communication with the atmosphere, and the cavity of which they completely fill. This last fact is proved by the correspondence that is observed between the convolutions or prominences on the surface of the brain, and the depressions or hollows that are seen on the inner surface of the cranium. It is proved, likewise, when the entire head is first frozen, and then sawn through in a vertical direction, when the relative position of every part is distinctly seen, while at the same time you will perceive that there is no vacuity; nor is there any elastic matter present. The next observation to be made, is, that the contents of the cranium, as enumerated above, are all incompressible substances; at least, by any force that can possibly be conceived to be applied to them during life—as much so, in fact, as water itself.

Now the necessary consequence of this state of things is, that the entire quantity of blood in the whole organ (under ordinary circumstances) can neither be increased nor diminished, though it may be very differently distributed at different times. The arteries, for example, may contain more blood at one time than at another, so as to occupy greater space than before; but if this be the case, the veins must contain proportionally less of blood. The same irregularity may take place in different parts of the brain, in respect of each other. There may be, for instance, a greater quantity of blood circulating in one part, owing to an increase of action in the vessels of that part; as when inflammation arises in a particular part of the brain, attended by more or less of swelling, which must necessarily impede the circulation in other parts of the organ: and, doubtless, in such cases the functions will be affected in a corresponding way.

Again, although the quantity of blood in the brain altogether remain the same, the force and rapidity with which it circulates may be greater or less; this depending, principally, upon the activity of the cerebral vessels themselves, and comparatively but little upon the heart, or general vascular system. This comparative independence of the cerebral upon the general circulation, it is of importance to bear in mind when we are considering the diseases of this organ.

The brain altogether, including the cerebrum, cerebellum, and medulla oblongata, may be considered a double organ, consisting of the two hemispheres, each divisible into a number of corresponding parts, each of which is marked by some peculiarity of figure and position; and doubtless, also, by peculiarity of internal arrangement or structure; by which each is fitted to perform a distinct office, and for which none other, probably, is capable of acting as a substitute. This peculiarity of organization and of office, in the individual parts of the brain, is the true and only foundation of phrenological science. Whether this science is justly entitled to be carried to the extent its advocates suppose, is still, and probably will long remain, a matter of dispute. Without a general admission, however, on the subject—as far, at least, as the more obvious divisions of the brain goes—it is impossible to comprehend a variety of phenomena that present themselves, in regard both to the physiology and pathology of the brain. It is undisputed that a certain part of the brain is devoted to the sense of vision, another to hearing, a third to smelling; and so on; and we are therefore warranted by analogy in concluding the same with respect to the other portions of the brain, although we are not able, as yet, to afford the same satisfactory proof of the fact.

The circulation of blood in the brain

has likewise its peculiarities, as compared with other parts, and which will be found to have a most important influence on its diseases. The arteries going to the brain enter the cranium through narrow bony channels, a circumstance which serves to check the force of circulation in the organ, and guard it against any increased impetus from without. The same end is further attained by the anastomosis of the arteries within the skull, before they enter the substance of the brain. In this way the brain is in a great measure withdrawn from the influence of the general circulation, so that no violence of action on the part of the heart can have much effect on the circulation of the brain; this depending almost solely on the action of its own vessels, the heart doing little more than supplying it with the necessary quantity of blood to enable it to carry on its functions. Accordingly, we often find the brain performing its functions quietly, and in the most orderly way, where the general circulation is in a state of great and violent disorder; as in many cases of acute rheumatism, as well as other inflammations; while, at other times, the different functions of the brain are performed with unusual energy, accompanied with evident signs of high arterial action going on within the skull, where the general circulation is at the time in the feeblest state.

The causes capable of exciting the arterial action of the brain are both numerous and various; and they probably influence the organ unequally in its different parts. Among others may be mentioned impressions made on the different organs of sense: various stimulants that act on the brain through the medium of the stomach, and certain emotions of mind, have all the effect of increasing the circulation in the brain, as the result of increased arterial action there.

But the most striking example of a stimulus acting on the vascular system of the brain, so as to increase in all degrees the cerebral circulation, with a corresponding influence on the functions, is that afforded by alcohol, which appears to exert a specific operation, as it were, upon the brain and its functions, producing at first a state of simple excitement of the organ, and at length (if carried far) a total suspension of its functions—an apoplectic state, as it might be justly termed. The first effect observed from the use of intoxicating drinks is that of increased arterial action in and about the head; as shewn by the greater pulsation of the carotids—the bloated and suffused state of the cheeks and eyes—as well as by the increased heat of the whole head. This increase of arterial action occasions an increased circulation in the internal as well as external parts of the head—in the brain as well as in the neck, face, and scalp. The effect produced upon the cerebral functions during this first stage of increased arterial action is that of excitement simply; the sensorial functions, in general, are performed with greater energy, and with little or no disorder. This is the first stage of intoxication, if, indeed, it deserves the name. A second succeeds, and which is easily explained on the same grounds. As the vascular excitement goes on, the arteries not only continue to act with violence, but also become enlarged in their dimensions; probably because the *vasa vasorum* are equally excited with the arterial trunks to which they are distributed. Then it is that the distended arteries begin to press upon the cerebral veins; and this they may do, although remotely situated from each other: the intermediate substance of the brain being incompressible, will readily transmit the pressure to the most distant part of the organ. The circulation then begins to be impeded, and the function disturbed, a state of wild delirium taking place. This may be considered the second stage of intoxication.

But there is a third stage also, where, from the increasing arterial fulness, the circulation of the organ becomes more and more interrupted, so as at last to put a stop to the exercise of the sensorial functions altogether—a state of perfect apoplexy, differing from other apoplexies only in regard to its exciting cause. The recovery from this last stage of intoxication is easily understood. The increased arterial action subsides after a time, and the arteries resume their former dimensions. The pressure is thereby taken off the veins, and the circulation is restored. There is no necessity to resort here, as is often done, to the nervous system, in order to explain the phenomena, for these are readily understood by reference to the vascular system alone.

The brain performs a variety of functions, some of which are primary or special, while others are less direct. It is, for example, exclusively the source of sensation—of the voluntary power—and of thought. Hence alterations in any or all of those functions imply a corresponding change in the condition of the part of the brain in which those functions are, as it were, lodged. Thus altered vision (the eye or external organ being perfect) implies a change in the optic thalamus, or the optic nerve leading to it; and so of the rest. In like manner, an alteration in the mental faculty implies a corresponding

change in that part of the cerebrum, whatever this be, through the agency of which the powers of mind are manifested; so that we chiefly judge of the condition of the brain, in regard to health and disease, by observing the state of the functions which it exercises.

But we have likewise other modes of judging of the soundness or unsoundness of the brain; namely, by the feelings of the part, and by the state of other functions that are known to be dependent on, or intimately connected with it. Such, more particularly, is the stomach, and, in short, all others, though in different degrees. This universal influence of the brain over other organs you will readily see must give rise to a far greater diversity of symptoms than in the case of other organs; and as these symptoms often manifest themselves remotely from the brain itself, although originating there, the affection of the brain is liable to be, as it often is, overlooked.

With respect to the feelings of the part, as serving to point out the existence of disease in the brain, it is important to know that this organ, though the source of feeling to other parts, is yet itself among the most insensible. The most violent and fatal diseases of the brain are often unattended by pain in the head; as is observed in many apoplexics, and in fevers of the worst description, though pain may be felt in a distant part of the body, as cramps in the legs, &c. So, again, disorganization creeps on in the brain, till it at length terminates suddenly and fatally in apoplexy or palsy, without any preceding pain in the head. But although the affections of the brain are characterized by an almost infinite diversity of symptoms, there is no organ more simple in regard to the intrinsic nature of its diseases than the brain itself. The only disease of this organ, in fact, of which we have any certain or distinct knowledge, is inflammation, with its consequences. And it is here, more especially, that the observation I formerly made applies with the greatest force; namely, that almost all diseases either consist in actual inflammation, or are consequenecs of it, more or less remote. But you will have a very limited and imperfect notion of inflammation of the brain, if you confine the term to phrenitis, as usually defined. This is the least frequent of its forms, and applies only to cases where the mental function is particularly and violently disturbed. But there are innumerable instances of inflammation going on in the brain, and proceeding to a fatal termination, without any disturbance of the mental function from first to last. Without a knowledge of this fact, the disease will be often overlooked, and danger incurred. When inflammation arises in the brain, it may affect the organ generally, so as to disturb all the functions at once, as well as to produce general disorder throughout the system, as is the case in what is termed idiopathic fever. Or the inflammation may be confined to any one or more of the individual structures that altogether constitute the brain as a whole; and the symptoms will vary, according to the particular seat and extent of the inflammation present. This frequent and almost constant connexion of the diseases of the brain with inflammation, as stated above, will naturally lead you to expect that blood-letting must be very frequently required for their relief; and such, undoubtedly, is the case. Yet there is no class of diseases that require greater caution and discrimination in the application of this remedy, than those we are now considering. It is not enough to say that fever, or any other disease, consists essentially in inflammation of the brain, in order to justify the application of blood-letting; for without due regard to various qualifying circumstances, the remedy may become worse than useless. I shall proceed now to speak of its application to different forms of cerebral disease, commencing with those of the most acute description.

Of the Use of Blood-letting in Phrenitis.

The term phrenitis has been applied to inflammation of the brain, when accompanied by active and furious delirium; and it has been supposed that this is a case peculiarly calling for the use of blood-letting. Such a state of mind, however, is not a sufficient justification of the practice: on the contrary, the most furious delirium may take place, where the other circumstances are altogether adverse to the practice. Delirium of this kind, for example, occurs suddenly and unexpectedly in the advanced stage of many fevers, where the corporeal powers are utterly prostrate, and death approaching; when, in fact, blood-letting could but hasten the fatal event. In short, I may tell you once for all, that our indications of cure are not to be drawn, in this or any other case, from the state of mind merely, but from corporeal signs. The most violent delirium is not of itself an indication for bleeding; nor, on the other hand, is the absence of delirium a proof that blood-letting may not be required. You are to look to the general circumstances of the case, the age and habit of the patient, and more especially the stage of the disease, as affording the safest guides. This is

still more necessary in regard to the extent to which the remedy is carried, supposing its use to be proper. The bleeding a patient largely, merely because the delirium is violent, is a practice as unreasonable as it is dangerous in its result.

These strictures apply with equal force to the employment of other means, where the violence of the delirium seems to be taken as the only measure in the administration of remedies. Thus, because the patient raves, and does not sleep, opium is often exhibited to a dangerous extent; as if the procurement of sleep were an effectual remedy for the disease; than which, nothing is more unfounded. The condition of the brain may be changed, indeed, by this and other narcotics, and stupor (rather than sleep) induced by them. But the real question to be determined is, have such means a power, or even a tendency, to relieve inflammation in such an organ? Generally speaking, I should say not, but rather the reverse.

In connexion with this subject, I may here allude to what is called (absurdly enough) *delirium tremens*. This state, manifestly a brain-affection, is induced by the long-continued use of intoxicating drinks. A remarkable change has taken place in the minds of practitioners regarding the disease, both in respect to its nature and treatment. Formerly, it was classed with inflammations of the brain, and considered as a mere variety of phrenitis; and it was also treated as phrenitis then usually was treated, that is, by blood-letting and other anti-inflammatory means. Within a few years, however, very different views have been entertained on the subject. It was the late Dr. Sutton, of Greenwich, I believe, who gave the disease the name of *delirium tremens*, and who first suggested a different and even opposite mode of cure to that generally in use before. Blood-letting was condemned, as not only unnecessary, but as positively injurious; and he substituted for it the use of opium in large doses, and even a regulated employment of the very cause that had first induced the disease. And such appears to be the prevailing opinion on the subject at the present time. The notion seems to be, that the affection of the brain, in what is called *delirium tremens*, instead of being inflammatory in its nature, is a state of debility, the result of previous over excitement (the indirect debility of the Brunonian school); requiring, therefore, a regulated administration of stimuli, in order to enable the organ to resume its healthy state of action. The hypothesis involved in these notions is hardly worth your attention; but the practical inference deduced from it is of some importance.

No distinct meaning seems to be attached to the term debility, as applied to the brain in this case. Both the phenomena of the disease, and the nature of the exciting cause, go to shew a state of increased arterial action there. That such a repeated excitement of the organ should at length induce a diseased condition of the part, it is quite natural to expect; while the signs of increased circulation about the head, which is the invariable result of the action of this stimulant, prepares one to expect an inflamed state of the brain as the result—an expectation that is verified by the febrile state of the system that accompanies the disorder, as well as by the occurrence of the delirium itself.

The propriety of blood-letting in such a case is quite another point of consideration, which experience only should decide. I have seen and treated numerous cases of the sort (some of them, indeed, members of our profession); and I am prepared to state, with confidence, that blood-letting will be found of the greatest service on many occasions. There is, therefore, no foundation for the opinion that this remedy is adverse in its nature to the disease. But while I say this, I wish you to understand that such a remedy is not generally required, and only where the febrile symptoms are such as to lead to the belief that the inflammation is of an active kind, and where the general strength and other circumstances warrant the adoption of it. In a large proportion of cases it is sufficient to withdraw the patient from the habitual indulgence he had been accustomed to. A gradual abstraction of the exciting cause would probably be advantageous, and preferable to an immediate and total withdrawal of a long-accustomed stimulus. Much advantage, also, may be expected from the use of slightly tonic remedies on these occasions.

But in speaking thus favourably of blood-letting in certain cases of *delirium tremens*, I must add, that in no case is the remedy to be used without great caution, and when used, to a very limited extent only. With this precaution, and under the proper circumstances, you may resort to it without apprehension.

As to opium in these cases, I may remark, that the motive for employing it is little more than imaginary; while I know that it is in many cases unnecessary, the disease terminating well without it. That it is altogether free from inconvenience, or even danger, when employed to the extent often recommended, my observation leads me to doubt. Applied to a moderate degree, it may possibly do good at times.

ON UTERINE HÆMORRHAGE,

WITH PARTICULAR REFERENCE TO A CASE OF PARTIAL PRESENTATION OF THE PLACENTA.

By JOHN RENTON, Esq.

[Concluded from p. 833.]

Of the particular cases in which the plug may be advantageously employed it does not become me to speak, having had no experience in its use. I have a general dislike to it in the latter months of pregnancy, on the ground that when hæmorrhage, either from accidental or unavoidable causes, occurs, procrastination becomes, in every sense of the word, "the thief of time," and "deferred hope" in no circumstances is more apt to make the heart sick, not only of the patient, but of the practitioner. Dr. Burns says *, "As long as the os uteri is firm and unyielding—as long as there is no tendency to open, no attempt to establish contraction, it is perfectly safe to trust to the plug, rest, and cold. But I must particularly state to the reader, that the os uteri may dilate without regular pains, and in almost every instance it does, whether there be or be not pains, become dilatable. Did I not know the danger of establishing positive rules, I would say that as long as the os uteri is firm, and has no disposition to open, the patient can be in little risk, if we understand the use of the plug; we may even stuff the os uteri itself, which will excite contraction." "But if the patient be neglected, then, I grant, that long before a tendency to labour or contraction be induced she may perish." "It is evident that when the uterus has a disposition to contract, and the os uteri to open, delivery must be much safer and easier than when it is still inert, and the os uteri hard." He adds, "In some instances we shall find that by the plug alone we may secure the patient. In these circumstances, who would propose to turn the child and deliver it? Who would not prefer the operation of nature to that of the accoucheur? To determine in any individual case whether this shall take place, or whether delivery must be resorted to, will require deliberation on the part of the practitioner. If he have used the plug early and effectually, and the pains have become brisk, he has good reason to expect natural expulsion, and the labour must be conducted on the general principles of midwifery. But if the uterus have been enfeebled by loss of blood—if the pains be indefinite—if they have done little more than just open the os uteri, and have no disposition to increase—then he is not justified in expecting that expulsion shall be naturally and safely accomplished, and he ought to deliver. When he dilates the os uteri, he excites the uterine action, and feels the membranes become tense. But he must not trust to this; he must finish what he has begun."

Mr. Gibson, when he was first called, in order to restrain the hæmorrhage until such changes had taken place as might render delivery easy, stuffed the vagina and os uteri. It might be said, that in consequence of the continuance of the draining the operation had not been efficiently performed, and that therefore no valid objection can be urged on this ground against the utility of the plug. But though the external hæmorrhage had been arrested by the plug, there was reason to apprehend, from the dark appearance of the clots which were discharged, that uterine distension from internal accumulation of blood might have taken place to an alarming extent. According to Dr. Merriman, the advantages of the plug are limited, and can only answer when the uterus is not very distensible *. "It can never be relied upon after the uterus has acquired much bulk, or is capable of being largely dilated; for, however completely the vagina may be closed, as no pressure can be made upon the open vessels within the uterus, these might continue to pour out blood into the uterine cavity, sufficient to destroy the patient's life, though not a drop of discharge was visible without the vagina."

"The arteries of the uterus cannot be closed except by a due contraction of that viscus. Whenever the uterus is in a distended state, the arteries will continue pouring out blood, and the greater the accumulation of coagula within it, so much the greater will be the amount of hæmorrhage. The presence of the plug, then, so far from benefiting the patient, by preventing the escape of the

* Vide Burns' Principles, p. 326.

* Vide Merriman on Difficult Parturition, 4th edition, p. 133.

coagula, adds to her peril. The plug, therefore, as it seems to me, is inapplicable in all cases where the bulk of the uterus exceeds that of a pregnancy of three or four months, or when the parietes are so easy of distension as to yield readily to the accumulation within it. Even in the case of unavoidable hæmorrhage, I have known a large quantity of coagula collected in the cavity of the uterus—much greater than could have been suspected, and adding greatly to the peril of the case*."

Although it may be conceded that the proper practice lay in rupturing the membranes for the purpose of effecting a diminution of the volume of the uterus, by the removal of the liquor amnii, and that in the circumstances of the case it was found necessary to excite and establish and keep up uterine action by manual assistance, the question remains still to be answered how far assistance in that way can be persevered in, without injury to the patient, and with advantage to the labour. My general objection to the use of the plug, I have said, arises from this, that by trusting to it we are apt to substitute an uncertain for a more decided line of practice, and by delay may lose an opportunity, which may never return, of adopting a safer mode of management by means of turning when possible, or by stimulating uterine action by the dilatation of the os uteri. The patient should seldom or never be left until delivery be accomplished, when hæmorrhage occurs, whether the placenta be implanted over the os, or attached near the cervix uteri. The patient's life must always be in danger from returns of the flooding, until the uterus be completely emptied of its contents; and all I maintain is, that by manually assisting the dilatation of the os uteri we can always save hours of labour, and very often fatal effusions of blood. I have long practised this plan, and my experience confirms all that Dr. Hamilton has written on its advantages and perfect safety. I have never known inflammation or even simple irritation of the os or cervix uteri to occur in any case where I have operated; while I believe that these parts are not easily injured* by the finger, although long-continued and active assistance be afforded in their dilatation; at the same time, all sudden and violent force used for that purpose must be strongly deprecated. I am convinced that the congested and apparently sphacelated condition in which the os and cervix uteri have been found after death, may have occasioned much misapprehension on this subject. These appearances, however, are frequently *post-mortem* changes, and I have observed them as often after easy as after severe labours.

The dangerous consequences of procrastination—of trusting to accessory means of treatment, when active remedial measures are in our power—are best illustrated by cases. The narration of cases 139, 140, 141, 144, 147, 149, 151, &c. &c., in Dr. Ramsbotham's Practical Observations, part ii., p. 832, puts in a stronger light the fatal effects of constant slight "drainings" without interference, than any language I can employ.

In all these cases, the dangerous symptom is the continued loss of blood. I cannot, however, agree with Dr. Ramsbotham, when he says, "When this alarming symptom subsides, there can be no necessity for interference." Dr. Hamilton's remarks† on this point are too valuable to be omitted. "His conviction is, that the means employed to complete the delivery rouse the living powers, and he can solemnly affirm, that the chief error he has witnessed in the treatment of these cases has been procrastination. That on some occasions of uterine hæmorrhagy during the latter months of pregnancy, no human means can save life, is a melancholy truth; but that, in a very great majority of fatal cases, the indecision of the practitioner is the chief cause of the mis-

* Dr. Hamilton, in his Practical Observations, part ii., p. 234, records two cases in which retention and accumulation of blood within the uterus took place before delivery, consequent to a partial separation of the placenta. There was no discharge from the uterus, and no symptom of labour in one of the cases which proved fatal.

* I am supported in this opinion—1st, by the cases in which the os and cervix uteri are subjected to long-continued compression, without injury from the child's head, in contracted pelves; and 2ndly, from the successful result of the operations performed in the divisions of those parts. In the 3rd vol. of the Edin. Med. Essays, a case is recorded where Dr. Simpson "cut through a callosity of an os uteri half an inch thick." In the Brit. and For. Med. Review for July 1838, is related a case of Dr. Burdach's, of cartilaginous os uteri, requiring an incision of one and a half inch, with perfect recovery; and also one of Dr. Gruhn's, of prolapsus uteri, in which an incision three inches long was made in the os uteri, and the woman recovered.

† Vide Practical Observations, part ii. pp. 268 and 269.

chief, he verily believes; and when this subject is seriously considered, a conscientious practitioner would naturally ask himself, what harm can ensue from active interference, before decided symptoms of immediate danger manifest themselves?

"Hitherto, the bugbear which seems to have haunted the minds of practitioners in the treatment of those cases, is the supposed difficulty or danger of dilating the os uteri. But if it be practicable, when the membranes are entire, to hook down a lower extremity of the infant, without carrying the hand through the os uteri, which the author most positively affirms he has done innumerable times—and if, when a lower extremity is drawn through the uterus, the cervix and os uteri dilate readily and safely (being duly supported), which, according to his experience, invariably happens, all objections founded upon the difficulty or danger of turning must be held to be futile."

To leave the completion of the labour to the full effect of the natural agents, embraces a rule far too general in my opinion to be a justifiable one in practice. It cannot be acted upon in country practice, where the practitioner is often called to patients at great distances from home, and whom it is impossible regularly to attend. The progress of such cases can neither be narrowly watched, nor, in the event of any return of hæmorrhage, can assistance be readily procured. The styptic treatment recommended by Dr. Dewees cannot safely be followed in such circumstances. When summoned in such a case, though the discharge may have abated before my arrival, provided it has not sunk the living powers, I always propose immediate delivery—that is, I never leave the patient to the mercy of palliative means, nor make any endeavour to enable her to go on to the full period of gestation, but consider the period has arrived when the connexion between the mother and the fœtus, which has been partially, should be wholly dissolved. Many may suppose I am advocating a rash incautious practice, both in reference to the means employed and their period of application. With regard to the first, I have always found the finger a safe and efficient dilator of the os and cervix uteri; and with regard to the latter, I consider that when a patient's life has once been in jeopardy from hæmorrhage, it is the practitioner's blame if she be in danger from its return, as the means which are always in his power to prevent it should at once be put in practice. I have long adopted such a practice, and have found it to succeed; and I recommend it, because it has never failed.

I urge this advice with the more earnest confidence, that it does not originate in matter of opinion, but has been founded on and confirmed by long experience. Dr. Osborn* lays it down as an invariable rule in practice, as affording the only chance of saving the patient's life, "that recourse be had to artificial delivery immediately upon the first attack (of hæmorrhage), and long before danger is apparently incurred; for if we wait till symptoms of danger arrive, the event will prove that, in general, we shall have already waited too long." The most besetting sin, and certainly one of the worst evils in operative midwifery, is indecision; for, next to forming an accurate opinion, is acting decidedly and promptly upon it.

My experience extends only to four cases of hæmorrhage from low attachment of the placenta, which, in two of them, was implanted over the os uteri. They all terminated favourably. Two of the children were born alive. Three of the cases occurred in women who had had large families, and one in a first labour. I experienced little or no difficulty in dilating the os uteri in the former cases, the operation of turning and delivering being performed within ten minutes. In the latter case the os and cervix uteri were not nearly so dilatable, and a much longer period of time was required. In one of the cases I was benefited by the assistance of Dr. Beilby, Edinburgh. The symptoms in the other cases were, in my opinion, so urgent, as to warrant me to proceed immediately to delivery by turning, without waiting for a consultation. I have no doubt, had delivery been postponed until that had been procured, that the result in the majority of the cases would have been fatal. I may be permitted, therefore, to repeat Dr. Osborn's precept, that "we cannot in such cases be too quick in the determination of the measures to be pursued, or too prompt in the execution of them." Caution and

* Vide Dr. Osborn's Essays on the Practice of Midwifery, p. 49. 1795.

deliberation there are worth nothing; celerity and expedition are every thing.

In cases of twins, and especially when there were two placentas, I have saved in more than one case the woman's life, by having speedy recourse to turning when hæmorrhage occurred after the delivery of the first child, and where external compression by the hand had failed to excite uterine action. As far as my experience goes, the irritation of the hand introduced into the vagina, very generally, and within the cavity of the uterus almost always, renews the dormant and suspended action of that organ in such cases.

In several cases of accidental hæmorrhage from partial separation of the placenta during the last three months of gestation, I have had occasion to accomplish delivery by turning, and I have never known any injury done to the os or cervix uteri when the hand has been used as a dilator, although considerable time and force have been used for that purpose. But here the precept must be reversed in the execution of the means that I recommended in their adoption; the parts must be slowly and by intervals dilated, and all attempts at turning and extraction must be gradually and deliberately made. Here caution and deliberation are every thing; celerity and rashness the reverse.

"If the os uteri is so much contracted," old Smellie remarks*, "that the finger cannot be introduced, some authors have recommended a dilator, by which it may be gradually opened, so as to introduce a finger or two. Doubtless some cases may happen in which this may be necessary, though in all those to which I have been called, when there was a necessity for forcing delivery, the mouth of the womb was open enough to receive the tip of my finger, so that by gradual efforts I could effect a sufficient dilatation; and it is certainly a safer method to dilate with the fingers and hand than with an instrument."

The instrument Smellie refers to, I presume, is the dilator invented by Roonhuysen, an engraving of which, and a description of it by Rathlaw, is given by Dr. Spence*. This instrument was long known under the name of "the Roonhuysian secret;" and it had been well for the inventor's character that he had kept the secret inviolable to all but himself. I have only mentioned it, as affording, among many other instances, an example of one of the most absurd and impracticable devices to which the older obstetricians had recourse, when their ingenuity was excited by a "zeal not according to knowledge."

I need scarcely add, in cases of hæmorrhage during the last three months of gestation, it is of the greatest practical importance to determine whether the discharge proceeds from the large blood-vessels supplying the placenta, or the small vessels supplying the decidua, or those which may be slightly connected with the membranes.

If this communication had not already exceeded the ordinary limits, I could give, in further proof of the danger of non-interference in attacks of flooding, the particulars of the histories of six cases terminating fatally, which have occurred within the last twenty years in this neighbourhood. I did not see any of these cases during life, and my assistants were only called in to some of them at the last hour. They appeared to me to be all mismanaged, in consequence of gestation being either allowed to go on to a late period, when the natural powers sunk, or of the want of timely assistance, *partu durante*. One woman died undelivered; three of them survived delivery only half an hour; and the other two remained in a state of scarcely perceptible animation for two days. The occasional occurrence of such melancholy instances has indelibly impressed on my mind the imperative necessity of active treatment.

In town practice the powers of the patient's constitution can be well tested by delay, and the practitioner may learn the extent to which she can bear the loss of blood, without the chance of much loss of reputation, for he can divide the responsibility of the case with the medical friends he consults, and may make up for the want of activity of treatment by overwatchful anxiety and regular calls. In country practice,

* Vide a Treatise on the Theory and Practice of Midwifery, by W. Smellie, M.D.; page 332. 752.

* Vide System of Midwifery, by David Spence, M.D.; pages 575 and 578. 1784

however, the practitioner would hazard too much of his own character in putting his patient's strength to so severe trials. There, all his acts are more rigorously examined, and judgment is more summarily pronounced. By a single failure his professional confidence might be destroyed. If a woman were to die undelivered, or in his absence, from hæmorrhage, a life-rent of public unforgiveness would be entailed upon his name; and he is therefore compelled, by his peculiar position, to trust more to the devices of art, and to leave less to the resources of nature.

"Suis hæc viribus, sola, sine medico, plerorumque tuetur sanitatem, morbis medetur. *Hac deficiente, aut repugnante, irrita artis molimina.*" Dr. Hamilton employs this illustration of the impropriety of leaving hæmorrhage to be arrested by the coagulation of blood in the extremities of the ruptured uterine vessels, and of promoting a tendency to such coagulation, by the direct and local application, as well as by the internal use, of styptics. He asks* "whether, if any of the audience should wound his finger in mending a pen, he would direct a vein to be opened, and then swallow a dose of sulphuric acid, or of the acetate of lead; or whether he would bind up his wounded finger?"

Although an illustration be no argument, I would draw another illustration from an accident not uncommon among carpenters, in the division of the anterior tibial artery by their hatchet. Before the patient is visited he has fallen into a state of syncope, from the great loss of blood—at all events the hæmorrhage has stopped, and the attendants have plugged the wound with cotton, and covered it up with dressings wet with some styptic wash. The medical attendant, although he may not be credulous enough to believe that the wounded artery will not discharge more blood, yet acts as if he really entertained that opinion. He leaves the patient to contraction and coagula. Soon afterwards there is another return of hæmorrhage, which also stops before he answers the second summons; and still he does nothing to secure the injured blood-vessel. In this way successive attacks of discharges of blood come on and go off; and although no doubt can exist as to the source of the hæmorrhage, whether he regards the instrument which inflicted the wound, the colour of the blood, or the manner in which that fluid flows, still nothing is done until the patient's strength is exhausted, either by the extent and number of the discharges, or by the unmanageable nature of the last effusion, which, in spite of bandage and compress, will not stop, but threatens to destroy the unhappy sufferer. Then that is done at last which should have been done at first; ligatures are put round the extremities of the bleeding vessel, and there are no more returns of blood.

So is it with a dangerous attack of uterine hæmorrhage. The divided artery is the partially detached placenta; the cotton stuffed into the wound the obstetrical plug; and the opodeldoc the styptic injection. The passive treatment in both cases is attended with parallel results. There is the same blind reliance on the uncertain operations of nature—a similar culpable non-interference, in spite of the frequent forewarnings of danger, until the patient be reduced to a state nearly, if not entirely, irremediable.

* Vide Practical Illustrations, Part II. p. 248.

ON SECALE CORNUTUM.

By John Armstrong, Esq.

[Concluded from p. 734.]

I shall now endeavour to point out three cases in which the secale cornutum may be used with propriety and advantage, and also the best mode of administering it.

In cases of abortion, when there is much flooding, and when, from the delicacy of the patient, it is desirable to avoid hæmorrhage, if the uterus be at all open, it may be used with decided benefit. But I should prefer, if the uterus be not opened, having recourse to the ordinary remedies in the first instance, as I think when the ergot has been administered abortion will almost certainly follow. When, however, the os uteri is dilated (I speak of abortion during the first four months) to the size of the disc of a sixpence, and the ovum can be felt, the ergot may be used with the most certain success. The mode in which it acts in suppressing hæmorrhage I apprehend to be as follows: the first effect of the secale is to throw the abdominal muscles into a state of

spasmodic action; these contracting on the uterus excite its peculiar action: the large abdominal vessels will, of course, be compressed, and the current of blood necessarily diminished, whilst the uterine vessels will be compressed by the action of that organ, whereby scarcely any blood can flow into it.

In ordinary labours, where the head has descended into the pelvis, presenting naturally, the external parts lax, the os uteri almost entirely dilated, soft, and yielding, and where the woman has previously borne children—with such a state of affairs, if the pains are tardy, or inefficient, a dose of the ergot will infallibly and safely expedite delivery. It is in such cases that its effects are most clear and satisfactory; not only to the attendants, but to the patient and friends. I have been repeatedly solicited by persons to whom I had administered it under such circumstances on former occasions, for some more of the medicine, expressing at the same time, "How soon and easily they got out of their trouble after taking it before."

The next class of cases to which I would beg to call attention, and in which, when judiciously managed, it is of great utility, is breech presentations. Every practitioner knows how repeatedly children are asphyxiated in consequence of the length of time between the expulsion of the breech and the head, even when every thing has been done that can be done to release the funis from compression; and frequently every means to resuscitate the child will prove unavailing. Since I have adopted the plan which I am about to mention, I have not had a single instance of breech presentation in which the child has been still born: in fact, where formerly I almost invariably informed the friends that in all probability the child would be dead, I now feel as certain of its safety as under the ordinary circumstances of natural presentation. The plan is as follows:—When the breech begins to press on the perineum, the os uteri being fully dilated, the external parts beginning to relax, the bladder and rectum empty, I administer a dose of the ergot; and if given in proper time, *cæteris paribus*, there will be little doubt but the shoulders and head will speedily follow the expulsion of the lower extremities. I have now tried this method in eight or ten instances, and invariably with the most satisfactory results: all the children have been alive, most of them vigorous; and I think in only two instances was it necessary to use the warm bath. I have no hesitation in recommending the plan as safe and efficient under such circumstances.

In twin cases, after the expulsion of the first child, when the uterine efforts are tardy and inefficient, and when the presentation was fair, vertex or breech, I have used it with good results. I had some months ago a case of triplets; the woman was very delicate, and, after the expulsion of the first child, the powers of life seemed so feeble that I thought my patient perfectly incompetent to the task. There was a choice of either introducing the hand or administering the secale. I chose the latter, and with the happiest results. Two of the children are now alive, and as hearty and vigorous as most children of the same age. The parent's recovery was most satisfactory. In hæmorrhage before delivery, I have occasionally found it especially beneficial; and these are some of the most alarming cases which present themselves, especially when they arise from attachment of the placenta to the os uteri. Many cases of hæmorrhage occur before delivery from other causes, which are occasionally profuse and alarming, rarely, however, endangering the life of the mother or child. In such cases, quietude, the application of cold, plugging the vagina, digitalis, and in some instances bloodletting and opiates, are generally sufficient. I have found rupturing the membranes occasionally a beneficial expedient. But when the hæmorrhage still continues, the secale may be exhibited sometimes with the most happy effects: of course cases will occur where every prudent practitioner would see the absolute necessity of immediately turning and extracting the child, &c. I will narrate, in illustration, a case of a most unusual character which occurred to me about three years ago.

Mrs. R., ætat. 30, has had four children; is a delicate woman, of a pale leuco-phlegmatic temperament; she is now entering the eighth month of gestation. Early in the morning she had a considerable discharge of blood from the uterus, which caused faintness; but as she soon recovered, and the discharge had nearly ceased, I was not sent for

until it had returned. I found her pale, with a pulse feeble and quick, and on examination the os uteri was dilated to the disc of a half-crown, with the placenta attached round it. From the urgency of the case, I informed the patient and friends that it would be necessary to deliver immediately, as another occurrence of the same nature would probably prove fatal. I could not get them to consent, the patient declaring her determination to die rather than submit. I endeavoured to convince them of their folly, stating the case and safety with which it would be effected. All I could say was ineffectual. I then expressed my intention to relinquish the case, advising them to send for another practitioner: to this also they would not consent. Under these painful circumstances I resolved and adopted the following plan:—A full dose of the ergot was administered, a bandage placed firmly round the abdomen, and the vagina plugged. In about 15 or 20 minutes, finding the pains not coming on cleverly, a second dose of the ergot was given: the pains soon became more efficient; there were a few coagula, and some slight draining; and in about an hour the placenta was expelled. In a few minutes a dead fœtus followed. The after treatment was plain and simple, and the woman recovered slowly and steadily.

I have endeavoured to point out some of the principal circumstances in which this medicine may be improperly used, and also to show what are the cases in which it is indicated and may be used with safety. It would extend this paper too much to enter into *all* the circumstances connected with its exhibition: I shall probably on some future occasion endeavour to complete the subject. It remains now to state a few particulars of the mode of exhibition which has been most satisfactory in my hands. The powder is the form which I now invariably use; I have tried tinctures, extracts, essences, decoctions, &c. &c. and every form of it of which I have heard. I have no hesitation in saying that they are all decidedly inferior to the recent powder.

A scruple of this, if good, is almost invariably sufficient: in very rare instances have I found it necessary to repeat it. Coffee, good and strong, is the best vehicle, and generally so completely covers the peculiar flavour of the ergot, that many persons are not aware of having taken any thing in the coffee: milk and sugar may be added. In this way it is scarcely ever rejected. Previous to its administration I generally request the patient to get up for a few minutes, and as soon as the pains begin to increase, to return to their bed; at other times, when it is not advisable that they should get up, they are requested to lie on their back or right side: by this plan I find the secale produces its effects more speedily.

P.S.—Within the last fortnight I have had a most remarkable case of abortion; a precisely similar case to one noticed in the last number of the British and Foreign Medical Review, page 236. My patient was in the fourth month also. She had received a blow a few days previously, and from that period she felt ill. About six days from the period of the injury she was attacked with uterine pains, with but little hæmorrhage. Finding the case proceeding safely, I desired the nurse to send for me should any thing unusual occur. About eight hours after I was sent for, and found the fœtus had been expelled; but as it exhibited such strong signs of life, the nurse did not know what to do. When I arrived, the heart was pulsating strongly; it drew its legs up twice or thrice, and moved its arms, and once seemed to make an effort at inspiration: the heart's action continued for some time after the other movements had ceased; and finally ceased about fifteen minutes after I arrived. I did not measure it, but I should suppose it was about five or six inches in length; the eyes were closed; the cranial bones felt firm. I should suppose the woman's statement as to the period (four months) was quite correct.

Gravesend, August 1838.

ON SECALE CORNUTUM.

To the Editor of the Medical Gazette.

Sir,

If the following remarks be of sufficient importance, you will oblige me by giv-

ing them insertion in your well-conducted journal.—I have the honour to be,

Your most obedient servant,

EDW. AUGUSTUS CORY, M.D.
Member of the Royal College of Surgeons, Obstetrical Superintendent of the East London Lying-in Institution, &c. &c.

Cannon Street Road,
Aug. 10, 1838.

The perusal of a paper in a late number of the MEDICAL GAZETTE, by Mr. Armstrong, of Gravesend, on the Use and Abuse of the Secale Cornutum, has induced me to offer some observations upon the same subject. Your intelligent correspondent deserves much credit for the unostentatious manner in which he has communicated to the profession the result of his experience, and I feel assured that his candour and real desire to promote the advancement of medical science, will readily pardon the critical remarks which I have thought proper to offer upon some of the contents of his paper. I entirely agree with him as to the general inadmissibility of the ergot in abortion, unless, indeed, there should be hæmorrhage so copious as to threaten destruction to the life of the patient, and accompanied with parturient pains, as well as a fully dilated state of the os uteri. Under such circumstances I might be tempted to administer it, but on no account if the uterine orifice should be wanting in the necessary dilatation. The communication of your correspondent, however, appears to me to contain a few "*rari nantes in gurgite vasto*," which I cannot exactly reconcile to my own individual experience. I wish to ask him, what "high obstetrical authority" has sanctioned the exhibition of repeated doses of the ergot in cases where abortion is threatened at the fourth month of utero-gestation, and with a rigid and contracted os uteri? Indeed, I am inclined to the opinion that in Mrs. H.'s case, which he has quoted, abortion might, in all probability, have been prevented by the timely administration of an opiate, by perfect quietude, and cold applications; at any rate it ought, I think, to have been attempted. It also appears inexplicable, to my humble comprehension, how your correspondent can substantiate his assertion that the principal action of the ergot is to produce *firm contractions of the abdominal muscles*, and that the power which it exerts on the uterus is only secondary; or, to use his own words, that "*it is these contractions of the abdominal muscles which, by acting on the uterus, excite the increased efforts*," &c. It must be confessed that the *modus operandi* of secale cornutum, in the present state of our therapeutical knowledge, is involved in considerable obscurity. It is entirely *sui generis*. We are at present only acquainted with the effect of its operation; but I think it would be more in accordance with the principles of that inductive philosophy which was taught and practised by the illustrious Bacon, and which elucidates with all the rigour and exactitude of a geometrical demonstration, to state simply that it exerted primarily its peculiar action upon the uterus itself, and that the abdominal muscles were only secondarily affected. Hypothetical speculation regarding the operation of remedies, unless we have sufficient data whereon to base our deductions, is at best, if I may be allowed the expression, but a learned way of acknowledging our entire ignorance of the subject. I would not, however, have it imagined from these observations that I am inimical to sound and rational theory; by no means; for that would be equivalent to the assertion that a man ought not to exercise his reasoning faculties. The power of ratiocination is the highest prerogative of the human race. It is only against those fanciful hypotheses that I more particularly declaim — hypotheses not founded upon rational deduction, and which are therefore calculated to exert an injurious influence upon the principles which guide us in practice. They are like the blazing meteors which glitter for a moment in the firmament of heaven, and then rapidly disappear and are no more seen.

I cannot also understand how the intelligent and respectable practitioner alluded to by your worthy correspondent, could have been induced to administer the secale at the full period of gestation, the orificium uteri being rigid, and not larger than the circumference of a shilling, and more especially as the parturient pains were at the same time, as he very poetically expresses it, "like angels' visits, few and far between." It appears, moreover, that in this case the practitioner was subsequently obliged to give an opiate.

Such a mode of procedure certainly does not argue much *intelligence* on the part of the practitioner, but, on the contrary, a decided breach of established obstetrical rules.

I beg leave to be allowed to subjoin some practical remarks upon the secale cornutum, and to state the result of my own certainly not limited experience of the efficacy of that drug as an obstetrical agent.

The ergot of rye has been used empirically on the continent for nearly two centuries, by old women and midwives, for the purpose of accelerating the process of parturition when it may have been unnecessarily tedious. Some account of its peculiar properties will be found in an old French work, published in 1668, called "Actes des Curieux de la Nature." About one hundred years since, according to Desgranges, it was popularly esteemed in Lyons and its neighbourhood, and was thought to possess considerable power as an auxiliary obstetrical agent*. It has been also very strongly recommended by some American writers, and considered by them to be fully entitled to the praise which had been bestowed upon it†. The Germans, among whom it has been known for a long period, have attributed to it the possession of poisonous and intoxicating properties‡. This remark is, of course, only applicable to its administration in large and repeated quantities.

There is considerable contrariety of opinion regarding the obstetrical properties of the secale cornutum. Gardien§, Madame Lachapelle||, as well as Capuron, Desormeaux, and many other writers, both in this and foreign countries, with whose names every obstetrician must be familiar, are inclined to deny its utility under this point of view; whilst Chevreul¶, Villeneuve** and Velpeau††, have as strenuously maintained its utility and capability of exciting the contractile power of the uterus. Schneider* has cited a hundred examples of its obstetrical efficacy. It has also been supposed to possess properties deleterious to the life of the infant. This opinion is very prevalent in America; and Merriman†, as well as Ingleby‡, appear to coincide in this latter supposition. A full account of the injurious properties of the ergot has been given in the "Mémoires de la Société Royale de Médecine, p. 303—Paris, 1799." For my own part, although I have used this substance in upwards of one hundred cases, I do not recollect an instance where the death of the infant could fairly be attributed to its employment.

I am of opinion that the secale cornutum, when administered in those cases which are applicable to its exhibition, is of indubitable potency, and extremely valuable as an auxiliary obstetrical agent; and I can only attribute the discrepancy of opinion which prevails upon the subject to some impropriety in its administration. I mean, that either the drug itself had previously lost its activity, or that the case had not been adapted for its employment. I must confess that at one time I entertained a very strong prejudice against its utility, until the *experimentum crucis* of practical experience demonstrated to me its excellent effects in a very satisfactory manner. I have now given it at the full period of gestation, as a remedy for uterine inertia, in about one hundred and five cases, and it has scarcely ever deceived my most sanguine expectations. I would, *in limine*, wish to inculcate as an axiom, which I think the most extensive observation will not be able to controvert, that the secale ought never to be given at any period of utero-gestation, unless there be complete dilatation of the uterine orifice. I must, however, be permitted to make one solitary exception to this rule, and that is with regard to its administration for the purpose of inducing premature labour in the seventh month of pregnancy, in order to supersede the revolting necessity of craniotomy. It has been asserted by a most respectable authority (Dr. Francis Ramsbotham), that the secale cornutum is capable, *per se*, of exciting

* Nouveau Journal de Médecine, Chirurg., Pharm., &c. Janvier 1818.

† Vide Essay on the Medicinal Effects of the Secale Cornutum, by O. Prescott, M.A. New York, 1813. Also, New York Medical Repository, vol. ii. p. 303.

‡ Bautzmann, de Secale temulente, decuria iii. an. 7, p. 52. 1699.

§ Traité d'Accouch. tom. ii. p. 253.

|| Pratique des Accouchemens, tom. iii.

¶ Précis de l'Art des Accouchemens.

** Du Seigle Ergoté, &c. 1827.

†† Traité complet de l'Art des Accouchemens, tom. ii.

* Journal für Geburtshülfe, &c. von Siebold, vol. ii.

† Synopsis of Difficult Parturition.

‡ On Uterine Hæmorrhage.

uterine action and the expulsion of the ovum at the seventh month of gestation; and several examples of its successful operation have been recorded by that gentleman. I have tried it in a few instances of this kind, but only with partial success. I had recourse to the same formula for its exhibition as recommended by that eminent individual. One of the cases in which I used it was published some time ago in the MEDICAL GAZETTE, wherein the peculiar effect of the ergot was minutely detailed. I am rather inclined to the opinion, that had the powder been given in the cases alluded to, instead of the infusion or decoction, the event might have been more in accordance with my expectations. I would on no account advise its administration in the earlier months of utero-gestation as a remedy for hæmorrhage. I consider it extremely inappropriate in such cases. In abortions where the fœtus has been expelled, and the placenta, from some cause or other, is retained within the uterine cavity, so as not to permit of its removal by the fingers, and where the patient has been much reduced by the continuance of the hæmorrhage, I am accustomed to prescribe the ergot in order to promote the expulsion of that body, as well as to ensure afterwards the permanent contraction of the uterus. As a remedy for atony of the uterus, where the orifice of that organ has been for some time fully dilated, and there is sufficient pelvic capacity to permit the passage of the child with tolerable facility — where the pains have been previously in a state of activity, but have ceased almost entirely, or have only demonstrated their presence at long intervals, and moreover if the genital passage be well lubricated with moisture—the best possible effects will be produced by the exhibition of half a drachm of finely powdered ergot in water or tea, and which may be rendered palatable by the addition of sugar. Within twenty minutes, or before the expiration of half an hour, there will be a gradual increase in the severity of the pains, and the fœtal head will be found to descend. In a quarter of an hour the parturient throes will usually have acquired considerable activity, and the child will be expelled soon afterwards. If the first dose of the ergot should not produce the anticipated result, I give another dose (a scruple) of that substance in the same vehicle, which, if it be an applicable case, will seldom fail to produce the hoped-for effect. The operation of the ergot is liable to produce one very troublesome consequence, and that is, a constricted state of the os uteri, or even hour-glass contraction of that organ, which in a case of active hæmorrhage would be extremely embarrassing to the practitioner. I am justified in stating, from multiplied observations, that when this impediment to the passage of the placenta is found to exist after the use of the ergot, it may be generally inferred that that body is morbidly adherent to the uterus. I am not aware that the experience of other practitioners has led them to similar conclusions. This difficulty must of course be overcome in the usual way. I have also found the secale cornutum useful in small repeated doses, in some examples of hæmorrhage, after the fœtus and its appendages have been expelled from the uterus. There will doubtless be certain modifications of these conditions of the uterus occasionally presenting themselves, which the limits of this paper will not allow me to particularize; but from the remarks which have already been made, the judicious obstetrician will easily be enabled to discover those cases in which a beneficial effect may be reasonably expected from the administration of this medicine. It is, perhaps, unnecessary for me to state, that in order to ensure its proper action, it is of first-rate importance that the medicinal virtues of the drug should not have been dissipated; and this will certainly occur if the greatest circumspection be not used. The grains of the ergot must be selected as perfect as possible. According to the darkness of the colour, and the relative intensity of the peculiar odour of the drug, so will its activity be proportionably great. It ought to be finely powdered at the time it is required, and put immediately into a well-stopped bottle. I have found the powder comparatively inert when it has been kept in paper for only twenty-four hours. I have now used the secale cornutum in the form of powder for about six years. I consider this mode of exhibition preferable in many respects to decoction or infusion. I find that Velpeau, whose work I have only had an opportunity of perusing

within the last two years, has also recommended the powder as the most efficient preparation. He thus speaks of its superiority, in his "Traité Complet de l'art des Accouchemens," tom. 2, p. 67:—"La poudre fine de tout l'ergot me paraît préférable aux décoctions, aux extraits, pourvu qu'elle soit récente, et tirée de grains bien complets et bien conservés. On conçoit, au surplus, à combien d'opinions diverses il peut donner lieu sur son énergie et ses propriétés, quand celui qu'on emploie se trouve altéré."

In conclusion, I beg leave to state that I believe the ergot of rye will increase the action of the uterus with as much certainty as quinine will cure an intermittent fever, or as the best established aperient medicine will produce catharsis. I have thus given, although I fear but an imperfect description of those states of the uterus which are best adapted for the use of this singular medicine. I have been obliged to discuss the subject with Spartan brevity, lest I should encroach too much on the valuable pages of the MEDICAL GAZETTE.

MESMERISM.

MR. TERRY IN ANSWER TO MR. MAYO.

To the Editor of the Medical Gazette.

SIR,

IT is not without regret that I find myself again engaged in writing to you upon the subject of animal magnetism, for I have but little time, and still less inclination, for carrying on a public correspondence. But, sir, having lately come forward to assert my entire disbelief in the existence of any such real power or principle as that to which the name "Mesmerism, or Animal Magnetism," are applied, as well as to arraign the conduct and censure the judgment of those who are so assiduously, and, in my opinion, dangerously bringing it forward, giving by the aid of their respectability an open sanction to what I call absurdity; I say, that for these and other reasons, which I shall presently allude to, I feel that I cannot now consistently remain silent. My first letter to you, dated June 26, was called forth by the perusal of several papers which had been published in your GAZETTE, by Mr. Mayo, on the subject above alluded to; but it refers more particularly to a paper in the GAZETTE of the preceding week, June 19, which was then Mr. Mayo's last publication upon this subject. I thought it, at that time, more correct and courteous not to mention Mr. Mayo's name; my only wish was to oppose his doctrines. In deviating from that line of conduct now, I trust and expect that Mr. Mayo will excuse me, as I beg to assure him that nothing can be further from my intentions than the idea of writing in a manner even in the slightest degree personally offensive to him, or in any other way than is perfectly compatible with the established customs and usages of polite and professional etiquette. I have not the honour of knowing Mr. Mayo; but when I see his name announced as F.R.S., Senior Surgeon to the Middlesex Hospital, Lecturer on Surgery, and formerly Professor of Anatomy and Surgery to the Royal College of Surgeons in London, and when I know the value of his excellent work on Human Physiology, I cannot but look up to him with respect and esteem as one of those eminently and honourably distinguished by labour and by learning in the profession which he adorns. Still, he will allow me freely and unreservedly, though I hope respectfully, to canvass his opinions. As a public man, Mr. Mayo is well aware that he is amenable to the judgment of public opinion, whether for applause or censure; and as an eminent man, he is as well aware that his works ought to be the more minutely and carefully examined, inasmuch as their influence and effect for good or for evil, for weal or for woe, is greatly enhanced by his celebrity. Mr. Mayo's former publications about mesmerism I have thought most extraordinary; and in my letter to you I took the liberty to say that I requested all sound and sober-minded men to read them through, and study them attentively; but his performance, in the GAZETTE of the present week, No. 46, outdoes all that has gone before. I have just now mentioned Mr. Mayo's work on Human Physiology; it has happened to be my travelling companion for the last two or three days; and I have much pleasure in offering to the author the humble tribute of my praise. The work, so far as I have looked into it, is entirely free from those dangerous

and destructive physiological principles of which Mr. Mayo, I lament to say, is now the advocate; the arrangement is good, and the matter most interesting and useful. I hold it immaterial in such a case how much of it may be original composition, or how much compilation. The sentiment adopted as a motto, by the author of one of our medical dictionaries, as an old Latin quotation, seems applicable here:—"Non arenearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." To-day, my travelling companion on the road has been the GAZETTE above alluded to; and I have really laboured hard to understand the meaning, or unravel the mysteries of Mr. Mayo's observations. Can this, I said, be the man whose work I but yesterday studied with so much instruction and delight? Let it teach every man humility; let no man boast in pride of human intellect; let him learn from this sad example how soon fame falls, how frail is the tenure of earthly honours; how short the space and how rapid the descent from the sublime to the ridiculous. I have said that in my first letter I requested those who might honour me by the perusal of it to read attentively Mr. Mayo's publications, and then to favour the profession with their opinion of them. I the more earnestly repeat that request now, inasmuch as there are statements advanced and facts asserted in the paper I am at this time considering, which, if true, must completely overthrow and revolutionize all our preconceived opinions—theological, ethical, and physiological—of the powers and attributes of the human mind; "must set aside all true principles of reasoning, and make us victims of wildest theory." At the commencement of an article published in the Quarterly Review, vol. lxi., for January 1838, it is stated, "We are required to accept as indubitable facts, that a person can see with the tips of his fingers or the pit of his stomach—that the internal organization of his own frame or that of others placed in magnetic connexion with him becomes visible, so as to enable him to detect hidden disease and prescribe the efficient remedy—that he knows the unexpressed thoughts of people, and can foretel future events—that he can ascertain what is going on at indefinite distances, in defiance of the intervention of opaque bodies—and that all these things are best accomplished when the senses are closed, and the mind entranced. The Wilheimer somnambulist reads in the dark, with emphasis and earnest attention, a book of which he had no previous knowledge, by pressing it on his stomach. One of Professor Kieser's sleepwalkers recognised a seven of spades when the figured side of the card was laid upon his under lip. If a lens were placed near the tip of his nose, he could see with that organ as well as a waking man sees with his eyes."

Again, another author, describing the different stages of magnetism, says, "In the sixth and highest stage the powers of clairvoyance are freed from the limits imposed upon them in the fifth stage, and the perception of the patient is extended to persons at any distance, while his power of prevision, prophecy, is spread over an unlimited extent of future time." When I first met with these passages I thought, and that of course, that they were greatly exaggerated. I did not conceive it possible that any man, still less any body of men, considered to be, in the common affairs of life, "compos" or "compotes mentis," could hold forth and defend such delusion. But let us hear what Mr. Mayo says. He does not in this paper perhaps go into the whole of the mysteries of his art; but if I believe one such assertion, one such impossibility, as those I have quoted, I believe the whole. He says, "In this marvellous inquiry, wonder succeeds wonder. As soon as the mind has become reconciled to the idea that the contact of one's hand can communicate to certain substances the mesmeric influence, one finds beyond a yet more unlikely fact to examine and to be compelled to admit."

Again, more immediately to my point, I quote his words at page 774, section 4. He says, "Subsequently to my last communication I have witnessed in Elizabeth Okey the marvellous phenomenon of transposition of sensation. I have seen her on three occasions suddenly assume a new condition, and be without eye-sight, hearing, touch, or taste; while she has distinctly perceived some object placed a few inches from her through a sense more akin to vision than to any other, that has been suddenly developed in her hand." Again, in the preceding section, "A month before the 14th of June, Elizabeth Okey

declared during the mesmeric trance that on that day her usual delirium would assume a new character; that she would then cease to be without common bodily feeling, as hitherto was her state (and as her sister remains) during the mesmeric delirium or somnambulism;" and of course it was so. Again, to another part of the subject, page 773, sect. 3:—"Simply looking upon a mesmerizable substance placed at the distance of two feet from you will mesmerize it." Again, sect 4:—"If you look fixedly for a minute upon one spot of a mesmerizable surface, as for instance a stone mantel-shelf, a sovereign is then placed by the patient herself upon the spot which you have looked at (the sovereign having been ascertained the instant before not to be mesmerized)—if the sovereign be allowed to remain a minute upon the spot that you have so looked at, then on the somnambulist taking up the piece of money, mesmeric effects follow." But I need at present quote no more. The effect, however, of a battery of sovereigns—the greater influence produced by six sovereigns placed in Jane Okey's hand than that produced by only one, as at page 772, sect. 3, is certainly entertaining, and perhaps intelligible to ordinary minds. Now, sir, I would ask, is it not sad—is it not lamentable—in this enlightened 19th century, to have one's common understanding so assaulted? What is to become of the rising generation, if men high in repûtation, powerful in persuasiveness, and of great address—men who, as their teachers, are placed in authority over them, and to whose judgment they do and ought to pay much deference, hold forth and advocate such doctrines as these? As the father of ten children, six of them sons, one of whom is already initiated in the study of medicine, and must ere long be exposed to the dangers I am describing, I ask this question in sincerity, with feeling and dismay. And what, I would also ask, is to become of these poor infatuated girls? Have they no parents to rescue them from this thraldom? Not many weeks ago I read in your GAZETTE the account of a scene which one would have supposed must have enlightened even the most prejudiced mesmeric mind, and caused the immediate bursting of the bubble.

One of these girls, it appeared, had prophesied that something extraordinary would occur to her on a given day; and when the day arrived, the ordinary exhibitions were put a stop to by the girl in question suddenly assuming uncontrollable anger, and pouring forth upon her miscalled patrons such a volley of censure and abuse as hardly could be equalled in the lowest scenes of Billingsgate. Fool, impostor, rogue, &c. &c. worse and worse, were the terms in frequent use. How was this got over, and how was peace restored? Had the mesmeric battery of sovereigns no influence in this affair? Mr. Mayo, at the end of section 3, page 773, alluding to an experiment upon Jane Okey, says, "she being, of course (as for the other experiments), in the state of delirium." Does not this confession throw some light upon the subject? In a subsequent column he asserts, that "mesmerism is a peculiar physical influence emanating from living beings, and capable of producing sensible effects upon a greater or less number of our own species, and upon one or more species of the lower animals." Will he kindly inform me upon what species or individual animal less rational than mesmerized man, such effects can be produced? On Mr. Mayo's reference to facts in support of the doctrines he supports, I beg leave to quote the following appropriate observation of Dr. Cowan:—"We must never forget (he remarks) that the most visionary and hypothetical of writers is perpetually referring us to his facts, and the credibility of an author, especially in reference to such subjects as we are now considering, does not depend on his founding his opinion upon the result of positive experiment, but upon his powers of cool and unbiassed judgment." Now I have not the most distant intention of disputing or doubting Mr. Mayo's word, still less would I impute to him, even in the slightest degree, the intention to deceive, or any sordid unbecoming motive; but I disbelieve the accuracy of his observation, and I deny, *in toto*, the soundness of his conclusions. I disbelieve that any effect whatever could be produced even upon a tender infant, or upon any one individual species or member of the brute creation. I deny that upon any man whatever, whose judgment is not strongly biassed, and whose mind is not previously worked up to a pitch of high excitement, upon any man calmly and quietly possessing the "*mens sana in*

corpore sano," could any effect whatever be produced by mesmerism. Dr. Cowan states, "I have been tried by Du Potet himself, and two other professed magnetizers in succession, even for an hour and a half, and have not felt the slightest possible effects." What, then, can this power be which is now inert, now most energetic—to a man of sound unprejudiced mind the object of scorn and derision, to another the source of terror and dismay? Can it be other than the working or wandering of a disordered imagination, with the novelty of the circumstances by which the magnetized is surrounded; and is it not most dangerous, is it not positively a crime, so to disturb the mind, so to abuse those powers which God has given us for nobler and for better use? Mineral magnetism we know, electricity and galvanism we know; their effects, *cæteris paribus*, are uniform, and admit of demonstration; they act not only on rational but upon irrational animals—not only upon man, but upon beast—not only on animate, but upon inanimate matter. I can only presume, and that in charity, that as it is acknowledged to be indispensable that the patient for the exhibition of these experiments must be previously in the state of delirium, so the conductor of the experiment must be somewhat wandering too. I cannot suppose that Mr. Mayo wrote the article I am alluding to in the quiet retirement of his own study, but probably at the scene of action, and under the influence of a highly excited imagination. Is there not something much allied to monomania in this? Mr. Mayo, speaking of the caution he adopts to prevent the possibility of fraud or delusion, says, "For sake of brevity, I do not mention the precautions taken to prevent the patient, in this and in the other experiments, guessing when the effect is to take place and when not." How far Mr. Mayo has practised himself in the difficult task of detecting feigned disease I am not aware, but assuredly it is often a matter of great nicety and doubtful discrimination. I have had some practice in this way, and I feel myself competent to offer some remarks. Many years ago I served as an assistant-surgeon in the army, and I had the honour and particular good fortune to be engaged, not with sword or spear, but with mine own proper implement, at the great and ever memorable battle of Waterloo. Subsequently I have been, as I also am now, surgeon to the Northamptonshire regiment of militia. Now even amongst this honourable class of men, the soldiery—and as a body in that particular sphere of life I know of none whom I more highly appreciate—a black sheep, a malingerer, a man who, in the hope of escaping some unpleasant duty, will use every possible device to deceive his surgeon, is occasionally found; and I really know of no task in military practice more difficult than that of detecting feigned disease, or that determined and persevering asseveration of falsehoods, which it is hard to believe, yet difficult to deny. See the army surgeon attending a military punishment (in my opinion the very best mode ever devised for stopping the progress of military crime, and in every respect infinitely preferable to a gaol), the culprit, if an Irishman, will very soon exclaim, "Oh, doctor, doctor, take me down, for indeed I am dead!" He will feign or pretend to faint, and actually drop as far as his shackles will admit, and perhaps, by cunningly effecting some pressure upon his arm, he will stop the pulse at his wrist. Now if the surgeon, upon whom, on this occasion, all power and responsibility rests, with fear and apprehension liberates this man unnecessarily, he offends his commanding officer, and defeats the ends of justice; yet if, on the other hand, he admit of the whole punishment being inflicted, be it great or little, and the man, from some idiosyncrasy or latent peculiarity of habit, perhaps calling forth erysipelas, by chance does ill, that surgeon is brought to trial for manslaughter. See, again, the surgeon of a large gaol (and for nearly twelve years past I have held that appointment, in the prison and House of Correction of this county): here I am well aware that it is the first object of every prisoner who comes in sentenced to hard labour, to get on the blind side of the doctor; or, in other words, to induce me to believe that he is not fit to undergo the labours of the treadmill—that happiest instrument of man's ingenuity for the correction and punishment of vice, and from the proper use of which I have, as yet, known no ill effect to ensue. Not long ago, a case occurred in our gaol, illustrative of the above observations. A man whom I had examined told the

turnkeys, after I was gone, he did not like that doctor at all, for he (the prisoner) "knowed how to do his tongue very well, but he did not know how to do his pulse." It appeared, that in some other prison his easily discoloured tongue had been regarded as a symptom of disease, and his pulse had not been felt. Am I, then, far beyond a just conclusion when I say, that in such society I believe nothing that I hear, and only half what I see—that is to say, I am guided entirely by my own conviction, giving credit only to such assertions as I can understand. And if such be the case in the instances I have alluded to, what can we say—what can we expect—of these poor deluded hysterical girls, to whom, of course, in a pecuniary point of view, these exhibitions ought to be abundantly profitable, who are said to be of necessity in a state of delirium when the tricks are tried upon them, and of whom it is repeatedly said with the utmost *naïveté*, "so she was put into the state of delirium," "so she was put into the state of coma," &c. &c. and "so she recovered from the state of coma, first into a state of trance, and then into that of her ordinary delirium"! Still, sir, I am by no means ignorant of, or disposed to deny, the wonderful influence of mind acting upon bodily structure. I believe, and even magnetizers will probably be surprised at the assertion, that in some rare instances even hard horny substances, warts, have been removed by the agency of the mind alone; in other words, I believe that what is popularly called "the charming away of warts," has in some few instances actually taken place; but then this has usually been the work of gipsies and fortune-tellers, not of Physicians and Fellows of the Royal Society.

We all know the influence of mind acting upon the secreting glandular system; look, for instance, at crying, which in many persons, like myself, is as readily excited by joy as by grief—indeed, by any strong emotion of the mind; and look at many other secretions: may we not believe that the absorbent system may, under some circumstances, be as powerfully excited to inordinate action? Could the old and disgusting, but not yet entirely exploded, custom of rubbing a bronchocele with the hand of a dying malefactor, have originated and maintained its popular reputation so long, without some semblance of truth in regard to its effects? But all these facts, I contend, have nothing whatever in common with mesmerism—nothing to induce a comparison, still less to justify an analogy, between them, and those ridiculous, unphilosophical, and untenable practices, which I am endeavouring to expose.

And now, sir, I proceed (and I approach the subject with fear, and an entire distrust of my ability to do it justice), to a much more vitally important part of Mr. Mayo's paper; I refer to page 774, section 6, where Mr. Mayo advances doctrines which, as far as I can comprehend them, do not fall short of this assertion—that the study of physiology tends to materialism; that in proportion as our knowledge of the structure, the mechanism, and wonderful contrivance of the human body increase, so in an equal ratio does our propensity increase to deny the hand of God who made it. I am aware that such doctrines are not new; but I ask Mr. Mayo, has he studied with a devotional mind—has he read that wonderful work, his own creation, with the view of increased adoration to his Maker; or has he not rather given way to the dangerous inclination, of late too common, of indulging in vain, sceptical, irreligious, and even irrational hypotheses, till by doubt, distrust, and false philosophy, he has involved himself in a maze, a whirlpool of mystery—has cast off the anchor of religion, and sunk in fathomless, bottomless infidelity? for reason not less strongly confirms our assurance, than revelation originally inspired our belief of the agency and omnipotency of the Creator in the formation of his creatures. Has Mr. Mayo studied the Bridgewater Treatises?

From this passage we are led on to the end of section 1, page 775, where we arrive at what I consider the *acmé* of profanation, in a comparison and analogy of the ravings of mesmeric mania to the spiritual nature of the Deity:—"Man, we are told, was made in the image of God; these may be partial revealings of the parity of the spiritual nature of the created being to that of his Creator," &c. &c. Can I, as a sincere but humble Christian—a real believer in my Bible—in those doctrines of Christianity by which I try to live, and in which I pray to God that

I may be permitted to die; can I allow such doctrines to be spread—spread, too, in the minds of youth at the age of most dangerous susceptibility — at a moment when my younger professional friends have just left the home of their parents, or, I hope, the equally watchful home of some kind conscientious friend or master, whose first object it has been, with useful medical instruction, to keep his pupil's mind aright, and who, acting a parent's part, has kept him in the paths of religion and virtue? can I allow them, just freed from such restraint, and just commencing in the wide and wicked metropolis of this mighty empire, to think and act for themselves, "ullius addicti jurore in verba magistri?" can I, I repeat it, allow such doctrines as these to be spread without entering my determined, honest, and conscientious protest against them? And let me call upon Mr. Mayo to remember, that a publication which is erroneous becomes additionally pernicious in proportion to the influence of its author. Mr. Mayo states, p. 774, section 7, that the phenomena he has described "are already admitted as a part of nature by some of the first physicians and surgeons in France and Germany." My recollection may fail me, but I have an impression upon my mind, that whenever, even in these credulous nations—the latter of them, I believe, the birth-place of mesmerism—the question has been taken up as a public investigation, it has been invariably rejected; and will Mr. Mayo kindly furnish the public with his list of English advocates of mesmerism? Where are those eminent men of London, my tutors, whose lectures I heard with admiration and delight some thirty years ago, and whose high professional and moral bearing I have honoured and esteemed ever since: are *they* converted?

I regret that my remarks have extended to this unexpected length; I also regret that I shall not be able to send them in time for your next Gazette, No. 47, as it happens unfortunately that the Gazette printed in London on Saturday, is not received by the bookseller who supplies our library till the ensuing Wednesday.

I fear, sir, that in my very superficial reading, which is almost exclusively confined to the time spent in my gig upon the road, I have overlooked some of your editorial remarks upon mesmerism. Will you do me the favour to direct my attention to those you allude to in the comment attached to my letter of the 26th June*. I am aware of some subsequent observations, expressive of your indignation at one particularly disgusting scene; but I shall look back with pleasure to those of a prior date; had I seen them, I should probably not have troubled you with that, still less with this address. I am not desirous of publicity; and as a proof of this, I told you before, that though I had seen your work from its earliest infancy, I had never before troubled you with correspondence; but the subject has compelled me to come forward as I have done.

If in this discussion I have used Mr. Mayo's name too freely, or combated his opinions too severely, I here beg leave to make apology. I am opposed to the doctrines only, and not in the most distant degree to the man. If he censure my judgment, as I also do his, I trust that by neither one nor the other of us will there be any imputation of wrong motives.

I request the favour of an early publication of this letter; and have the honour to be, sir,

Your obedient servant,

H. Terry,

Surgeon to the Northampton General Infirmary.

Northampton, August 20, 1838.

TREATMENT OF FRACTURES.

To the Editor of the Medical Gazette.

Sir,

As the attention of your readers has been lately called to the treatment of fractures of the extremities by what has been termed the *appareil immobile*, perhaps you may deem an account of some cases of fracture of the leg, to which this plan is peculiarly adapted, that have been treated in the Middlesex Hospital, as not unworthy a place in your journal. These cases were all under the care of Mr. Arnott, who has most kindly permitted me to publish them.

The first case which was treated was one of simple oblique fracture of the tibia, about the junction of the middle

[* We alluded to a set of editorial articles, the first of which will be found at page 824 of our preceding volume; and also to a leading article, published Sept. 16th of last year.—Ed. Gaz.]

and lower thirds, occurring in a young man aged 24, and occasioned by a twist of the foot whilst struggling with some of his companions. In six hours after the accident, there being very little tumefaction, the limb was done up with the rollers, starch, and pasteboard. Four days after, the covering being perfectly dry and firm, he was allowed to get up and go about the ward on crutches, the leg being supported by a sling passed round the neck and under the foot.

On the twelfth day after the accident, the patient having stumbled, and fancied that he had displaced the ends of the bone, the apparatus was removed; but as his apprehensions were found to be entirely groundless, it was again done up as at first, but, from the rollers not having been drawn sufficiently tight, it became necessary, five days after this, to do it up afresh; and this third covering was allowed to remain on till the bones had become firmly united. The patient left the hospital thirty-seven days after his admission, with a strong and useful limb, without any deformity.

The second case was that of a lad, aged 16, who was admitted June the 4th, with simple fracture of both bones of the left leg. As there was some degree of tumefaction of the limb, it was considered that it would not admit of being immediately bound up; it was therefore placed in junks, and cold water applied. On the 14th day, the swelling having almost entirely subsided, it was done up with the rollers, starch, &c. In three days after this, the covering having become quite dry and firm, he was allowed to go about with crutches and a sling. In thirty-six days after the accident (three weeks after the application of the apparatus), he left the hospital; and as the bandages were still quite firm, they were allowed to remain. The patient returned a week after this, when the bandages were all removed, and the limb was found quite straight and firm. In this case the apparatus was never changed, slackened, or even nicked, from the time of its first application to its final removal four weeks afterwards, and on no occasion did the patient complain of the slightest pain or uneasiness.

The third case is one of simple fracture of the tibia, in a young man of 19, who one night, whilst soundly sleeping on a hay-stack, contrived to roll off and break his leg, a misfortune which he did not discover until he awoke on the following morning. The leg, which at the time of his admission was somewhat swollen, was kept in junks for 13 days, and was then done up with the rollers, &c. In two days, the weather being very warm, the apparatus was quite dry and firm, and the patient was allowed to get up, with the same precautions as in the former cases. At the end of three weeks the apparatus was removed, and the leg found to be quite straight and firm.

As these cases are, I think, sufficient to prove the great superiority of the present mode of treatment of fractures of the lower extremity over those commonly in use, it will be as well to describe more particularly the mode of constructing the permanent covering for the limb which has been adopted in this hospital. It consists in first of all applying, with a tolerable degree of firmness, a narrow leg or arm roller from the toes up to the knee. This is then washed over with strong, warm starch; a compress of lint is then placed on each side of the tendo Achillis, and a second roller is brought down from the knee to the toes; and a second coating of starch applied. Four strips of strong pasteboard, broader above than below, previously softened by immersing them in boiling water, are placed on each side of the limb, extending from the upper part of the leg to the sole of the foot. These are then firmly secured by a third roller, which is afterwards well soaked with starch. The limb is then placed on a pillow resting on the outside with the knee bent, until the whole apparatus is thoroughly dry.

The principal points to be attended to in the application of this apparatus are, 1st, the degree of tightness of the rollers, which must be such as to give a firm but equal pressure, without being so tight as to interfere with the circulation of the limb; and, 2ndly, the length of the pasteboard splints; for unless these are sufficiently long to reach down over the heel and malleoli to the sole of the foot, their ends will, to a certainty, so fret and irritate the skin, as to render it necessary to ease it by cutting the roller or pasteboard, or to remove the apparatus altogether. By proper attention to these points, there is no difficulty, as proved by cases 2 and 3, of so applying the apparatus as to render it per-

fectly unnecessary to remove, or in any way disturb it, until the fractured bones are firmly united.—I am, sir,

Your obedient sevant,

HENRY ROGERS,
House Surgeon.

Middlesex Hospital,
August 14th, 1838.

ORIGIN OF THE OPTIC NERVES.

To the Editor of the Medical Gazette.

SIR,

THE fact of the optic nerves arising in, or at all events having an intimate connexion with, the corpora quadrigemina, is, I believe, generally admitted in the present day, but is not so decided as to render uninteresting any pathological facts which may tend to confirm it. If, in this point of view the accompanying case should appear to you of sufficient interest, I shall feel obliged by its insertion.—I have the honour to be, sir,

Your obedient servant,

GEORGE KERRISON, M.D.

129, Mount Street,
Grosvenor Square, Aug. 13, 1838.

I was called, on the evening of April 23d, to see Mr. B., æt. 66, a person of very temperate and regular habits. He stated, that for the last three years he had been gradually getting more and more blind; and that during the last two months he had suffered from a constant "heavy pain" in the occipital region, which he described as being "like a worm moving about inside." It had been gradually getting worse, and he had never had any medical advice for it.

When I saw him, the symptoms were as follows:—There was complete amaurosis, with dilated and insensible pupil on the right side, and very indistinct vision (almost amounting to amaurosis) in the left eye. There was much dulness, and at the same time anxiety, in his countenance; his mind was much confused; his answers sometimes rambling; and his utterance slow and heavy. His hearing, and all his other senses, were perfect; he had the full use of all his limbs, and he never had a fit of any kind. There was no tenderness on pressing the scalp in the region affected; the pain was not influenced by any variety in his position (as erect, or horizontal), but the least quantity of wine or beer rendered it insupportable; pulse 76, small and feeble; action of the heart natural; appetite tolerable; tongue white, and somewhat furred; he complained very much of a bitter taste in his mouth; the bowels were in general regular. Occasionally there was some irritability about the neck of the bladder. He stated that he had never had any fall or blow on the head.

I ordered a blister to the back of the neck, and directed him to take a saline aperient regularly, so as to ensure a full evacuation of the bowels every day, with an occasional dose of blue pill. He was afterwards cupped twice in the back of the neck, the pulse having become stronger; and a second blister was subsequently applied. Under this treatment his general health improved very much, and on the 7th of May he said that he felt "quite well," and had no pain in his head.

May 8th.—He had this day, for the first time, a fit, which was attended with some convulsive twitching, and was followed by coma, which lasted for two hours. From this he had recovered when I saw him; but the pulse being 110, and rather sharp, I ordered him to be bled to ʒviij., which relieved him very much, and in a few days afterwards a seton was made in the back of the neck, which discharged well, and gave him great comfort. From that period up to the 2nd of June he gradually improved in every respect; his only complaint being of the blindness, which continued unrelieved, and of weakness.

June 3rd.—This day he had a second fit, similar to the former one. It came on quite suddenly, and the coma which succeeded it was of longer duration than that which followed the first.

13th.—Since the last report there had been no particular change; but on the afternoon of this day he had another fit, the bowels having been for a day or two previously obstinately constipated. This was relieved in the evening; but he remained for two or three days afterwards in a very feeble state.

17th.—His nights for some time past have been very restless, notwithstanding a pill with Ext. Hyos. gr. vi. which he has taken at bed-time. Last night he was exceedingly violent, trying to get out of bed, &c. He was perfectly composed when I saw him; but I ordered him a draught with gr. ss. of Morph. hydro. chlor. to be taken at bed-time.

18th.—I was unable to see him early this morning; and at 3 P.M. was called to him, in consequence of his having been in a comatose state since the previous night. He died a few minutes after I reached the house.

Sectio cadaveris, 42 hours after death.—The calvarium was with much difficulty removed, on account of its firm adhesion to the dura mater, the vessels of which were in a state of considerable congestion; and on the external surface of this membrane, in the occipital region, there was an effusion of about four ounces of blood. On slicing the right hemisphere of the brain from above downwards, the anterior part of the roof of the lateral ventricle appeared healthy; but the posterior portion was much softened, though not discoloured. The same was found on the left side, but to a greater degree. Behind the third ventricle, and pressing on the corpora quadrigemina, and also partially on the cerebellum, there was a tumor the size of a walnut, of a cartilaginous nature, but which in some parts was soft and easily broken up. It was partly surrounded by a softish substance, having some points of bloody infiltration. This extended for some distance into the left hemisphere, on which side there was also about ʒiss. of an amber-coloured gelatinous effusion lying over the choroid plexus, and in the posterior cornu of the lateral ventricle.

The other cavities of the body were not allowed to be examined.

REDUCTION OF DISLOCATIONS.

To the Editor of the Medical Gazette.

SIR,

THE inclosed is the draft of an apparatus for reducing dislocations. If it is deemed sufficiently important to place before your numerous readers, it is at your service.—I am, sir,

Your obedient servant,

WILLIAM CRAIG.

Ayr, August 6, 1838.

I would have had more satisfaction in offering the present communication if I had enjoyed an opportunity of using the apparatus upon a case of dislocation of the hip-joint; but the comparative infrequency of this kind of accident, even in hospital practice, held out little hope of meeting a case. The difficulty sometimes experienced in accomplishing reduction of old luxations of this joint by the means hitherto used, renders any attempt by other means to lessen the difficulty, of some importance in these operations. It is to be hoped, therefore, that the mechanical arrangements here proposed will in some measure contribute to this end. It has been completely successful in my own hands in reducing several dislocations of the shoulder-joint. It has been used with equal success in the Glasgow Royal Infirmary, as may be observed in the report given by Dr. Davidson, in the Edinburgh Medical and Surgical Journal for January 1838. Of the cases on which it has operated, none have been of longer standing than two or three days; but the ease with which reduction was effected, the prospect is held out of equal success in the most difficult cases.

It will be observed, that the power here employed is the screw. The gradual and steady manner in which this power may be used, makes it valuable in recent cases; and the ease with which it can to any extent be augmented, renders it peculiarly fitted to cases of long standing.

I was not aware, till it was pointed out to me by a gentleman eminent in the profession, to whom I exhibited this draft, that the screw had been used by the ancients to reduce luxations. The manner in which it was employed is represented in one of the works of Scultetus (*Sculteti Armamentarium Chirurgicum*, plate 23, vol. ii.—1741).

It is there exhibited applied to the shoulder-joint. The construction of the apparatus is unwieldy and inconvenient, and wants useful provisions which are found in the one represented in the draft before you.

I was unable, without presenting an additional figure, to point out in what manner the shield *r* is supported under the axilla when the apparatus is applied to the shoulder-joint. It is effected in the following manner:—There are two perforations in a direction from before backward, near the lateral edges of that part of the apparatus which rises behind the patient, when he is seated as in fig. 1. They are at a height convenient to the shoulder. The shield *r*,

fig. 1, is fixed to an iron bar, and one of the ends of the bar passes through the perforation, and is secured by means of a nut and screw at any point of the perforated six inches, suited to the size of the patient. The patient is placed, as represented in fig. 1, with the arm tightly braced by the straps *g*, to the cross bar *d*, which bar at the beginning of the operation ought to be at the point of the screw *a*. In order to make extension, the screw is turned by the

Fig 1.

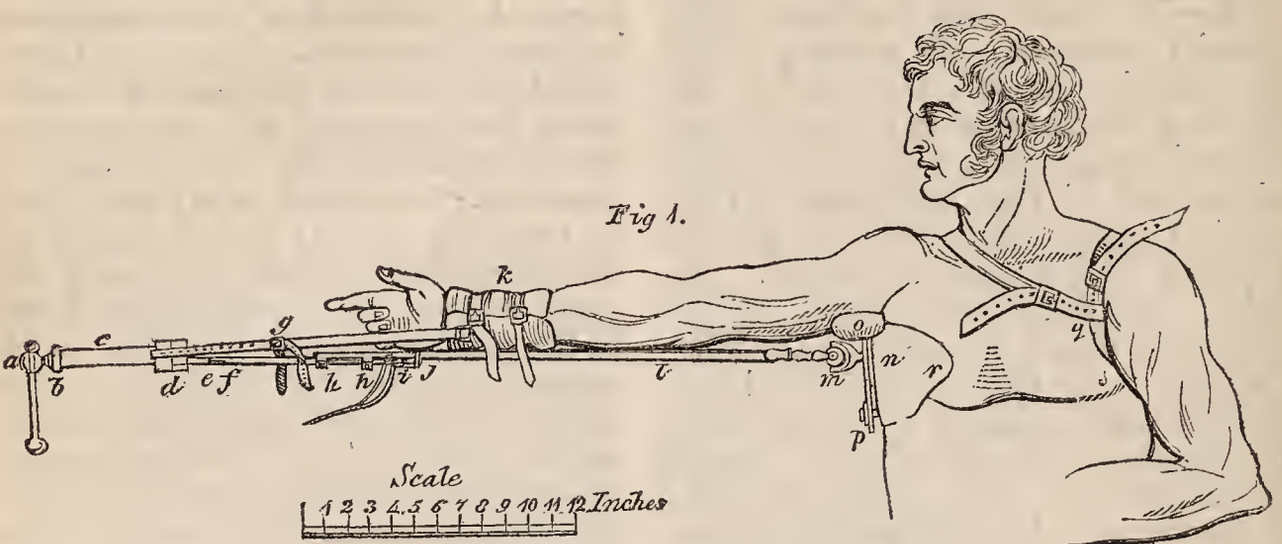
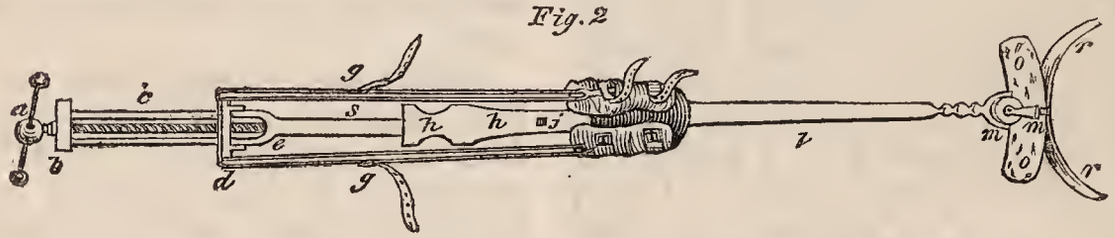


Fig. 2



a Is a lever to which the hand can be applied to turn the screw *c*, which acts in the female screw formed in the centre of the cross-bar *d*, causing it to approximate towards either end of the frame *b e*.

b e Is a frame containing the screw *c*, and terminates in the rectangular rod *f*.

d, A cross-bar, perforated at both ends to allow the straps *g g* to pass through, and in the centre is found the screw *c*.

f, A rectangular rod continued from the frame *b e*, and passes under the flat rod *l*, to which it is held close by the perforated studs *h h*, and prevented during extension from slipping beyond the point *j*, by the spring lever *i i*, the end of which is bent to a right angle with the horizontal part from the fulcrum *s*, and enters into the flat rod *l*, at the point *j*, on fig. 2.

m, A universal joint, permitting extension to be made at any angle with the body.

o, A firm cushion fixed on the shield *r*, and forming a fulcrum for the humerus, when the arm is made a lever to raise the head of the bone from the axilla into the glenoid cavity.

r Shews the shield applied to the chest, being well padded on the side next the thorax, and is fixed in such a manner to the chair, as to be easily raised or depressed to suit the size of the patient.

lever at *a*, which makes the bar *d* approximate the *b* end of the frame, *b c*. When sufficient extension has been effected, it is suddenly broken off by depressing the lever *i*, and the iron rod *f* sliding on the rod *l*, the arm is immediately depressed; and the humerus acting as a lever, having the pad *o* for its fulcrum, raises the head of the bone to the glenoid cavity.

When reduction of the inferior extremity is to be effected, the apparatus is put in the form of a couch, upon which the patient is laid. The adaptation to the perineum is made to rest on the tuberosities of the ischia, which causes the force of the counter-extension to be borne principally by these parts; and at the same time it is so formed as to secure the genitals from any risk of injury. To prevent the pelvis from following the traction

of the extending power, the sound extremity is firmly embraced behind the condyles of the femur by a projecting spar. The extension is produced in the same manner as above described in the case of the humerus. A pad pressing on the thigh, close to the adaptation to the perineum, is intended to raise the head of the bone over the edge of the acetabulum, and this is effected by drawing the extended extremity towards the sound one.

In those cases where the head of the bone is in the ischiatic notch, the thigh may be raised, as is directed when operating with pulleys; or a pad might be introduced in a proper situation under the thigh, to raise the head of the bone, which would be accomplished by depressing the limb, thereby using it as a lever, with the pad for its fulcrum. The apparatus was out of my hands before I thought of this provision.

There are several advantages possessed by this apparatus over the means commonly employed, which at first sight may not be observed.

One advantage is, that the surgeon, without assistance, can perform the whole operation. The best instructed assistant at the pulleys cannot apply the proper amount of power required in the case, so well as the operator himself, as he feels what effect is produced on the extremity as the extension progresses, and is thereby enabled with his own hand more exactly to proportion the power to the necessities of the case.

Another advantage is, that less power seems to be required, on account of its moral effects on the patient. There are few patients who can properly estimate the power of the screw; and even considerable extension may be effected before the patient is aware that it is in progress; whereas at a formidable array of power, either by six assistants or the less fearful rope and tackle, the patient takes the alarm, and braces every muscle that is connected with the dislocated joint, to resist a power which he supposes may altogether deprive him of his extremity; and there is thus produced an opposition to the extension which is very considerable, and which is sometimes attempted to be counteracted by the efforts of the operator to abstract the attention of the patient.

Another matter, in cases of the shoulder-joint, which is of some importance to the patient, is the little annoyance given him by the method in which counter-extension is effected. The shield under the axilla embraces a large surface of the chest, over which the force of the counter-extension is equally diffused, and is unaccompanied with that compressing effect which is inseparable from the manner of applying all kinds of bands.

It will be observed that these remarks apply more particularly to the provision for the shoulder-joint As the apparatus has not been yet applied to the inferior extremity, its fitness for operating on the hip-joint has not been determined by actual trial, but it is now in the North London Hospital, under the care of Mr. Liston, where an opportunity may soon be presented for testing its efficacy in these important luxations.

EXTIRPATION OF THE EYE,

On account of a Tumor developed within the Optic Sheath.

By R. Middlemore, Esq.
Surgeon to the Birmingham Eye Infirmary.

(From Transactions of Provincial Medical and Surgical Association.)

—— Hands, æt. 3, a very healthy-looking child, was brought to me at the Eye Infirmary, in consequence of a slight strabismus, presumed loss of sight and fulness of the left eye, which had been first noticed by his parents two months ago.

State of the Eye. — The cornea is slightly nebulous, the eye a little more protruded than its fellow, and it is evident that the power of sight is entirely lost. The parents and brothers and sisters of the child are very healthy. The defect of the eye cannot be traced to a fall or blow, or to any accident or circumstance of any kind.

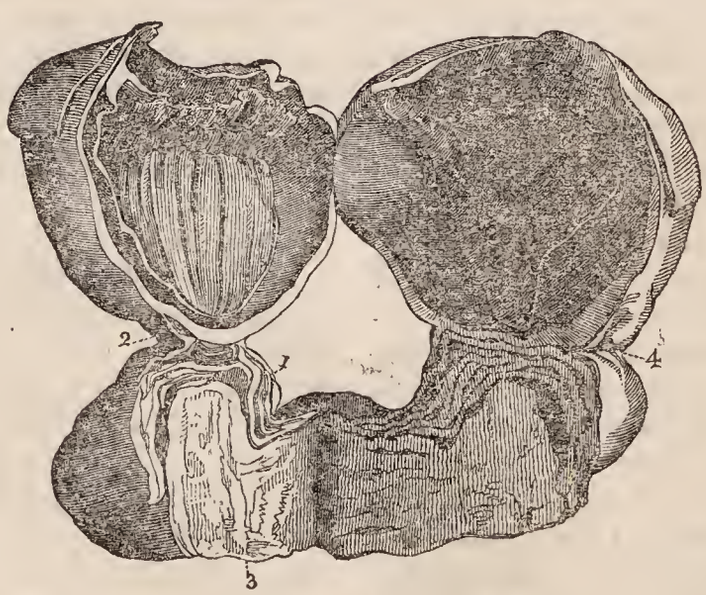
Progress of the case.—In the course of three months, the eye-ball became considerably protruded and much inflamed, and the whole cornea assumed a decidedly nebulous appearance. The iris was pushed towards the cornea, but was not distinctly inflamed. The pupil was slightly muddy, but there was no deep-seated shining opacity at its fundus, nor did the eye itself appear to be much enlarged. A degree of fulness at the upper and outer side of the eye-ball may be perceived on close examination, when the palpebræ are widely separated. The child is very restless, and

is manifestly suffering some degree of pain. It was evident that the performance of an operation constituted the only feasible means of relief.

Operation.—March 18th, 1837: present, Mr. Hodgson, Mr. Ledsam, and Mr. Crompton. Having lengthened the intertarsal slit by an incision towards the temple, discharged the humours of the eye, passed a strong ligature through the sclerotica, a little behind the margin of the cornea on each side, and, by its agency, drawn the eye forwards and upwards, I made a pretty deep semicircular incision through the conjunctiva, and somewhat beneath the globe, from the inner to the outer canthus, and united the extremities of this incision by a similar one made at the upper part of the eye-ball. The tumor was of considerable size, and extended, as I imagine, through the optic foramen, so that it was not, perhaps, wholly removed; however, with the curved scissors I succeeded in clearing the orbit. The soft and slippery character of the tumor, and the depth and situation of that small portion which, I think, remained, rendered it somewhat difficult and dangerous to continue my attempts to extirpate every portion in the situation of the optic foramen. On the completion of the operation, the little patient was extremely exhausted from loss of blood, and required the use of pretty active stimulants for at least an hour afterwards. The orbit was now carefully sponged, a thin fold of linen dipped in cold water was lightly bound upon the eyelids, and the patient put to bed.

On the following morning the lids were a little swollen, and on the next day they were excessively tumid; this, however, subsided in the course of a few days. The orbit discharged a good deal of pus for three weeks, and when this ceased, as the child had apparently perfectly recovered, I ceased to attend it. Since this period I have occasionally seen the little patient, and with a view of completing my notes of the case, called and examined him today (Feb. 28, 1838.) The orbit appears free from disease; the eyelids are quite healthy, and are slightly drawn inwards by the absence of the eyeball. The right eye has a rotary motion, but vision is perfect. The intellect is unimpaired; but the power of the right (the left eye, it will be remembered, was removed) hand and arm is diminished, and the child drags the right foot slightly, very slightly, along the ground when walking or running.

Dissection of the contents of the orbit immediately after their removal.—The eye-ball appeared healthy, except that its humours were slightly turbid, the cornea somewhat opaque, and its back part near the optic nerve slightly indented by the pressure of the tumor. The optic sheath was a little thickened and much dilated by the large tumor, and especially so near the optic foramen. A portion of cellular matter, ap-



Section of the Eye-ball, Optic Sheath, and Tumor.

1. The optic sheath.
2. The connexion of the optic sheath with the sclerotica.
3. The tumor.
4. The unabsorbed cellular membrane of the optic nerve.

parently the cellular membrane formerly connecting together the fibrillæ of the optic nerve, was observed between the tumor and the optic sheath; this was of a yellowish colour, most abundant near the cribriform portion of the sclerotic coat, and condensed into one or more layers in those situations where, from the greater size of the tumor, &c., it would be exposed to the greatest degree of pressure. The tumor, covered by this cellular tissue, and by the sheath of the optic nerve, was of considerable size; its greatest bulk being situated near, but not close to, the optic foramen. It was of a yellowish colour, and of a texture resembling the mucilaginous nasal polypus, only rather fibrous. By immersion in spirit, it assumed a firm, fibrous, and whitish appearance. The preparation is added to my pathological collection, which I shall have real pleasure in shewing to any member of the profession who may wish to examine it.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Treatise on the Nature and Treatment of Hooping-Cough, and its Complications. Illustrated by Cases, and an Appendix. By GEORGE HAMILTON ROE, M.D. Oxon. Fellow of the Royal College of Physicians, and Physician to the Westminster Hospital.

FOR many years past we have had opportunity of witnessing the course and effects of hooping-cough on a considerable scale, and under various circumstances, and have felt the want of a history of the disease more fresh and full, and especially better illustrated by cases, than could be expected in dictionaries of medicine and other systematic compilations. We have pleasure, therefore, in finding a subject so important occupying the attention of a physician possessed of apparently ample means for its elucidation. In official, domestic, and other private practice, the author seems to have made hooping-cough an object of especial attention for a considerable series of years; and in this volume he submits to the judgment of the profession and public the more important results of his experience.

The essay on Hooping-cough, which occupies about four-fifths of the volume, is divided into twelve chapters, including the introduction, detailing successively, in separate chapters, the symptoms — the morbid appearances — the causes and seat—and the different methods of treatment recommended by previous writers; after which the author treats, in separate chapters, of simple or uncomplicated hooping-cough (chap. viii.); and of hooping complicated with bronchitis (chap. ix.)—with pneumonia (chap. x.)—with hydrocephalus (chap. xi.)—and with remittent fever (chap. xii.) The author closes with a summary of rules for the treatment, according to his own views of the disease. There are also separate chapters (iv. and vi.) on the explanation of the symptoms, and on the question of contagion.

From the nature of the disease there is no difficulty usually in the diagnosis of hooping-cough; we shall therefore pass over the first chapters. The third chapter contains a better account than any we recollect to have seen, at least in any British work, of the morbid changes observed in connexion with the disease, illustrated by particulars of eleven dissections. The fourth chapter, on the *ratio symptomatum*, is interesting to the general reader, for whose satisfaction apparently the author intended it. The fifth chapter gives a particular account of the views entertained by authors as to the proximate cause and seat of the disease, the author's opinion being, as expressed in the words of Blache, whom he cites as having anticipated him in the conclusion, "that hooping-cough is a nervous affection, having its seat both in the bronchial mucous membrane and in the pneumogastric nerves; an affection very often complicated with bronchitis and pneumonia, but capable of existing without them; and, like all other diseases of the same kind, having no anatomical marks of any importance." Chapter vi. discusses the important subject of contagiousness, upon which the author decides in the affirmative; and then, after having, in chap. vii., enumerated and commented shortly on the principal remedies recommended by previous writers, he enters, in chap. viii., on the treatment according to his own views. This practical part of the Essay is that by which the success of the writer is mainly to be judged of, since he informs

us that his principal motive in undertaking the work was to put in a just light the merits of a remedy not previously unknown, certainly, but yet, in his opinion, very inadequately employed and appreciated — namely, hydrocyanic acid. As the uncomplicated hooping-cough is the common and only pure form of the disease, and as the author's mode of using the prussic acid is best illustrated by his account of the treatment of the simple form, we shall analyse the 8th chapter, which is occupied with that form exclusively, at some length, passing lightly over the following, from want of space for fuller details.

Simple hooping-cough the author defines to be, practically, that form in which there is no graver complication than a slight catarrh. The symptoms of this form are most effectually encountered, he finds, by a combination of prussic acid and ipecacuanha or antimony: to such a combination he has found most cases yield sensibly in three or four days; the whoop ceasing altogether before the close of the first week of treatment. The dose of the agent to which he attaches most importance, viz. the acid, varies according to age and other circumstances, not exceeding a minim every fourth hour, for children under three years, of Scheele's acid. The ipecacuan or antimonial should be given in doses too small to excite vomiting. Hemlock has occasionally produced in the author's practice a powerful effect in allaying cough, though not equally powerful with the acid, and has been most useful when the catarrhal symptoms predominated over the spasmodic. His dose of the hemlock for an infant has been 1 gr. of the extract with three or four drops of the vin. ipecacuanhæ, every second, third, or fourth hour, until the dyspnœa yielded. When the cough became hard and dry, and without expectoration, the author found benefit from adding some laudanum or liq. opii sed. to the acid and antimonial — three-quarters of a minim of the former and half a minim of the latter to each dose.

Emetics the author has found useful only as facilitating the expectoration of mucous accumulations, when such are indicated by loud wheezing with dyspnœa. He advises the suspension of the acid whenever symptoms of weakness shew themselves, such as languor, feeble pulse, &c., and the substitution of stimulant antispasmodics, especially opium and ammonia, with improved diet. The form preferred by the writer for cases where the skin has "become cool, the pulse feeble, the eye languid, and the cough rather teasing than violent," consists of spirit. ammon. comp. 3 drops, with three-quarters of a drop of laudanum, or half a drop of liq. opii sed., to be given every fourth hour to infants, and proportionally large doses at more advanced ages; the diet comprising for infants beef-tea, and for older children meat, and even wine

With regard to *change of air*, pronounced by Heberden of itself sufficient, the author "is satisfied that it cannot be relied on," "having frequently known it tried, but very seldom with success." "Its principal use," the author conceives to be, "to strengthen the system after exhaustion by a severe protracted cough, so as to enable it to overcome the diseased habit." "At the commencement of the complaint," the author very judiciously, as we think, remarks, "if the child be in tolerable health, perfect quiet and a warm atmosphere contribute much more towards the cure of the disease than change of air; but if the subject be a child whose constitution is so weak as to require more attention than even the disease, and whose delicacy renders him unfit to contend with the malady, it may be necessary to strengthen him by all natural means, at the same time that appropriate remedies are administered: for this purpose, if it be warm, summer weather is specially useful."

With respect to relapses, the author considers that the abridgment of the disease by the acid treatment, by preventing general exhaustion and diminishing local excitement, decidedly diminishes also the liability to relapses; and it is principally on account of the power of shortening the duration of hooping-cough, shewn in his experience to be possessed by the Prussic acid, and the consequent diminution of risk to the whole constitution and to the organs more particularly affected, that the author would recommend that remedy for the disease to the especial attention of his professional brethren. To this account of the author's method of treating simple hooping-cough are subjoined about two dozen cases illustrating the practice recommended.

The following chapters treat of complicated hooping-cough, and are each illustrated by appropriate cases. It is out of our power to enter into any analysis of these; but we may state summarily that the general impression left on our mind, by a careful perusal of those, as of the other practical parts of the work, including the chapters on the management of children, is, that they recommend a practice generally bold and decisive, and at the same time prudent and trustworthy; untrammelled by scholastic prejudices, and guarded in its application by every precaution suggested by a large experience and conscientious sense of duty. On the whole, we feel ourselves called on to say that the present volume is a well-timed and valuable addition to the literature of juvenile disease, and is highly creditable to its author as a practical physician.

A Treatise on Inflammation. By James Macartney, M.D. F.R.S. F.L.S. M.R.I.A. F.S.S.L. &c. London, 1838. 4to. pp. 214. With two plates.

The first section of Dr. Macartney's work is entitled the "History of Inflammation," and begins by the observation that "there is no reason for believing that a process of the same nature of [as] inflammation exists under any circumstances in the vegetable kingdom." (p. 1.) Nor does it take place in worms, oysters, or crabs; nor is it possible to produce the genuine effects of inflammation in either of the two classes of vertebrate animals with cold blood. When worms perforate the tunics of the alimentary canal in fishes, no change in structure is caused, except an increased vascularity around the perforations. Crabs and lobsters, however, in whom there is no appearance of inflammation, are able to reproduce a lost limb; fish, in whom there is only a flush of inflammation, reproduce the fins. From these and other instances, the author concludes that the power of reproducing a lost part is in proportion to the incapacity for inflammation; and that inflammation, far from being necessary, as every one has hitherto supposed, to the process of reparation, is a hindrance to it. The author attributes the established opinion to "those rude ages of the art, when nothing was trusted to nature, and when the treatment of every wound was such as to induce and maintain for a certain time the most severe inflammation. The ignorance of the ancients of the use of the ligature for suppressing hæmorrhage, led them to employ, instead of it, strong compression and the actual cautery in cases of wounds and after amputation; and being accustomed to see wounds ultimately heal after such barbarous treatment, they naturally supposed that inflammation and all its evil consequences were necessary; which they took care to insure in all cases, by boiling oils, hot and irritating ointments, tents, setons, and strict and cumbrous bandages." (p. 7.)

The reader who turns from this quotation to Celsus's directions for treating wounds, Lib. v. cap. 21 et seq., will be surprised to find that there is not a word about boiling oils; that, instead of producing a severe inflammation, we are cautioned against it; and that "the use of the ligature for suppressing hæmorrhage" is mentioned in the very plainest terms. The words of Celsus are—"Quod si illa quoque profluvio vincuntur, venæ quæ sanguinem fundunt, apprehendendæ, circaque id, quod ictum est, duobus locis deligandæ interciendæque sunt, ut et in se ipsæ coëant, et nihilominus ora præclusa habeant." If this fails, the actual cautery is to be used.

At p. 184 our author again attacks the Augustan physician; for, after mentioning the water-dressing employed by Hippocrates, he says, "This simple practice was set aside, by the Arabian physicians and Celsus having introduced a variety of absurd and complicated medicines into fashion, which held their ground until the 14th century," &c.

Leaving the Arabian doctors to shift for themselves, we must protest against Celsus being included in the indictment, for he is in favour of simple dressings. His words are—"Licetque sine peregrinis, et conquisitis, et compositis medicamentis vulnus curare." (Lib. v. cap. 23.)

If, however the practitioner is not satisfied with this, he may use compound preparations, varying according to circumstances. The first recommended in this passage is a plaster called *barbarum*, consisting of verdigris, litharge, alum, pitch, resin, oil, and vinegar—a composition not deserving the hard names

bestowed upon Celsian medicines by Dr. Macartney.

Passing over the next section, "On the Phenomena of Inflammation," we come to the one on "The Consequences of Inflammation." The only true ones, according to our author, are, chemosis, œdema, vesication, suppuration, and the total disorganization and death of the part.

He excludes the effusion of coagulable lymph and ulceration, both of which he places in the next section, which treats "of the reputed consequences of inflammation." After observing that coagulable lymph may be thrown out by a healthy action, as in the function of the decidua of the uterus, and that it is eminently conservative in arresting hæmorrhage from opened vessels, and so on, he says:—

"In discussing this question with surgeons, it has generally been raised as an objection to my opinions, that the cure of hydrocele is effected by injury, followed by inflammation. There are two ways of obliterating a serous cavity. The one is, to commit such an injury on it as will furnish the motive for the effusion and organization of lymph, by which inflammation is anticipated and prevented; the other is, to cause inflammation and suppuration to supervene; and after they have terminated, the remains of the granulations serve as the uniting medium, as in any common wound. The two modes of treating hydrocele are founded on these rules. When injection is used with success, the inflammation of the tunica vaginalis is prevented by the effusion of lymph. When incision, caustic, or the seton, are employed, the hydrocele is cured after the inflammation has ceased, and not before. We might with equal propriety consider the approximation of the edges of an ulcer, and its final cicatrization, inflammatory actions, as the cure of hydrocele by the medium of granulations."—P. 38.

The difference in opinion concerning the method in which hydrocele is cured by injection, is a dispute about words merely. The surgeon who employs an injection does so in order to produce the minor degree of inflammation characterized by the effusion of lymph; no, says our author, it is in order to anticipate inflammation by the effusion of lymph. In the other mode of curing hydrocele, it is necessary that the granulations should be preceded by inflammation, and therefore the objections of the surgeons seem to us rather hard to get over. Could these granulations be produced if the inflammation was immediately stopped, after the application of the caustic*? The practical difference between Dr. Macartney and those surgeons who endeavour as much as possible to cure wounds by the first intention, is next to nothing; what they would call stopping the necessary degree of inflammation, he calls stopping the process of reparation, and that is pretty nearly the whole difference. In the section on the remedies for inflammation, when recommending the employment of cold as a remedy, he cautions us against using it without limitation, because "the reduction of the heat of any part of the body in an *extreme degree*, renders the existence of inflammation impossible in that part during the time; but as cold is a direct sedative to all vital actions, there is a temporary suspension also of the process of reparation. The remedial operation of a *moderate degree of cold* is in most cases to be preferred. It diminishes in place of extinguishing sensibility and vascular action, and under its operation the reparative processes can be carried on."—P. 157.

Having thus given the most prominent part of our author's theory, we will just mention a few other remarkable points, without attempting to give an abstract of the remainder of the work:—

Ulcer. — Dr. Macartney strangely enough objects to this word;—"The term *ulcer* is not a correct one. It expresses only a part of the history of the object to which the name is given; or applies to that state in which the ulcerative process only is going on," &c. &c. (p. 46.) Why, surely these objections apply to all words whatever. Are *artery*, *abscess*, and *cancer*, a bit nearer perfection?

Reparation by the modelling process. —This means the supposed process of healing without inflammation, and is described in the following words:—"The mode of reparation by the mo-

* It might be alleged, perhaps, on Dr. Macartney's side, that although inflammation is requisite to form a preternatural adhesion in the cure of hydrocele by injection or caustic, that it may still be unnecessary when the organic instinct throws out granulations to fill up the cavity of an ulcer, the latter being a process of reparation, but not the former.

delling process has never been described; because surgeons, heretofore, did not know that it was possible for open wounds to heal without inflammation in the higher classes of animals. However, when healthy parts are injured, although it may be to the greatest extent, if placed under the most favourable circumstances for carrying on their natural actions, the process of reparation is nearly the same, even in the human subject, as that which I have described as belonging to the animals of a simple structure. The pain arising from the injury soon ceases. No tumefaction ensues separating the edges of the wound, and its surfaces are not only disposed to lie in contact, but even to approach each other so much that they cannot be kept asunder by mechanic restraint; there is, therefore, no necessity for the effusion of lymph; and as there is no cavity to be filled up, granulations are not formed. The surfaces of the wound, although they come into contact, do not unite by vessels shooting across; they are smooth, red, and moistened with a fluid, which is probably serum, and present the appearance of one of the natural mucous surfaces of the body. If any parts have been killed by the injury, they are separated by simply as much interstitial absorption as is sufficient to set them free. The wound is finally healed by the same means which determine the shape of the natural parts of the body. It gradually diminishes in extent until it is obliterated, or it may be cicatrized before the surfaces are abolished; after which the same process of natural growth goes on, until no part of the original wound is left. The cicatrix which succeeds the cure of the injury by the modelling process, is small, pliant, free from those callous adhesions to the parts underneath, and the morbid sensations that so often belong to those cicatrices which have for their bases the deposits of lymph or the formed structures called granulations. When the modelling process or cure by natural growth goes on perfectly, there is no inflammation in the part, and the patients are so entirely free from all uneasy sensations, that I have known instances of their being ignorant of the real site and extent of the injury, until they had examined the part with their hand, or saw it in a looking-glass."—(Page 53-4.)

Morbus Anatomicus.—This disease is to be prevented by washing the wounded part, and afterwards keeping it wet for a few hours with a solution of alum in water. According to our author, this preventive is infallible; and he speaks from the experience of the last fifteen years that he was professor of anatomy in the University of Dublin.

Tartar Emetic. — Dr. Macartney says, "it has become fashionable of late to give and repeat large doses of this remedy, on what grounds I cannot understand, as small doses are sufficient to produce all the effects we wish for, provided they be repeated with sufficient frequency."—(Page 143-4.)

The grounds are clear enough, being that real or supposed experience on which every thing in physic or surgery depends. Rasori, Laennec, and other practitioners, *haud infimi subsellii*, found, or thought they found, that large doses of the triple salt arrested inflammation sooner than small ones, and prescribed them accordingly.

Venesection and Arteriotomy.—Our author says, that "directions are not commonly given in works on surgery on this subject," and therefore describes these operations at considerable length. They are, however, described at still greater length in Cooper's Dictionary—a work which is in everybody's hands.

Corrosive Sublimate.—Dr. Macartney observes, "some forms of mercury are more deleterious than others. The corrosive sublimate, when administered in full quantity, acts like a mineral poison on many persons. In a case that occurred in this city, where a woman attempted to poison her husband with this preparation, although she failed in her object, he became a miserable spectacle; being afflicted with tremors of the muscles, a variety of nervous symptoms, and continued disorder of the alimentary functions."—(Page 163.)

It strikes us that there is a singularity in these sentences amounting to a considerable defect in style; as the young reader would conclude, from the way in which the facts are set forth, that there was something new in them. Nevertheless the case is a valuable corroboration of a well-known point in toxicology.

Cure for Tinea.—The lead lotion, says our author, never fails to cure tinea capitis, if properly applied. "The hair should first be cut close to the head, but

need not be shaved off; water dressing, or a poultice of any kind, is then to be applied, merely for the purpose of cleansing the skin of the crusts, and all other impurities. There will then be seen under each crust a red spot of the skin, denuded of its cuticle, and the villous surface exposed. The lotion should now be applied by means of lint thoroughly wetted with the fluid, and covered with a piece of Indian-rubber, or a piece of oiled silk, to prevent evaporation. Every time this dressing is changed, which should be frequently at first, the head should be washed with some of the lotion, and the lint should be replaced by some that is clean, which is to be completely wetted with the lotion, and covered as before." (P. 172-3.)

The practitioner is to observe, that if the application is suspended, even for a few hours, the viscid secretion will reappear, and the whole treatment must begin again. It is sometimes not safe to cure old cases very quickly; and different parts of the head must then be cured in succession.

Perhaps the most interesting part of the work, in a practical point of view, is the account of the steaming apparatus, invented by Dr. Macartney, with a portion of which we will conclude our extracts.

"After many experiments, I arrived at the principle, in the formation of the boiler, which enabled me to generate not only steam as hot as it could be be borne, but at all degrees downwards, to below the standard temperature of the human body.

"The apparatus consists of a small tin boiler, supported on a platform, on which a spirit lamp is placed. The peculiarity of the vessel is, that the superior opening is an expanded funnel, in consequence of which the steam "ascends from the boiler with a vertical motion, being attracted to the smooth and infundibular aperture. The effect of the steam escaping in this manner is to diffuse and cool it so much, that if the vessel be uncovered, the hand may be placed within an inch of the surface of the boiling water, without experiencing any unpleasant feeling of heat, although, if the steam be made to spread in a straight line, by diminishing the opening of the funnel, it will scald the hand held two or three feet above the water. The steam is conducted to any part of the body, by means of a tube of woollen cloth, about twelve inches wide and three feet long. Its cylindric form is maintained by circular pieces of whalebone. One end of the woollen tube is tied round the contracted neck of the boiler, and the other end admits of being adapted to the shape of any part that is intended to be fomented, from holding within its opening a piece of flexible wire.

"By this apparatus, steam at any temperature may be applied for any length of time, with only the momentary interruption of renewing the boiling water, and the spirit in the lamp. The great advantage of making a continued, instead of a temporary application, at a determined temperature, and without the intervention of the woollen cloths used in common fomentation, which irritate many wounds and ulcers, gives to the mode I have described for administering steam, the character of a *new remedy*, which it exhibits also in its more extended and more beneficial effects than those of common fomentation." (P. 176-7.)

Steam, at a high but comfortable temperature, according to our author, when applied immediately after the infliction of lacerated, gun-shot, and punctured wounds, fractures near joints, &c. &c., removes all pain and consciousness of injury in a very short time. Hot steam improves the condition of indolent ulcers; while steam at a low temperature is the best local remedy in active inflammation. In this state it removes almost immediately the painful sensations of ophthalmia, and the vascularity of the conjunctiva which attends it. An entire steam bath may also be procured by means of the apparatus; the apparatus itself is figured in one of the plates, to which a minute description is appended.

This long account, and these copious extracts, will have enabled our readers to anticipate our judgment—that, although Dr. Macartney's theory is probably not strictly correct, yet that the practice founded on it is generally defensible, and often laudable. Many of the details relating to water-dressing will be consulted by the student with great advantage. The book is a very handsome one, but printed in too expensive a form for general circulation. There are only thirty-two lines in a full page; and although the fair margin will

delight the lovers of tall uncropt copies (to speak *à la Dibdin*,) yet the attendant price will affright the student of medicine. In plain English, we think the book so instructive, that we would rather see it in the form of a five-shilling duodecimo than a fifteen-shilling quarto.

MEDICAL GAZETTE.

Saturday, September 1, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

RECENT EVIDENCE ON THE MEDICAL RELIEF OF THE POOR.

IT must be confessed that the question which has lately occupied the attention of the Poor-law Committee, some of whose reports are now lying before us, is no easy one *. Stripping off the thin disguises with which it has been invested, the real problem to be solved is, how to obtain the services of medical practitioners for next to nothing, and at the same time put a stop to their complaints of being underpaid, and the complaints of the poor at being ill attended.

When we find that the medical officers of the parish of Great Yarmouth, containing a population of 21,115, receive among them a salary of £120 per annum †, we are obliged to allow that the first part of the problem is completely solved in this district; as to the second, the sceptical may be permitted to have their doubts.

The plain truth is, that both under the old and the new system, the remuneration for attending the poor has always been so small, that every thing like an approach to a decent compensation would be scouted as something too absurd to be thought of for a moment; for as the wildest dreams are said to be founded on waking thoughts and the ordinary events of life, so the most sanguine hopes and largest demands have usually some relation to previously existing facts. Hence from this constant and undeviating system of infinitesimal pay, the expectations of the paid have been lowered with their miserable remuneration, and parish surgeons are perhaps the only class of persons who never even ask for one-fifth of what in justice they should receive. A very easy calculation will make this evident to every one. Suppose a philanthropist to request some practitioner to attend a poor patient at his expense; it will be allowed on all hands that half-a-crown a day would be a very moderate charge; and if we suppose this to include medicine, many would be inclined to divide the merit of the charitable act between the philanthropist and the practitioner. Half-a-crown a day, however, little as it is, amounts to £45. 12s. 6d. a year, a sum which, according to established skin-flint opinions, is amply enough for the surgeon who attends all the poor in a district containing three or four thousand souls; nay, according to the Great Yarmouth scale, £45 will pay for about 8000! Or, to view the question on another side, as threepence per head on the whole population is a common rate of payment, a surgeon ought to attend all the sick poor of a moderate-sized county, containing 120,000 inhabitants! in order that he might obtain the same salary as an assistant-commissioner under the act for *amending* the poor-law (1500*l.*)

It is almost unnecessary to observe, that the cost of good and wholesome drugs, given without let or stint, would

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* Forty-fourth, forty-fifth, and forty-sixth Reports from the Select Committee on the Poor-law Amendment Act; with the Minutes of Evidence, and Appendixes. (Medical Inquiry). Ordered by the House of Commons to be printed, 20th and 22nd June, 1838.

† Reports, &c., p. 15, Appendix No. 1.

swallow up the greater part of the salaries, and in many instances would go far beyond them. Thus the parish of Great Yarmouth, with its population of 21,115, would furnish at least fourteen hundred cases a year; and it appears from Dr. Bigsby's pamphlet on Dispensaries, that in twenty-two of these institutions, where the average annual number of patients was 26,479, the cost of drugs and medical appliances was 2s. 1½d. per patient *. Even, therefore, if we suppose some of the "medical appliances" to be furnished by the parish, it is clear that the drug-bills will far outrun the salaries.

In another instance (p. 103-4), the practitioner who attends half of a London parish, containing 40,000 inhabitants, tells us that he receives 110*l.* per annum, and that his drug-bill is about 50*l.* The constant number on his sick list is from 25 to 30, and his net salary being about 3s. 4d. a day, his remuneration per case is from 1¼d. to 1½d. Supposing him to have only one thousand cases a year, his drug-bill is less than half the dispensary average. Is the parish dealing fairly with this surgeon, or does it encourage him to give the proper medicines to his indigent patients?

As the dissemination of truth, and not the maintenance of any exclusive theory, has always been our object, we are ready to acknowledge that this branch of relief was almost, if not quite, as ill administered under the old as under the new law. Nay, more; the same tricks are practised abroad as at home; and the mayor of a commune in the south of France might seem worthy of being a guardian of the poor in the Bridgewater Union. In a letter dated from St. Astier, near Périgueux, Dr. Valbrune gives an account of the treatment to which country physicians are subjected in France*. On one occasion he was requested by the mayor of a neighbouring commune to visit a Polish refugee, who had been shockingly ill-used by the fellow-labourers who were employed with him in making a canal. His lungs were in a state of congestion from the blows he had received on his chest; and Dr. Valbrune attended him with the greatest care for twenty days, being obliged to go a great distance to see him. The charges in Dr. Valbrune's bill, according to the tariff fixed by law, amounted to 85 francs, but only 15 were paid; and he was told to get the rest either from the mayor, who had desired him to visit, dress, and treat the wounded man, or else from the unfortunate patient himself. On remonstrating, he was informed that he ought to have had a fresh order for each visit.

Another time two gendarmes came to his house with a letter from the King's Attorney at Périgueux, ordering him to go with them to the chief place of a neighbouring commune. Dr. Valbrune went there, and lost the day in listening to the depositions of an *officier de santé*, and several other persons, on the subject of a sudden death; his opinion being thought necessary for the discovery of the truth. He gave his opinion and went away, and, after some time had elapsed, sent in his account. A month afterwards it was returned to him, with the order that it should be drawn up according to the form published in an old ordinance. This was done, and the bill was deposited in the bureau of the King's Attorney. No answer was given, but meantime the year rolled away: then, and not till then, Dr. Valbrune was told it was too late; a statute of limitations was pleaded against him! Really this is mighty neat, and might have occurred nearer home.

——— mutato nomine de te (Bridgewater) Fabula narratur.

* Reports, &c. Appendix. Papers delivered in by Wm. Farr, Esq. p. 142.

* Gazette des Hôpitaux, June 12, 1838.

Another time he was summoned as a witness to the Court at Périgueux, to give evidence in a case where an *officier de santé* had dressed a wound in his presence. His recompense was a piece of paper promising a payment of three francs; which, however, have not yet been paid: three francs for travelling six leagues, losing his day, and leaving his patients unseen; three francs, just as if he had been a common labourer, who, having but twenty sous a-day for wages, would have thus gained two francs, besides the pleasure of amusing himself in court.

Some months afterwards a patient of Dr. Valbrune's having received several blows on his ears, called him in, and afterwards summoned him on the trial. This took place at Ribérac, nearly five leagues from Dr. Valbrune's house. His evidence was not required, but he was allowed six francs, on paper that is, for they have not yet been paid.

Last summer a body was found in the river, at the extremity of the commune in which Dr. Valbrune lives. At about six in the morning he was desired to go to the spot. On arriving, he found marks on the body which might be taken for signs of violence; the high-road from Paris to Bordeaux passes near the spot, and the deceased was not recognized as an inhabitant of the country. He put down every thing scrupulously, without forgetting the smallest particular. Before drawing up the *procès-verbal*, he observed, reflected, entered his study, and began to write; but before finishing, he returned to see the body again, to survey all the surrounding objects, and satisfy his conscience. Twelve hours were spent in fulfilling this painful duty. On asking what he was to be paid, "Nothing," was the reply; "a physician ought to be proud of attracting the attention of official persons, and deserving their confidence: such labours are not paid for."

From these and other facts detailed in this letter, Dr. Valbrune draws some conclusions in which we entirely concur, and which would be equally applicable on this side of the channel.

That in the country, far more than in towns, the poor would often perish were it not for the humanity of medical men.

That as in towns there are benevolent institutions for the poor who cannot obtain admittance into hospitals, so there ought to be similar ones in the chief places of rural districts, which should pay their medical attendants.

That the practitioner who is required to visit a patient by a mayor, justice of the peace, King's attorney, &c., should be remunerated for all the visits paid, and operations performed during the treatment undertaken in consequence of a regular order.

That written statements (*procès-verbaux*) require time and trouble to draw up, and subsequently deserve their reward, as well as a journey or an operation.

The picture which these statements and proposals give of the medical relief of the poor across the channel is so sombre, that few of our readers will be tempted to exclaim with Sterne, "They manage these things better in France;" on the contrary, the candid will admit, what they would previously have had some difficulty in believing, that it is possible to manage these matters worse than in England.

In a future article we will proceed to consider the evidence given before the select committee, and the remedies proposed for the present unbearable state of things.

ADMIRALTY GOLD MEDAL.

The Admiralty gold medal has been awarded to Dr. C. M'Arthur, surgeon of the Rodney, for having furnished the best medical journal in the service.

ANIMAL MAGNETISM.

THE following interesting experiments, performed by Mr. Wakley, completely confirm the view taken by our correspondents, Mr. Blake, Mr. Cooke, and Mr. Wooldridge, with regard to the O'Keys, who have recently figured so conspicuously at University College Hospital: we copy them from the Lancet of this day, September the 1st:—

Experiment on Elizabeth O'Key.

After some of the often-repeated experiments had been performed by Dr. Elliotson, with various results, it was proposed by the Doctor that the metal, *nickel*, should be used, the effects of which he said had been found by him to be, and would now prove to be, quite astounding. A piece of nickel was produced by the Doctor, of about three-quarters of an ounce in weight, and of an oval form, and also a piece of lead, of nearly the same shape and smoothness, but somewhat larger. Elizabeth O'Key was then seated in a chair, being, as was stated, in the "ecstatic delirium." A piece of thick pasteboard was placed in front of her face, and held in that situation by two of the spectators. By this contrivance it was rendered impossible that she could see what was passing either below or in front of her. Mr. Wakley being seated directly opposite to the girl, and at a short distance from her, received the lead and nickel from Dr. Elliotson, in order that he might rub the two on her hands in such a manner that from merely touching the substance, or from its form, it would be impossible for her to decide which of the two was being used. Dr. Elliotson had, as has been stated, previously described the effect of the magnetic nickel to be of a most extraordinary character, and said at the same time, with much earnestness, that the lead might always be applied with impunity, as no magnetic effect ever resulted from the application of that metal to the skin.

The substances were then applied to the hands of the girl. First the lead was applied to each hand, alternately, but in a manner which might have led the girl to believe that both metals were used. No effect whatever resulted from these applications. After the expiration of a considerable period, the nickel was employed, having been received from Dr. Elliotson, who for some time had held it in his hand in order to charge it strongly with the magnetic influence. By this proceeding the metal was necessarily heated to the temperature of the skin. Mr. Wakley had previously thought it right to hold the lead in his hand, and heat it in a similar manner. In consequence of this obviously necessary precaution, no indication was offered to the mind of the girl by which it could be guided, during the experiment, by the mere temperature of the substances which were employed.

The nickel was now used, precisely as the lead had been applied. There was a pause. The expected results did not appear. After, probably, a minute had elapsed, the lead was again used; and then again; and after the last application of the nickel, the lead having been repeatedly employed during the interval, the face of the patient became violently flushed, the eyes were convulsed into a startling squint, she fell back in the chair, her breathing was hurried, her limbs were rigid, and her back and abdomen assumed the position which are produced in an attack of opisthotonos. In this state she remained during nearly a quarter of an hour. Certainly that time elapsed before the condition of the patient appeared to warrant a repetition of the experiment. A short conversation then ensued between Dr. Elliotson and Mr. Wakley, as to the cause and reality of the symptoms. The Doctor contended that the effects clearly resulted from the application of the magnetised nickel, but that they had not come on with their usual rapidity. Mr. Wakley expressed a contrary opinion, and wanted to know of what value the experiments could be if there were nothing like certainty in the results, and if the effects were to be attributed to one metal so long after another had been employed, as on that occasion. Ultimately it was determined that another experiment should be tried with the nickel, Dr. Elliotson suggesting that that metal, in its magnetised state, should alone be employed.

Mr. Wakley was now again the operator, and before the experiment was tried he stated, privately, to Mr. Clarke, that instead of using nickel only, he would not on this occasion employ any nickel, and desired Mr. Clarke to take notice of the fact, that he would put aside the nickel, unperceived by any other person, the moment that it should be handed to him by Dr. Elliotson, and before either of his (Mr. W.'s) hands should be allowed to come in contact with those of the patient. The experiment was then again performed. Mr. Wakley had taken the nickel from Dr. Elliotson, and put it on one side, when it was taken, unseen by any other person, by Mr. Clark, who placed it in his waistcoat pocket, and walked with it to the window, there remaining during the performance of the experiment. Mr. Wakley employed both hands, but his fingers were so held that it was impossible for any person excepting the operator to know what he was hold-

ing. Presently, on applying the substance which he held in his left hand to the right hand of the patient, the pasteboard being again held before the eyes of the girl, Mr. Herring, who was standing near, said, with much sincerity of feeling, in a whisper, but loud enough to be heard at a short distance, "Take care; don't apply the nickel too strongly." Scarcely had these words escaped from his lips, when the face of the girl again became violently red; her eyes were fixed with an intense squint; she fell back in the chair; a more evident distortion of the body ensued than in the previous paroxysm; the contractions of the voluntary muscles were more strongly marked, producing a striking rigidity of the frame and limbs, and the shoulders were thrown back to the utmost, the spine displaying as complete a bow as in an attack of opisthotonos: in a word, the severity of all the symptoms appeared to have undergone marked increase. Dr. Elliotson again observed, that "no metal other than *nickel* had ever produced these effects; that they were most extraordinary;" in fact, that "they presented a beautiful series of phenomena." This paroxysm lasted during upwards of half an hour, and was admitted by all who were present to be much more violent than the one which had preceded it.

Mr. Wakley now suggested that the girl should retire into an adjoining room, where her sister was waiting, as he was anxious to make a statement to Dr. Elliotson in her absence. The girl objected to depart, and therefore her sister was called from the adjoining room, and the gentlemen retired into that room. Mr. Wakley then said to Dr. Elliotson, "that he felt it to be his duty to state, that the Doctor was entirely deceived respecting the character of the experiments and the cause of the symptoms. That all present had been witnesses of the violent effects which appeared to result from the application of the nickel to the hand, and had heard Dr. Elliotson state that such extraordinary symptoms could be produced by no other magnetised metal, whereas he had not used nickel on that occasion; he had not even *approached* her with it: but that on the instant that it was handed to him by Dr. Elliotson he had put it aside, unobserved, and had merely rubbed upon the skin of the girl a piece of lead and a farthing, which he had respectively held in either hand, but that the metals were so held that he was certain that no person could discover what he was applying."

Dr. Elliotson replied, that "he saw the nickel used; that Mr. W. must have touched her with that metal without knowing it himself; that he was certain of the fact, and that he was positive that the effect could be produced in no other way."

Mr. Wakley then said that there was a gentleman present who could confirm the accuracy of his statement—a witness, in fact, who had the nickel at that moment in his pocket, and had stood with it at the window during the whole of the time that he was applying the lead and the farthing to the hands of the girl.

Dr. Elliotson again declared that this was impossible, when Mr. Clarke produced from his pocket the piece of nickel, and said that it had really been there during the whole of the experiment, and that it had not been near the girl during the entire trial.

After a somewhat lengthened conversation Dr. Elliotson suggested that the experiment with the nickel should be tried once more. This proposition was consented to; and during the performance of the experiment Dr. Elliotson remained in the other room, while Mr. Wakley, Mr. Herring, and Mr. Clarke, went to the patient to renew the operation. Again was the nickel handed privately to Mr. Clarke, and the lead and farthing were applied as before, with the pasteboard held in front of the patient's face. In three or four minutes there was a re-appearance of the flushed countenance, the staring eyes, the rigid limbs, the bent back, and the distorted frame, although *no nickel had been used*—nothing, in short, but the lead and the farthing.

A report of these results was conveyed to Dr. Elliotson and the gentlemen who had remained with him, when Dr. Elliotson said that the occurrence was most extraordinary; that he could not at that moment account for it; and that he had no doubt that an explanation could soon be found which would remove all appearance of anomaly in the results. He would, he said, again suggest that the nickel should be re-employed; and as this request was so urgently made, Mr. Herring, Mr. Wakley, and Mr. Clarke, again visited O'Key, for the purpose of proceeding with the trial, but, instead of using the nickel, the lead and farthing were again employed, with the same results as before. There was another fit. Afterwards, when the girl had recovered from the apparent paroxysm, Mr. Wakley suggested that the magnetized nickel should be rubbed over both hands freely, on the skin, in different places, but not exactly in the manner in which the lead and farthing had been employed. No effect was produced by this application of the nickel.

On hearing a further report of the effects which appeared to arise from the use of the lead and the farthing, and the absence of effects when the nickel was

really used, after the other experiments had been concluded, Dr. Elliotson candidly admitted that he "could not explain how the thing had occurred; it was most extraordinary, but still he had not the slightest doubt that the whole would yet admit of a satisfactory explanation."

Mr. Wakley, on the other hand, contended that what had been done was, in his opinion, perfectly conclusive with reference to the character of the supposed phenomena, and that he did not consider that a single additional experiment could ever be necessary in connexion with such an inquiry.

Similar experiments, and with similar results, were again made on the 17th, and Jane O'Key, the other sister, also subjected to scrutinizing trials with mesmerized water, sovereigns, &c. &c. It is sufficient to remark, that on proper precautions being used, all the experiments totally failed. So much for mesmerism!

GLASGOW EYE INFIRMARY.

Clinical Lectures by Dr. Mackenzie.

August 25th, 1838.

Lecture IV.

Chronic Entropium—Crampton's Operation.

Alexander Gavine (No. 8740), aged 45 years, admitted August 22, 1838, says his right eye has always been tender. At present the right upper eyelid is affected with chronic entropium; the eye-lashes, which are few and dwarfish, rub on the eye-ball; the conjunctiva is inflamed and thickened, and the greater part of the cornea is opaque. This eye is intolerant of light, the conjunctiva discharges puriform mucus, and the eyelids adhere together in the morning. For two or three days after evulsion of the eye-lashes, he feels the eye much easier, and the vision becomes a little clearer.

Between three and four months ago his left eye became affected with catarrho-rheumatic ophthalmia, which produced a large onyx, ending in an extensive ulcer of the cornea. This disease was treated with blood-letting, general and local; blisters, calomel and opium, paracentesis oculi, and other remedies. There is a large leucoma, occupying the centre and lower part of the cornea, hiding almost entirely the pupil, and probably attended by synechia anterior. The conjunctiva lining the left eyelids is still inflamed, and the eyelids adhere together in the morning.

The right eye is fully the more affected with pain. Pulse 84; general health good. He was ordered an ounce of castor-oil.

23d.—Two incisions were made, with small probe-pointed scissors, through the whole thickness of the right upper eyelid, perpendicular to its edge; the one at its temporal, and the other at its nasal extremity, avoiding there the punctum lacrymale and lacrymal canal. The superior palpebral artery bled considerably. A transverse fold of the skin of the eyelid was then laid hold of with the entropium forceps, and removed with a pair of bent scissors. The edges of this last incision were now brought together with two stitches, and the threads, being left long, were laid up along the forehead, and kept there by strips of adhesive plaister, so as to prevent the eyelid from falling down.

After the operation was finished, he said he could open the eye better than he had been in the habit of doing. The parts were covered with simple dressing and a roller.

25th.—Dressings removed. Threads keep their places. Eyelid as after operation. Left conjunctiva less red.

Entropium is the result either of injury or of inflammation.

Traumatic entropium is the consequence generally of a burn or scald of the conjunctiva, or of some caustic substance, such as quick lime, getting into the sinuses of the eyelids. It is often attended by symblepharon, and sometimes the cartilage of the inverted eyelid is partially destroyed.

Inflammatory entropium is either acute or chronic. In the acute, there is little organic change of the eyelid; in the chronic, there is much. The acute is more frequent in old, and the chronic in young subjects. The acute is oftener met with in healthy, and the chronic in scrofulous subjects. The acute affects chiefly the integuments; but the chronic involves the whole substance of the eyelid, the conjunctival surface as much as the cutaneous, or fully more; while the cartilage is thickened, shortened transversely, and curved inwards. But there is no rule without exceptions. In Gavine, a healthy man, past the prime of life, we have chronic entropium; while in James Bell (No. 8605), a boy of 14, we had lately an example of the acute.

Acute entropium occurs in cases of ophthalmia, where the eyelids have been kept long shut, and pressed upon by the application of a poultice, by a bandage, or by the hands of the patient. This last was the case in Bell, who was labouring under a severe injury of the eyeball, and constantly kept pressing his hand against his eyelids. After the operation of extraction, we have repeatedly seen the lower eyelid affected with acute entropium. It sometimes occurs as an attendant on catarrho-rheumatic ophthalmia, in old flabby subjects; while, in other cases, without any considerable ophthalmia, it

seems to arise from the subject having suddenly become lean, in consequence of acute disease affecting the system.

It is generally the lower eyelid which suffers acute entropium. Its edge is regular, but is rolled inwards, so that the eyelashes are out of sight. A little traction with the finger rolls it out, and for a few minutes it will perhaps remain in its natural position, till it jerks again into the state of inversion.

In old people there is always a superabundant state of the skin of the inverted eyelid; and to this, with an imperfect action of the orbicularis palpebrarum, the disease appears to owe its continuance.

The conjunctiva is inflamed, the eyelashes rub against the eyeball, the patient keeps the eye shut, and squeezes the eyelids together. If the eye is opened it waters much, and the cornea is observed to be inflamed and nebulous, and sometimes ulcerated.

Chronic entropium is generally the result of ophthalmia tarsi, or of chronic catarrhal ophthalmia. The upper eyelid is as often affected as the lower, and often both are inverted at once. The edge of the inverted eyelid is thickened, irregular, and often notched. The eyelid is shortened from canthus to canthus, and presses on the eyeball. The cartilage is indurated, and bent inwards. The conjunctiva, lining the affected lid, frequently presents the appearance of cicatrices and callosities, and is sometimes quite dry and cuticular—a state which is termed *xeroma*. The cilia are few and short; the cornea vascular and opaque; the conjunctival covering of the cornea greatly thickened; the eye has little or no vision; and the patient no enjoyment, on account of the constant state of irritation. No traction can bring the eyelid to its natural position; you may drag the eyelid from the eyeball, and bring the cilia into view, but the edge of the eyelid is still bent inwards.

In both varieties of entropium you must endeavour to remove the inflammation, upon which the displacement of the eyelid perhaps entirely depends, or which, on the other hand, is aggravated and kept up by the inversion. You saw in Bell, that the entropium was removed chiefly by guarding against the causes which had produced it, and by dropping in diluted vinum opii once a-day, on the inside of the eyelid. We sometimes meet with cases of severe catarrho-rheumatic ophthalmia, accompanied with acute inversion of the lower eyelid, in which dangerous ulceration of the cornea arises from the state of the eyelid; and in such cases we must both instaantly remedy the misplacement by an operation, and bleed the patient for the ophthalmia.

In acute entropium, the operative means which is found completely successful, is the simple removal of the transverse fold of the integuments from the inverted eyelid, and bringing the edges of the wound together with two or three stitches.

In chronic entropium this operation is of no avail. You may clip away piece of skin after piece of skin, but you do no good. Acute inversion is curable by an operation on the skin; but not chronic. In the latter you must attack the tarsus, and the whole thickness of the eyelid.

Saunders cut out the tarsus, or cartilage of the upper eye-lid altogether, along with the roots of the cilia. Others have amputated the edge of the eye-lid, or extirpated the bulbs of the cilia, operations which may be proper for trichiasis or distichiasis, but which do not counteract the transverse shortening of the eye-lid, and but imperfectly affect the inverted state of its edge, in chronic entropium.

In cases of this sort, Ware made a perpendicular incision through the eye-lid at its temporal extremity, or in its middle. This relieved the pressure, and allowed the cornea to clear; but it did not completely answer the intended object, and it left the eye-lid with a fissure like a hare-lip.

Crampton, taking the hint, I presume, from Ware's practice, proposed the operation which is now generally adopted, and which you saw performed two days ago on Gavine. It consists in two perpendicular incisions through the whole substance of the eye-lid, each about half an inch long; the one at the nasal, and the other at the temporal extremity. As soon as they are completed the eye feels unconfined, and a great part of the patient's uneasiness is relieved. Were the lid left to itself the incisions would heal, and matters would return to nearly their former state. To counteract the tendency to inversion, we remove a transverse fold of skin, as in acute entropium; and by means of the ligatures employed to bring together the edges of the transverse wound, we keep the eye-lid elevated, and its internal surface exposed, for eight or ten days. If these ligatures cut their way out, others must be inserted till the object is accomplished. The perpendicular incisions heal slowly by granulation; and to prevent them from healing too rapidly, they are to be touched occasionally with blue stone. In the meantime the cartilage and other textures gradually lose their induration and irregularity; the eye-lid, when reunited, is nearly natural in position; and the cornea having cleared, vision is restored.

QUACK ADVERTISEMENTS.

To the Editor of the Medical Gazette.

Sir,

Having noticed, during several weeks past, in your journal, the advertisement of a book, entitled "Van Buchell on Fistulæ, Piles, &c. without cutting," I was at length induced to procure it, thinking to obtain some new practical information respecting the diseases of which it professes to treat. Instead, however, of affording any instruction, this book turns out to be one of the most impudent ever attempted to be palmed upon the public under the garb of science. It consists of a few common-place observations on the diseases in question, and a series of what are termed "Cases"—a collection of puffing letters, in attestation of the author's successful employment of a *secret* remedy for their relief.

Now, sir, as an humble member of a noble and dignified profession, and as a well-wisher to the best interests of your valuable journal, permit me to say, that I would desire to see its pages—aye, even to the very wrapper—pure and unsullied by the most equivocal stain of empiricism. For my own part, sir, I can perceive but little, if any, difference between the act of giving publicity to a quack medicine, or a *quack publication;* and if the former bring discredit, as it undoubtedly must, upon a work whose object is the dissemination of sound and scientific professional knowledge, what other result, may I ask, can the *latter* produce, than the fatal one of degrading the character and principles of such a work in the estimation of its numerous friends?

Trusting that this may not be your case, I remain, sir,

One of your Oldest and Staunchest Supporters.

August 16, 1838.

[The advertising department is in no degree under our control.—Ed. Gaz.]

MR. AMESBURY'S SPLINTS.

To the Editor of the Medical Gazette.

Sir,

I beg to state that the splint represented by Mr. Hancock in the last number of your useful publication, is not that of Mr. Amesbury, nor is any such splint employed in the treatment of fractures of the thigh in this hospital.—I am, sir,

Your obedient servant,

Alfred Keyser,
House-Surgeon.

University College Hospital,
Aug. 27, 1838.

DECEASE OF M. BARRUEL.

M. Barruel, the eminent chemist, who was at the head of the chemical manipulations of the School of Medicine at Paris, died on Friday last, the 17th instant.

M. LECANU.

The distinguished chemist M. Lecanu has been elected a member of the Academy of Medicine, at Paris.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, August 16.

Samuel King Russell, Shrewsbury.—John Morgan, Pembrokeshire.—Frank Bampfylde Tuttiett, Newport, Isle of Wight.—James Jones.—John Ballard Pitt, Worcester.

Thursday, August 23*rd.*

John Baker—Wm. Stephenson Clark, York.

WEEKLY ACCOUNT OF BURIALS.

From Bills of Mortality, *Aug.* 28, 1838.

Age and Debility	35	Heart, diseased	2
Apoplexy	4	Hooping Cough	2
Asthma	4	Inflammation	20
Childbirth	4	Bowels & Stomach	4
Consumption	48	Brain	8
Convulsions	33	Liver, diseased	2
Croup	3	Measles	1
Dentition or Teething	8	Paralysis	4
Diarrhœa	3	Small-pox	21
Dropsy	9	Sore Throat and Quinsey	1
Dropsy in the Brain	4	Thrush	2
Erysipelas	2	Tumor	1
Fever	19	Unknown Causes	65
Fever, Scarlet	6		
Fever, Typhus	4	Casualties	8
Gout	1		

Increase of Burials, as compared with the preceding week . . . 111

METEOROLOGICAL JOURNAL.

August.	Thermometer.	Barometer.
Thursday . 23	from 51 to 64	29·37 to 29·64
Friday . . 24	49 65	29·85 29 97
Saturday . 25	39 61	29·98 29 94
Sunday . . 26	46 70	29 98 30·01
Monday . . 27	57 77	30·01 30·04
Tuesday . . 28	58 75	30·00 29·81
Wednesday 29	57 64	29 70 30 01

Wind, S.W. and W. by N.

Except the 23rd, 25th, and 28th, generally clear; rain fell on the 23rd, and two following days.

A brilliant meteor in the North passed just below Cassiopeia's chair on the evening of the 27th.

Rain fallen, ·2125 of an inch.

Charles Henry Adams.

NOTICE.

Mr. E. received. Types distributed.

Erratum.—At p. 808, *for* "Ol. Ricini, ℥iij.," *read* "Ol. Ricini, ℥iij."

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 8, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXVIII.

Structural Diseases of the Heart.—Hypertrophy and Dilatation; Causes.—Anatomical Characters of Hypertrophy.—Varieties and Combinations.—Pathological Effects.—Its Relation to other Lesions.—Symptoms of Hypertrophy.—Physical Signs.—Variations of the Signs.—Dilatation of the Heart; Causes.—Anatomical Characters.—Pathological Effects and Symptoms.—Physical Signs.—Dilatation of the Auricles.

Our next subject is the diseases of the *structure* of the heart. The shortness of time which we have left will prevent me from entering into much detail on this subject, which I regret the less since it has been treated by recent authors more fully than the functional affections. You will find, moreover, that many of the descriptions already given of functional disorder will apply also to cases of organic disease, for I have purposely avoided attempting complete distinctions between these, until they should appear from a rational examination of the nature of the two classes. When we speak of the symptoms of organic disease of the heart, we include the phenomena of its function, disordered in consequence of an injured mechanism or an altered tissue: these, as far as regards their physiological relations, may in great part be also produced by other causes less permanent than those of diseased structure; and it is only in case of their great extent or permanency that they can be judged to indicate organic disease. You will not be surprised, therefore, if I do not dwell much on the general symptoms of organic diseases of the heart; they are those of the several functional disorders that I have already noticed—increased, irregular, or diminished action, with variously modified sensibility—in addition to a few others, and to physical signs, which depend more directly and essentially on the particular lesions of the mechanism of the organ.

I shall occupy your attention, first, with diseases affecting the muscular walls of the heart; and afterwards with those of the valves and the great vessels.

The *muscular substance* of the heart may become variously altered in quantity, form, and quality; and its several changes may in different degrees be traced to causes affecting either the motory functions of the organ or its nutrition, or (as is more commonly the case) both together. The most common and familiar of these affections, hypertrophy and dilatation, opposite as they seem, often arise from similar causes, but operating on a different condition and quality of tissue. When from any cause, whether the quantity or quality of the blood or the irritability of the fibres, the heart is excited to excessive action for a considerable length of time, and the nutrient function of its vessels is not impaired by a general cachectic or chlorotic state, like any other highly exercised muscle it increases in substance, it becomes over-nourished or *hypertrophied*: and there is, in the case of the heart, an additional reason why increased action should eventually lead to hypertrophy—namely, the direct relation of its nutrition

to its own action. When speaking of inordinate action, I had occasion to observe that the great force of the pulsations is commonly expended on the first parts of the arterial tube; so that whilst the throbbing is strong in the aorta and immediate branches, the pulse in the radial and other distant arteries is often uncommonly weak. Now the coronary arteries, which supply the heart, are the first to profit by this partial force; and whilst they are furnishing the tissues of the heart with the nutrient fluid, in force and abundance greater than usual, distant parts may be languishing for lack of a due supply. Thus increased action may extend from function to structure, and continued excitement becomes perpetuated by augmented substance and strength. But if the blood be defective in its nutrient quality, in respect to muscular fibre, as it is in various cachectic conditions of the body, or if the strength of the heart itself be unequal to propel the mass of blood on which it endeavours to contract, excited action will not be backed by increased nourishment; the contractions, although still quick and abrupt, will have no extent of force, and the fibres, unable to expel the mass of blood to the usual amount, will become permanently extended — spread abroad: the walls will be thus *dilated*, and the cavities enlarged. There are other causes of dilatation, which we shall consider presently. Again, these conditions may be combined: as when the nutrition of the heart is augmented with the increased action, but still its force is inadequate to its task, and the walls suffer distension at the same time that they are over-nourished. This brief sketch of the pathological causes of hypertrophy and dilatation will render intelligible to you the circumstances in which they occur. The exciting causes of hypertrophy are habitual muscular exertion of an extreme kind, especially during growth, various diseases of the heart, both functional, increasing its irritability, and structural, exciting its action: these include rheumatism and other inflammatory and febrile diseases; various circumstances impeding the circulation and taxing the heart with increased labour—such as obesity, tumors, tense pulmonary emphysema, which compress the great vessels, and so forth.

There are a good many anatomical varieties of *hypertrophy* of the heart. It may be general or partial, accompanied by enlargement, or by diminution of the cavities. The size of the natural heart was stated, by Laennec, to be generally about that of the closed fist of the subject. This standard of comparison is more convenient than accurate; but unless we come to weights and measures, not of the heart only, but of the body in relation to it, we cannot readily get a better. This is a fit subject for the application of the numerical method by those who have time and opportunities for making it; and I have no doubt that interesting results might be obtained from it *. In the meantime we must be content with ocular and manual measurement — the "rule of eye and thumb," or rather of *fist* (I do not mean *pugilistic government*). You should, however, accustom yourselves to observe the position and dimensions of the heart, and the relative proportions of its walls and cavities, in every subject that you see opened, and you will soon get a tolerable notion of their healthy average in relation to the body. If you are inclined to be more accurate, I may refer you to the work of M. Bouillaud, who has given the measures and weights of a great many hearts. The following are about the averages which he gives of the chief measurements: — Circumference around the base of the ventricles, $8\frac{3}{4}$ inches; length, $3\frac{5}{8}$ inches; breadth, $2\frac{5}{8}$ inches; thickness of walls of left ventricle, $\frac{1}{2}$ inch; of right, $\frac{1}{4}$ inch; of the septum, nearly an inch, (this I think too much by $\frac{1}{4}$th); weight, between 9 and 10 ounces.

I believe these are pretty accurate measurements for adults, but they must be taken only as averages, subject to fluctuations somewhat above them and below them in varieties of size and age.

In cases of hypertrophy the heart may present various forms and degrees of enlargement, even to three times its natural size and weight. Lobstein has described an enlarged heart which weighed 32 ounces. Where the enlargement is considerable, there is always dilatation with the thickening of the walls. Here I would warn you against the common mistake of using the word hypertrophy as synonymous with thickening. There may be hypertrophy to a great extent without thickening; as where the cavities are much enlarged, and the walls therefore extended. The word hypertrophy means increase of substance, and may be very well applied to the whole heart or to an integral part of it, but not to express the increase in thickness, which is in one direction only. For the same reason I am not well satisfied with the names given by M. Bertin to

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* Since these lectures were delivered, two valuable contributions to this department have been made by M. Bizot (Mém. de la Soc. Méd. d'Observation) and Dr. Clendinning (MEDICAL GAZETTE for June 9th, *et seq.*) The researches of these careful observers have established, by numerical proof, several notions to which I had been led by general observation, and which are noticed in the lectures. There are other interesting deductions which arise from these researches, which I may have occasion to notice elsewhere.

the varieties of hypertrophy, for they fix the attention too much on the increased nourishment, and too little on the distending influence concerned in their modification. In these drawings and plates you see exhibited the varieties of hypertrophy. Here is *simple hypertrophy*, the walls being thickened, but the cavity natural. Here is *hypertrophy with dilatation*, the "excentric hypertrophy" of Bertin. You see by these two examples that the walls may be either thicker or not thicker than natural. It is the active aneurism of Corvisart: I would, for brevity sake, call it *dilated hypertrophy*. Here you have its converse, *hypertrophy with diminution of the cavity*, which Bertin calls "concentric hypertrophy:" it may as well be termed *contracted hypertrophy*, which in some measure expresses the cause of the diminution of the cavity.

These several forms of hypertrophy generally affect chiefly one of the compartments of the heart, and none so frequently as the left ventricle; next the right ventricle, and then the auricles. In the latter. dilatation is almost always combined with the hypertrophy. Dilated hypertrophy is, in fact, the most common form in the ventricles also. It is by no means uncommon to see particular parts of a compartment more enlarged than others. Thus the fleshy pillars of the mitral valve, those of the tricuspid valve, the cross muscular stays and net-work of the interior of the right ventricle, and the musculi pectinati of the right auricle, are frequently developed to an unusual degree. The increase in the pillars of the valves is generally associated with some defect of the valve. I have occasionally seen the thickening of the walls near the base much greater than near the apex; probably from the undue development of the fibres that encircle this part; rarely the converse is the case. In dilated hypertrophy of the ventricles there is a considerable difference in the shape which it assumes in different cases, the most remarkable varieties being those of elongation and lateral enlargement. Hypertrophy of the left ventricle, with elongated dilatation, is most commonly associated with disease of the aorta or its valves, especially those which permit regurgitation. In disease of the mitral valves or orifice the dilatation is usually more lateral or globular; but this form is met with also without valvular disease. In the right ventricle, also, the enlargement is in some cases more in the pulmonary, in others more in the inferior or auricular, portion of the ventricle; but I do not know that I can associate these differences with other particular lesions.

The muscular tissue in hypertrophy is not only increased, but it is often more or less changed. In the more sthenic forms, which are associated with active nutrition and a rich state of the blood, it is generally redder and firmer than usual. In leucophlegmatic or cachectic subjects, on the other hand, the increase of substance is often accompanied with duller colour and a laxity and softness which render the texture flabby, and probably cause its being, as it always is in such cases, dilated. In cases of organic disease of long standing, especially with close adhesion of the pericardium; I have repeatedly seen threads or laminæ of a dirty white tissue among the muscular fibres, many of these fibres having also partially lost their colour, and the others being in a flabby state. In other cases, the enlargement of the heart has been in great measure made up of a soft texture containing much fat, like that often found in the pelvis of the kidney. In rare instances, again, whole portions of the ventricles have been found transformed into a dense fibrous tissue, as you see exhibited in these drawings and in this plate of Dr. Carswell's. It is obvious that such changes must have an effect on the function of the organ, very different from that arising from simple increase of the muscular fibres. Yet it is not unlikely that they may both originate from inordinate action of the organ, and the modified circulation which it causes through its own vessels, with different conditions of the nutrient fluid. We have no time for more on this subject, which is too speculative in its present state. I return to muscular hypertrophy.

The pathological effects of hypertrophy will necessarily vary according to its degree, the part which it affects, and the other lesions with which it may be complicated. The commonest and most important form is that affecting the left ventricle, and manifesting its effects on the general circulation and its functions. If the hypertrophy predominate, and be not counteracted by any valvular defect, there will be an increased strength of the arterial pulse, which will commonly be felt most in the arteries nearest to the heart, but it may extend to the whole arterial system. You can readily understand that the increased force of the arterial current may occasion various disorders of function and structure in the several viscera and tissues of the body. In the first place, it may cause dilatation and other changes in the coats of the arteries, especially those that more immediately feel its force, the ascending portion and arch of the aorta. Then as conducted into various tissues and organs, it may excite and disturb their functions, exalt their sensibility, and especially dispose them to inflammation, serous effusion, and hæmorrhage, or aggravate

any inflammations, irritations, or hæmorrhages, when they occur. Hence apoplexy and phrenitis, epistaxis, ophthalmia, and other inflammatory affections of the parts of the body, have been traced to this form of disease of the heart. In time, the strong pulse accompanying hypertrophy of the left ventricle may cause an increased or modified deposition of nutriment in the different tissues which it reaches, particularly the parenchyma of viscera. The kidneys afford the best illustration of this, because they receive their blood only from the arterial system. In hypertrophy of any standing they are generally found enlarged, and otherwise diseased, and often presenting the granular albuminous deposit which has been described by Dr. Bright. The lungs and the liver are also very commonly increased in substance; but this must be in many instances partly referred to the obstructed venous circulation which so frequently accompanies hypertrophy of the left ventricle; being another consequence of its cause. The modification of nutrition especially affects the vascular system, whence arises thickening or ossification of the coats of the small arteries of the brain; and in the rupture of these there appears another mode in which hypertrophy of the heart may lead to apoplexy.

Hypertrophy of the right ventricle is commonly supposed to be a cause of congestions, inflammation, and hæmorrhage, in the pulmonary tissues; but as these effects are seldom observed where there is not also some cause of obstruction to the onward flow of blood through the left side of the heart, it is uncertain what share the hypertrophied right ventricle may have in producing them. In fact, if you remember that its auricular valve is not formed to close completely on an accumulating mass of blood, but permits regurgitation when the ventricle is distended, you can conceive that the pulmonary textures are not likely to suffer from its pulsations, unless their vessels are first distended by some other obstructing cause in or beyond them. In that case the force of the right ventricle must be felt; and when it becomes so much distended as to open its auricular valves, this force is then in part expended backwards into the auricle and veins, causing the venous swelling and pulsation often conspicuous in the jugulars. This venous obstruction, when considerable or permanent, leads to other effects, such as congestions in the portal system and in the sinuses of the brain; whence arise hepatic and gastric disorders, headaches, cerebral oppression, and apoplexy, and especially dropsical effusions of various kinds. These backward effects are the more likely to ensue when the right ventricle is much dilated as well as hypertrophied; whereas the effects on the pulmonary textures, congestion, hæmorrhage, hypertrophy, and excessive bronchial secretion, depend rather on the increased strength, without much enlargement of the ventricle.

When the auriculo-ventricular orifice is contracted, we occasionally find dilated hypertrophy of the right ventricle combined with contracted hypertrophy of the left. It has puzzled pathologists to account for this hypertrophy of the left ventricle, when its task must be diminished by its receiving less blood to propel from the left auricle. It has been attempted to explain it by the left ventricle feeling the obstacle at its own auricular orifice through the whole course of the arteries, capillaries, veins, the right side of the heart, and the pulmonary vessels; a round-about explanation truly, and one that supposes a degree of distension of all these parts that is very rarely seen. Then Dr. Copland urges this case as an argument for his favourite notion of an active expansion of the ventricles, which I cannot admit to be warranted by any physiological analogies. But how very simple is the true cause of this hypertrophy, if I understand it aright! Why should not the excitement of the whole heart dependent on the distension of all its other cavities produce increased action, and eventually increased growth of the left ventricle also? Is it possible that the same fibres which encircle both ventricles can be excited in one and not in the other? No, surely; the left ventricle, naturally the strongest and most active, is thus excited by sympathy or continuity of irritation; and when its walls become increased under this influence, the cavity must be contracted from the smallness of its contents.

I cannot tell you of any remarkable pathological consequence of hypertrophy or dilatation of the auricles: they are to be considered as effects more than as causes. The strength of their contraction is too trifling to produce any other effect than that of stimulating more powerfully their ventricles, which are apparently excited to contraction by their impulse. This influence is perhaps reciprocal in the case of the right auricle and ventricle; and the consecutive pulsations of the two propagated backwards is sometimes seen in a remarkable double pulse in the jugular veins. The dilatation of the right auricle is sometimes enormous. I have seen it large enough to contain an ordinary-sized heart.

From the general pathological effects of hypertrophy of the heart you may infer the prevailing character of its symptoms. A forcible, at the same time often not a

very effective, state of the circulation, with its phenomena, palpitation and throbbing, occasional flushing, and a strong hard pulse, manifesting its effects on particular organs, as by giddiness, palsy, or apoplexy in the brain; dyspnœa, asthma, with thin mucous secretion, or occasionally hæmorrhage in the pulmonary tissues; gastric dyspepsia, gastrodynia, sickness, discoloured alvine secretions, and other symptoms of disorder in the stomach, liver, and bowels; albuminous, high-coloured, and often scanty urine, with accompanying gouty or dropsical symptoms, referable to disordered functions of the kidneys; more or fewer of these may be variously grouped together in different cases, as frequent effects or symptoms of the more sthenic forms of hypertrophy of the heart. Some of these secondary effects may occur and be the subjects of complaint, when those of the immediate action of the organ, the cause of them all, are not sufficiently marked to attract attention. When dilatation is joined with it, there will be more weakness of the circulation, with consequent torpidity of the functions of the extreme vessels, manifesting itself in venous or visceral congestions, œdema, and coldness of the extremities, pains in the back and limbs, and the incapacity of the organ, will be greater; whence a pulse, unequal in force and often irregular in rhythm, severe attacks of palpitation and dyspnœa, may then present themselves, the disease is more manifestly one of the heart itself.

The physical signs of hypertrophy are intelligible and characteristic. The increased mass of the muscular fibres renders their act of contraction stronger than usual; hence the impulse is also strong. But the character of this impulse, as well as the sound which attends the contractions, will depend on the form which the hypertrophy has assumed. When it is a simple thickening of the walls without increase or diminution of the cavity, the impulse will be gradual and heaving as well as strong, both because thick muscles cannot contract so simply or abruptly as those that are thin, and because the enlarged size of the heart brings more of it in successive contact with the ribs. So, likewise, the first or systolic sound will be prolonged, but duller than usual, because the sounding transition of thick walls, from loose to tight, is less extensive, abrupt, and instantaneous, than when they are thin. I formerly illustrated this by the different sounds produced by suddenly stretching fabrics of different thickness, such as linen and sackcloth, or thin stuff and thick baize. With contracted hypertrophy, in which the cavity is smaller, and the walls therefore have less extent of motion, the diminution and dulness of the first sound in the region of the heart is still more remarkable; it may be quite destitute of the flap usually heard, whilst the impulse is remarkably heaving, as if the heart swelled against the ribs at each contraction. It often happens that the sound is louder at the top of the sternum and in the carotids, than in the region of the heart. The second sound, as heard at the mid-sternum, may be as loud as usual. When, on the other hand, the hypertrophy is dilated, there are better conditions for generating sound; the walls and valves being loose and flabby, pass with greater abruptness into the tense state of contraction, and yield a louder sound; whilst the impulse, although strong, is more abrupt, and in cases of extensive disease is followed by a motion of collapse, from the sudden falling back of the large heart into a state of passive loosenesss at the moment of diastole. Both sound and impulse will then be extended over a wider space than is natural; and when the disease is considerable, and unattended with an emphysematous state of the lung, there will be also dulness on percussion, more or less extensive according as the enlarged heart is in contact with the thoracic walls. The situation of this dulness, and of the impulse, will vary according to the form of the dilated hypertrophy and other circumstances which affect the position of the organ. In elongated enlargement the impulse is generally felt below its usual spot, between the fifth and sixth ribs, down to the seventh or eighth; and I have even felt it in the abdomen, below the margins of the ribs. The dulness reaches from that part upwards to the sternum. But constant, or even occasional distension of the abdomen, by any cause, which prevents the descent of the diaphragm, will make the enlarged heart take another position, by which its apex extends further to the left, and the dulness on percussion reaches from that point to the sternum, occupying the whole intra-mammary, and perhaps part of the mammary region. The same position of the heart, thus more horizonal than natural, may, perhaps, also be caused, as Dr. Hope has pointed out, by adhesions of the pericardium, which prevent the organ from enlarging downwards. Enlargement of the liver, distension of the stomach or colon, and dropsical effusions in the abdomen, are the most frequent causes of this lateral direction which enlargement of the heart often takes. Similar causes may also determine the displacement to be outwards, against the thoracic walls, occasioning them to project to the left of the sternum, in the

manner described by Bouillaud. This is most remarkable in children, and in young persons with narrow chests. When the enlargement is more transverse or globular, the dulness on percussion, and the impulse, are higher, independently of the position of the diaphragm; and this is particularly the case when the right ventricle is also enlarged.

I have known the dulness on percussion and impulse to reach over two-thirds of the left front of the chest, and as high as the third rib. You can understand why, under these circumstances, the impulse should be of a different kind as well as more diffused than usual, being produced by the swelling *body* of the ventricle rather than by the striking apex, and is therefore also felt higher instead of lower in the chest.

There are many other varieties, of which I would rather give you the principles than the details, for our time is very limited, and the circumstances which modify physical diagnosis are too many ever to be known by those who do not learn it fundamentally. I have told you that naturally the left ventricle touches the walls of the chest only at the apex, and that the impulse felt on full expiration at the sternum is that of the right ventricle. But if the left ventricle be much enlarged, and the right not, its impulse may be felt as far as the sternum, the impulse and sounds of the right ventricle being at the sternum only, or transferred to the right of it and the epigastrium. Again, if the right ventricle be also enlarged, it will keep the left ventricle to the left, and from the walls of the chest, so that its stronger impulse may be but little felt, nearly the whole front being occupied by the dilated right ventricle. Hence the reason why much enlarged hearts are often not felt beating with the force that might be expected from the thickness of the walls of their left ventricle; hence, too, the rules given by Laennec and others, to find the signs of each ventricle at particular spots, fail in many cases. The position of the whole heart, and of its compartments, is changeable, and when altered by disease it may be so different as to be entirely shifted from the common *landmarks*. It is rather by relative than by absolute rules that we can rightly judge here. For instance, if an hypertrophied left ventricle encroaches on the region of the right, we may know still that it is the left, by finding the different kind of sound and impulse to the right of it, and no other kind to the left. When the heart is displaced to the left, the signs of the right ventricle are those in the lower as well as right part of the space in which the sounds and impulse are felt most, those of the left ventricle being above and to the left.

To a certain extent you may also judge the strength of the left ventricle by the loudness of the sounds and the strength of the pulse at the top of the sternum and in the carotid arteries, when they are not exaggerated by dilatation of the aorta. Hypertrophy of the right ventricle could not cause these signs, whatever increase of sound or impulse it may produce in the region of the heart itself. Again, when the pulse is strong, and the sounds loud at the top of the sternum and in the carotids, although there be no strong impulse to the left of the sternum, there is probably hypertrophy of the left ventricle, but it is displaced from the walls of the chest by a dilated right ventricle, expends its force on the more yielding tissue of the lung, and in slender persons may be actually felt in the left back. You must perceive, on the other hand, how tumours, or a consolidated lung, which push forward the heart or prevent its receding from the walls into the spongy lung, must greatly exaggerate the impulse of the heart, and may give the impression of extensive hypertrophy where little or none exists. I have seen such effects from aneurism of the descending aorta, from tuberculous or inflammatory consolidation, and tense emphysema of the inferior lobe of the lung, and from extensive effusion in the right pleura. For analogous reasons hypertrophy of the heart is more perceptible and distressing in persons with narrow or distorted chests than in others. Many individuals with broad deep chests have large hearts that give them little or no trouble, but would cause distressing symptoms in those of a slender make. Such persons, if they gradually grow fat, may still retain much activity and strength. It is an advantage to them to have a heart strong enough for their increased bulk, with room enough for it to work in; their balance is preserved. But should their bulk be suddenly reduced, the heart may then be too strong for the smaller frame, and may evince symptoms of its disproportionate strength. When, instead of increase of the muscular tissue, the heart is enlarged by the deposition of fat around it, the sound is obtuse as in hypertrophy; but the arterial pulse and cardiac impulse are weakened also, and the dulness is more at the sternum, or middle portion of the chest.

Dilatation of the heart is in many respects the converse of hypertrophy. It stands in the same relation to diminished force of action, as the latter affection does to increased action; and although sometimes induced by similar causes of obstruction to the passage of blood from the

cavities, yet there must be in this case an opposite condition of the muscular tissue, a weakness and disposition to yield, instead of the tendency to react on, and struggle against, the obstacle which leads to hypertrophy. Where this reactive tendency is still unable to overcome the obstacle, or where the cohesion of the tissue gradually yields in the struggle, the two conditions, hypertrophy and dilatation, are at the same time produced. A softening or diminished cohesion of the tissue, may in itself be a sufficient cause of dilatation. To be brief, the causation of dilatation may be summed up in this: when the heart is incapable of sufficiently expelling its contents, whether in consequence of obstruction in the vessels from it—of regurgitation into it through imperfect valves—of want of power, of irritability, or of both, it becomes distended, and in time permanently dilated. The circumstances which determine these conditions, that is, the occasional or exciting causes of dilatation, are weakening influences in general, such as those of long-continued fevers, cachectic states of the system, with bad blood; and causes which weaken the heart in particular, such as continued inflammation, obstruction of the coronary vessels, and such circumstances which injure the nutrition of the organ.

The anatomical characters of dilatation are not only the thinning and extension of the walls of the affected compartment, but also generally a paler and more flaccid condition of their muscular fibres. In some parts, particularly of the auricles, and at the apex of the left ventricle, the attenuation of the walls has sometimes proceeded so far, that the pericardium and endocardium are in contact, and these are occasionally thickened by opaque deposit, as if to strengthen them at these parts. The right ventricle and the left auricle are the most common subjects of simple dilatation. In the other compartments it is occasionally met with, but more generally combined with some degree of hypertrophy; so that, although the walls be thinner than natural, their greater extent gives them an increase of substance. The right auricle and ventricle are sometimes dilated to an enormous extent, with thinning of their walls, but still increase of substance; and this condition is generally found to be associated with disease of the mitral valve. Dilatation of the ventricles is commonly in all directions, rendering the cavities globular; but it is occasionally partial, the walls being distended into a pouch or aneurism, which in rare instances attains a considerable size, and may end in rupture*. Some of these cases are curious, but we have no time to spare for them. With the dilated condition of the walls there may appear various other traces of disease in the lining membrane and valves; such as opacity, thickening, and roughness. They are most seen near the valves and orifices, and are evidently the result of an inflammatory process, which may have been either the cause or the effect of the dilatation. The orifices and their valves are commonly somewhat dilated, as well as the other walls, so that they may still maintain a sufficient proportion to perform their office. The dilatation of the semilunar valves is sometimes considerable, and renders them so thin that the fibrous threads can be seen forming an irregular network in them. Their thinning sometimes amounts to perforation, especially at the margins which apply to each other, and then this fibrous network may be the only part left. The tricuspid valve is seldom expanded in proportion to its orifice, which almost always partakes of the dilatation of its ventricle; hence there is free regurgitation through this orifice.

The pathological effects and symptoms of a dilated heart are such as we have already described as those of irregular and defective action. Having here become an effect of permanent change of structure, they are necessarily more enduring than where they are merely functional; and there are added to them others, arising from this more permanent influence on the other structures and functions of the body. When the left ventricle is dilated, its propulsive power is impaired; there may be faintings, alternated with palpitation and other irregularity; the whole systemic circulation is languid, and causes the symptoms of muscular debility, coldness of the extremities, defective and disordered secretions, imperfect nutrition, and eventually œdema of distant parts. When the change is chiefly in the right ventricle, the symptoms are those of impeded venous circulation, lividity of parts naturally red; anasarca, often not confined to the lower extremities, perhaps with ascites and general congestion of the abdominal viscera. To these are often added the symptoms of pulmonary congestion, with its distressing concomitants, asthma, cough, profuse expectoration, and hæmoptysis, induced by the same cause which led to the dilatation of the right ventricle—disease in the mitral orifice. The complication of dilatation with valvular disease is particularly unfavourable, for it consti-

* A very complete memoir on Partial Dilatations and Aneurisms of the Heart, by my friend, Mr. Thurnam, has lately appeared in the Medico-Chirurgical Transactions.

tutes a double defect in the power of the organ, whilst its irritability may continually excite it to fruitless and most harassing efforts of palpitation, which may terminate life by ending in syncope, or wear it out in a more gradual manner.

The physical signs of dilatation were well described by Laennec; they are the converse of those of hypertrophy, and are equally intelligible and characteristic. Extended into a thin layer of muscle, the contractions of the walls are simple, abrupt, and quick; and thus, although they have little power, they are well calculated to produce a loud sound and slight brisk impulse; and these are diffused over a wide space, proportioned to the extent of the dilatation and its proximity to the walls of the chest; but the sound can have little duration, and the impulse little strength; and even under the influence of palpitation do the pulsations not raise the walls, as an hypertrophied or even a healthy heart does when excited. The first sound of a dilated heart so closely resembles the second, that it is not easy to distinguish between them without placing the hand at the same time on the region of the heart, or on one of the carotids, to find which sound corresponds with the pulse. I should express this sound by the word *lup-tup*, instead of the *lubb-dup* of the healthy heart. The sound on percussion will be also dull in the region of the heart, in some proportion to the extent of the disease; more extensively so than is usual in simple hypertrophy, but less than in dilated hypertrophy, in which the greater size to which the organ attains, and its flaccidity, which exists as in dilatation, brings a larger surface in apposition to the walls of the chest. To a certain degree we may, by the position of these signs, determine which compartment of the heart is most dilated, those of the left ventricle being, as Laennec pointed out, situated between the cartilages of the fifth and seventh ribs, and those of the right at the lower half of the sternum, and in the epigastrium. A dilated right ventricle, however, sometimes extends far to the left of the sternum, carrying the left ventricle behind and above it away from the walls of the chest, which, as in dilated hypertrophy, may prevent the impulse and sounds of the latter from being readily distinguished from those of the right. Under these circumstances a complete expiration, and leaning forwards to the left, will sometimes bring the heart into more complete contact with the walls of the chest, and give the stronger impulse and duller sound somewhere under the left breast. On the other hand, when the left ventricle is dilated, the impulse is felt lower than usual; but it does not displace materially the position of the right ventricle, which may be heard and felt at the sternum. In consequence of their greater loudness, and the large size of the body in which they are produced, the sounds are heard over a greater extent of the chest than usual; but I cannot agree with Laennec that this extent is so perceptibly proportioned to the degree of the dilatation as to be an exact measure of it. The transmission of the sound through the lungs may be so much modified by even temporary conditions of the lungs (and much more so by consolidation, hypertrophy, or emphysema), that the distinctness of the sounds in different parts of the chest depends as much on these organs as on the intensity of the sounds themselves.

Extreme dilatation of the auricle, particularly the right, may cause dulness on percussion at the middle of the sternum, and on either side of it; in fact, it only adds to the extent of the dulness produced by the enlargement of the ventricle which is conjoined with it. Laennec taught that dilatation of the auricles increased the loudness of the second sound; and although his view of the cause of this is totally erroneous, I think I can from experience confirm the fact, that the second sound has been sometimes unusually distinct at the middle of the sternum and on either side of it, in cases in which examination after death shewed one or both of the auricles to be dilated. The second sound is, you know, produced by the flapping tension of both sets of semilunar valves; and although the dilated auricles which lie close to these valves do not increase the sound, they transmit it to the front wall of the chest better than the spongy tissue of the lung, which generally intervenes, could do. I once saw a double pulsation between the third and fourth right ribs, close to the sternum, which I was led to ascribe to a greatly dilated right auricle. The same double pulse was in the jugular veins; and I believe, with Laennec, very commonly attends extreme dilatation of the two right compartments of the heart, there being, in such a case, pretty free regurgitation through the auriculo-ventricular orifice.

In regard to the constitutional effects and symptoms of dilatation of the heart, I need say little in addition to what you have already heard of its origin and pathological tendencies. It is at once the effect and the cause of weakness in the compartments which it affects, and it extends the effects of this weakness by imperfect circulation of blood in the parts to which it supplies, or from which it receives, this fluid.

LECTURES ON BLOOD-LETTING;

Delivered from time to time,
At the General Dispensary, Aldersgate Street.

By Henry Clutterbuck, M.D.
Formerly one of the Physicians to that Institution.

Lecture XIII.

Of the use of Blood letting in Fever in General.

The term fever (according to the vulgar acceptation of the word, which, by the by, is really the most correct) is applied to all cases in which the heat of the body is morbidly increased. Such a state of system is, in most cases, obviously preceded and accompanied by some topical inflammation as its cause; and being, therefore, not a primary but a secondary affection, it has been called symptomatic fever, as being only a symptom of inflammation. A very general opinion, however, has prevailed among physicians, that a febrile state of system may exist as a primary disease, independently of any topical inflammation: and this they have called *idiopathic* or proper fever, to distinguish it from the symptomatic. This opinion appears to me to be without foundation. According to my observation, there is no febrile state of body that is not traceable to inflammation as its source: consequently, the term *idiopathic* fever, as explained above, is improper. If this position be true, it must have no small influence on practice; for, as a general rule, it is of more importance to attend to the primary disease than to any merely secondary symptoms; for, by removing the cause, we are most likely to remove the effect at the same time. If, for example, we succeed in taking off inflammation in any part, the febrile disorder, the symptomatic fever, produced by it, will immediately cease: whereas, by proceeding in the reverse way, we may remove the secondary or symptomatic affection, but the primary will not always or necessarily give way.

I have, on a former occasion, given you my reasons for believing that what is (incorrectly) called idiopathic fever, is always, and essentially, a topical affection of the brain; and that it consists in inflammation of this organ, as its immediate or proximate cause. The general disorder of the system observable in these cases, though often the most conspicuous, and exciting the chief attention, is in reality a secondary state; as much so as in any other inflammation. This conclusion, you will find, is warranted by an attentive investigation of the symptoms, both local and general; all of which (those, I mean, that are pathognomonic, and essential to the disease,) refer themselves to the brain as its seat, and to inflammation as the cause.

It is not very difficult to understand why the local nature of fever should have been overlooked, and the disease considered in the light of a general affection, when we advert to the great—it might be said, indeed, the universal influence, which the brain altogether exercises over every organ of the body; by which it is capable of producing a state of general disorder throughout the system. In this respect the brain differs widely from all other organs. Amidst this general disorder, however, the affection of the brain is always discernible, and that from the very commencement. The sensorial or proper functions of the brain,—sensation, voluntary motion, and mind,—are all more or less disturbed in fever, and that always in proportion to the violence and danger of the disease. Heightened sensibility in all the organs of sense, with an impaired state of the voluntary and mental powers, in the beginning, followed by disorder, and at length annihilation of all the sensorial functions,—these are the never-failing results of a continued and protracted course of the most simple form of fever, when so severe as to endanger life. And when to this are added the local pain and heat, the throbbing of arteries both in and about the head, together with that constitutional disorder (pyrexia, or a febrile state,) which inflammation, and inflammation alone, seems capable of producing—these, altogether, furnish evidence which it seems to me difficult to resist, both of the seat and nature of the disease. Pain in the affected part, which is so common, though by no means an invariable attendant of other inflammations, is, no doubt, sometimes wanting in fever; and indeed, this symptom seldom bears a just proportion to the real violence and danger of the disease, but rather the reverse. This, however, is readily accounted for; partly by the natural insensibility of the organ, and partly by the want of consciousness which the disease itself is so apt to induce.

But the seat of fever is not only shewn by the symptoms during life, but by the consequences which follow, and which are referrible solely to the brain. Loss of one or more of the external senses, paralysis of muscles, imbecility or derangement of the intellectual power, are by no means uncommon sequelæ of the disease. Such occurrences are not met with after other diseases, however severe. In Dr. Abercrombie's interesting little work, On the Intellectual Powers and the Investigation of Truth, several curious instances are adduced of disorder in the mental powers of thought and perception taking

place during and after fever. In one case the patient, during the course of the fever, heard double—every stroke of a clock was heard as if repeated. In another instance—a particular friend of the author, when recovering from typhus fever, the patient's body appeared to himself to be ten feet high, and his bed seven or eight feet from the floor; so that he felt the greatest dread in attempting to get out of it: and the opening of the chimney appeared as large as the arch of a bridge. Other examples are related of false perception arising in the course of fever, and continuing afterwards.

If it be said that dissection does not, as it ought to do, confirm this view of the nature of fever, I answer, that dissection is not at all times competent to the task; and yet it will be found to furnish proofs as strong as it is reasonable to expect from it.

In the first place, the disease, when it proves fatal, kills by violence of action, and by disorder of function, and not by change of structure—such, at least, as dissection is capable of shewing. The worst symptoms of fever may take place without any change of structure in the brain: this, I think, cannot but be admitted, when it is found that such symptoms often cease altogether within the space of a few hours, in cases where the fever terminates favourably, by crisis, or some critical evacuation; the organ then at once resumes its proper functions, though in a debilitated way. This it could hardly do, had it undergone any material change in its organization.

Again, alterations may take place in the structure of an organ so delicate as the brain, that may be quite sufficient to interrupt its functions, and even to destroy life, without being discoverable by dissection. Thus, concussion of the brain has often proved fatal, where no mechanical or physical injury to the organ could be detected after death. The coarse and obvious changes that often present themselves, on post-mortem examinations of the brain, are only to be taken as evidence of pre-existing disease in the part, and not as the immediate cause of the symptoms, or of the destruction of life; for similar appearances are observed in very different forms of disease in this organ; so that it would rarely be in the power of an anatomist, however skilled and experienced he might be, to say, from the appearances on dissection merely, of what particular disease the patient died; nor, on the other hand, would he be able to determine from the symptoms, what appearances would be found upon dissection after death. Indeed, if morbid anatomy were subjected to so rigid a test as this, I fear its incompetency to illustrate the real nature, or even the seat of disease, on numerous occasions, would but too plainly appear. And without abiding such a scrutiny, it can have no claim to unlimited confidence.

Another reason why dissection often fails to illustrate the seat and nature of fever, as seated in the brain, is, that the patient does not always die of the fever itself, but of some supervening disease, remote from the brain itself; such, especially, as abdominal inflammation, which, when it arises during the course of fever, may prove fatal: the fever itself, or brain-affection, subsiding before death, a *post-mortem* examination of the brain in such case, it is plain, would not decide the question.

The inadequacy of dissection, on many occasions, to point out the seat and nature of fever, is so far clearly shewn; and, in addition, it may be stated, that practitioners in general are not very forward to examine patients dying of malignant fevers; and it is chiefly such that prove fatal. The evidence we have on the subject is, therefore, necessarily scanty. But although dissection fails, in numerous instances, to shew the seat or nature of fever, we have the more satisfactory proof derived from symptoms; which, when carefully examined and weighed, can scarcely lead to error.

If the view of the subject here taken be correct—if idiopathic fever, as it is called, be essentially a brain affection, consisting in inflammation, the term *cerebritis* or *encephalitis* would best serve to designate the disease, as denoting at once its seat and nature. While, however, the point is in dispute, it will be as well, perhaps, in order to prevent misapprehension, to retain, for the present, the term fever; and I shall proceed to point out and explain, as far as I am able, the different varieties of the disease, and their treatment, especially as regards the use of blood-letting.

Of the Varieties of Fever.

Fever differs extremely in its characters at different times, so as to have received a variety of denominations accordingly. Admitting the disease to be, as here supposed, an affection of the brain, and consisting in inflammation, there is abundant room for all the varieties ever met with. When we reflect that the disease may occupy the whole organ at once, or any of its parts—that the inflammation may exist in all degrees, and is undergoing continual change—that the part or parts the most highly inflamed may press upon, and either irritate or impede in their functions the surrounding parts—that in consequence of the influence exercised by the brain over distant parts, such as the stomach, heart, &c., these may have their functions disturbed—and, further, that the disease must

be influenced by the constitution and habits of life of the patient—it is plain that a ground is laid for the greatest diversity of character, and an almost endless multiplicity of symptoms, such as no other organ can possibly exhibit. We may hence understand, why one of the organs of sense may suffer more than another—why the stomach is sometimes violently disordered, and not at others—why the action of the heart and vascular system should be in a state of great excitement at one time, and in the opposite state at another—why, also, the muscular power should be so greatly reduced in fever, in comparison with other diseases.

The differences observed in fever are sometimes essential or specific—sometimes accidental, and coming under the head of what naturalists call varieties. But as even the varieties require consideration in practice, it becomes necessary to advert to them, with a view to arrangement. All fevers, then, may be arranged according to the following scheme:—

1. Simple fever.
2. Complicated fever.
3. Specific fever. And, lastly,
4. Periodical fever.

Under one or other of these heads may be included, I believe, every variety of fever that requires to be particularly noticed; and especially as regards the use of blood-letting. Now there are two ways of treating fever in general; the one consists in allowing it to pursue its natural course, relying upon its known tendency, in most cases, to terminate spontaneously in health. According to this plan, we content ourselves with placing the patient in circumstances the most conducive to his recovery—avoiding all causes of aggravation, and palliating particular symptoms, as far as this can be done without disadvantage to the whole disease. This may be termed the palliative treatment—the *médecine expectante* of the French.

The other mode of treating fever is, by the employment of active means to endeavour to interrupt the progress of the disease, so as to bring it to an immediate, or, at least, a speedy termination. This may be termed the *curative* mode of treatment, of which blood-letting constitutes an important part, (supposing the circumstances to be such altogether as to justify its use, which may or may not be the case).

The former, or palliative mode, has been in the most extensive use in all ages, even down to the present time; for the James's, or antimonial powder, the saline draught, and the generality of other placebos (not excluding the homœopathic medicines), that are so assiduously exhibited every four hours, for two or three weeks together, can be considered in no other light—they give time for the disease to wear itself out, as it will do in a great proportion of instances. At present, however, a predilection appears to exist for the active or curative practice, the most powerful remedies being employed for the purpose, and blood-letting among the rest. Now, provided all the circumstances necessary to be considered in order to justify the practice are thoroughly known and judiciously directed, this active mode of treatment would unquestionably deserve a preference. It is, no doubt, a great advantage to reduce the disease to a period of a few days, instead of as many weeks, with a proportionally long period of convalescence. But although this may be effected in many instances, there are others in which it cannot be done, nor even attempted with safety. We have to consider the general character of the disease, and more especially the stage of it; and likewise the individual constitution of the patient.

Fevers differ much in different seasons—a circumstance that particularly attracted the notice of Sydenham; so that he found the disease much more tractable in some seasons than in others. Such peculiarities are only to be detected by careful observation at the time. The stage of the disease is always a matter of the greatest moment in laying down our plan of treatment. The active or curative method, especially the use of blood-letting, is only admissible in the very early stage, as within three or four days from the commencement. At a later period, blood-letting is seldom found capable of arresting the course of the fever, and may even prove injurious. If used at all, at an advanced period of the disease, it should be only to a small extent, and for the purpose of palliation merely. It is a general rule, that the earlier blood-letting is resorted to, it may be administered with the greater freedom; so that where, in the same individual, a pound, or even more, of blood may be safely and properly drawn within a few hours of the attack, and that with a reasonable prospect of bringing the disease to an immediate termination, the same quantity would not only be likely to fail if delayed but for a few days, but would perhaps prove injurious, by weakening the system without a corresponding reduction of the disease. When, therefore, blood-letting, though employed sufficiently early, is found to fail in immediately subduing the disease, or if several days should have elapsed without the use of this remedy, it can hardly be resorted to with propriety, unless it be for the palliation of a particular symptom; and we are then, of necessity, obliged to resort to the passive mode

of treatment. The objection to blood-letting here stated does not apply so strictly to other active means, such as emetics, which are often effectual in arresting the progress of fever in the early stages; and the same may be said of profuse sweating, which was formerly much in use for carrying off fever, as well as other inflammations. A combination of moderate blood-letting, with pretty active and repeated vomiting, and sweating, employed in the order here laid down, will be found exceedingly efficacious for the removal of fever, if resorted to in the first days of the disease. We proceed now to the consideration of the different varieties of fever, as mentioned above; and first, of simple fever.

1. *Of simple fever.*—By this term I mean to designate that form of fever where all the symptoms that are essential to the disease refer themselves to the brain, such as pain in the head, and greater or less disturbance of the sensorial or proper functions of this organ. Under this denomination of simple fever I include the inflammatory fever, so called (the *synocha* of Dr. Cullen)—the low nervous fever of Huxham (the *typhus mitior*)—and the violent or malignant fever (the *typhus gravior* of Cullen.) These are not, properly speaking, different species of the disease, but varieties merely, differing only in degree, and convertible into one another; and, indeed, they often are so converted, either by mismanagement, or from constitutional peculiarity. I retain these terms on account of their long use, while they involve no particular hypothesis. I hardly need observe here, that I totally exclude the definition of simple fever given by the late Dr. Armstrong and others, namely, "fever without any primary local affection," for no such in reality exists.

The inflammatory fever (*synocha* of Dr. Cullen) is characterized by acute headache; pain in the limbs, and over the whole body; a full, strong, and frequent pulse, with great heat of skin, and a whitish fur on the tongue. There is sleeplessness, but no mental disorder, nor any remarkable prostration of muscular strength: in a word, the violence of the disease is confined chiefly to the general vascular system, and is not proportionally great in the sensorium, which is the primary or essential seat of the disease. The violence, therefore, is more apparent than real. This variety of fever takes place chiefly in young and vigorous subjects, and in the winter or spring season. It is commonly induced by exposure to cold, or excess of any kind. Its duration is not long, it often terminating within twenty-four hours (*ephemera*), or at most within a week. Should this, however, not be the case, the general vascular excitement declines, and the disease subsides gradually into the typhoid state, as it is termed, the cerebral functions becoming more and more disordered by the continuance of the inflammation. This was termed *synochus* by Dr. Cullen.

This form of fever is particularly under the influence of blood-letting, provided it be early had recourse to. A single large bleeding, within a day or two of the attack, will generally either put an end to the disease altogether, or so reduce its violence, that it will gradually subside afterwards under the use of the simplest antiphlogistic treatment.

2. The second variety of simple fever which I mentioned is that which Huxham so well describes under the name of *low nervous fever*, and which Dr. Cullen calls *typhus mitior*. It usually sets out mildly, with symptoms similar to those that usher in inflammation in general; that is, slight shivering followed by heat, together with headache, listlessness, and want of sleep; thus indicating the seat of the disease. The pulse is more frequent than natural, but neither full nor strong. The tongue is coated, but moist, and of a yellowish white hue. These symptoms creep on, gradually increasing from day to day. The headache is succeeded by a degree of stupor; delirium appears, and is at first occasional only, but at length becomes constant. The voluntary power is more and more diminished, and the muscles act tremulously. The fur on the tongue turns darker in colour, and the evacuations take place involuntarily. In this way the disease is often protracted to a period of three or four weeks, and then either terminates fatally, or the symptoms gradually subside; the fur on the tongue separating by a kind of exfoliation, the heat of skin abating, and sleep returning.

Comparing this variety of fever with the former, the inflammatory, a striking difference is observed. The disorder of brain is greater, as is evident from the state of the cerebral functions, sensation, voluntary motion, and intellect; while the febrile symptoms, or general disorder of system, are less violent, the action of the heart and arteries being comparatively feeble.

The immediate or exciting causes of this form of fever are not always obvious. In many instances, however, it appears to arise from want, fatigue, exposure to cold, impure air, and depressing passions of the mind, such as care and anxiety. Our continental neighbours, perpetually hunting after novelty, call this "la fièvre adynamique," but, as I consider, improperly, as

it leads to the supposition that debility constitutes the essential nature of the disease, instead of being merely the effect of active disease in the brain.

The proper treatment of this variety of fever is not so obvious as that of the former, and practitioners are much at variance on the subject. Those, and they are not a few, who consider the disease as founded essentially in debility, feel little difficulty in regard to the treatment. The use of wine and other stimulants is freely inculcated, and every thing of a debilitating nature sedulously avoided. Experience, however, does not give its sanction to this mode of practice. The more the patient is stimulated, the more violently the disease proceeds. But it does not necessarily follow that an opposite course is to be pursued. It is evident that, in such a state of the general circulation, evacuations or other debilitating means cannot be used with much freedom. In fact, experience must be our chief or only guide.

In the treatment of this, as, indeed, of all fevers, it is of importance always to bear in mind the seat and nature of the disease; that we are treating a brain affection, and which consists in an inflamed condition of the whole or principal part of the organ; and we should regulate our measures accordingly. At the outset of the disease, or within a day or two of the first attack (where this can be perceived), blood-letting may be had recourse to with perfect safety, and will often bring the disease to a speedy termination. In the state of system, however, that generally accompanies this form of fever, blood should only be drawn to a moderate extent, as from eight to ten or twelve ounces. This may be followed by an emetic, which may be properly repeated at intervals of a few hours for a day or two. After this, if the disease proceeds, it should be left to pursue its course under the mildest palliative treatment.

In most cases this form of fever comes on so insidiously, and excites so little apprehension, that many days usually elapse before it attracts notice. You will find the patient complaining only of slight headache, with listlessness and inability for much exertion; and probably there will have been little or no sleep for several days. Upon looking at the tongue it will be found thickly and extensively coated; you may then be sure that the fever has been of many days' standing, and that, whatever be done, it will not terminate till after as long a period; for it is the character of inflammation, wherever seated, to induce progressively a change in the structure of the part affected—such as enlargement of vessels, with thickening and induration of solids. These changes are only slowly got rid of by a retrograde course. In such cases, therefore, a directly curative treatment is not required, first, because it seldom or never succeeds in accomplishing the object of putting an immediate stop to the disease; and, next, because it is injurious to reduce unnecessarily the general strength where a disease is likely to be much protracted. In fact, in the majority of instances of this mild form of fever, the palliative plan, for the reason stated, is to be preferred.

3. There is a still more aggravated form of simple fever, and which may be properly termed violent, as the brain—the essential seat of the disease—suffers in the highest degree. From the danger attending this variety of fever, it has been often termed malignant or pestilential—the *typhus gravior* of Dr. Cullen. This disease sometimes sets out with symptoms resembling the inflammatory fever, the pulse being full and strong, and the heat of body great. But these after a few days subside, and the functions of the brain become disordered in the highest degree. The arterial action of the brain goes on increasing; and as the arteries become at the same time enlarged in their dimensions, the necessary effect of this is, as before explained, an impeded state of the cerebral circulation—a state very similar to that induced by intoxicating drinks, with which, indeed, it has often been compared; in short, it is a modification of the apoplectic state, differing from ordinary apoplexy (whether the effect of extravasation of blood or simple turgidity of arteries) only in regard to its immediate cause, namely, inflammation.

In this violent form of fever the sensorial functions of sensation, voluntary motion, and intellect, are all in a state of the greatest disorder. The heart is enfeebled in its action, and the whole vascular system is in a state of extreme debility, as the consequence of the oppressed state of the brain. The vitality of the whole system, solids as well as fluids, is impaired in the highest degree. The blood is dark coloured, and coagulates loosely; the different secretions run readily into decomposition; blood is effused into the cellular texture of the solids, or escapes by hæmorrhage from the intestines or urinary organs; the tongue and teeth become incrusted with dark sordes, and livid or purple spots appear on the skin. Parts that are irritated much, or even simply pressed upon, quickly lose their vitality and become gangrenous. To this aggregation of symptoms, the term *putrid* fever

has been applied, a state that rarely admits of a favourable termination. It might be termed, with justice, the apoplectic state of fever.

The form of fever just described was of frequent occurrence formerly, when individuals were crowded together in gaols, or on ship-board, and where ventilation was neglected. The tendency to this state was promoted, also, by the injudicious use of stimulant remedies, under the name of anti-septics or anti-putrescents. Of late years, by the adoption of a more rational mode of treatment—as by attention to cleanliness, and the admission of cool and pure air, and by the avoidance of stimulants of all kinds—such a form of fever is rarely seen.

The pathology of this variety of fever may be easily understood, from the description above given. It is an apoplectic state of brain, the result of violent inflammatory action; by which the organ is rendered incapable of carrying on its functions, and by which every part of the body is, as it were, paralyzed. Such symptoms, when they have once taken place, are scarcely within the power of art to remedy, but they may generally be anticipated and prevented, by the early adoption of antiphlogistic means.

We have the authority of Sydenham in favour of early blood-letting in fevers of this description; but the safety, as well as the utility, of the practice, depends entirely upon the early administration of the remedy. If this period of the disease has passed by, and the symptoms of malignity or putridity appear, little effectual can be done for its relief. The moderate use of wine, bark, and acids, seems to be indicated in these cases, and are perhaps the best means that can be resorted to. The application of cold to the head, in this, as in other forms of fever, is a highly useful practice. It was a prevailing notion a few years back, that this, the typhoid state, was induced by blood-letting or other debilitating means. Physicians were afraid to bleed much, even in active inflammations, lest it should bring on typhus. But nothing can be more unfounded than this. The state termed typhoid is the result of active inflammatory disease in the brain, producing subsequently an oppressed state of the organ, the consequence of impeded circulation there. Bleeding freely, if employed at a sufficiently early period, prevents, but never causes, typhus.

Of the Acute Hydrocephalus of Infants.

I feel no hesitation in ranking this disease among simple fevers—in other words, as an inflammation of the brain and its membranes, since every symptom of the disease, both local and general, leads to this conclusion. Great heat, and, if we may trust to the expression of the countenance, pain in the head; throbbing of arteries, in and about the head; heightened sensibility and irritability, both of mind and body, at the beginning of the disease, ending, at length, in stupor; convulsive movements, not unfrequently terminating in paralysis; and, lastly, mental disorder, as far as this can be judged of in infants: these are symptoms not to be questioned, one would think, as indications of brain affection; to say nothing of the minor characters that often present themselves, especially at the onset: such as frequent vomiting, an altered expression of the eyes, and clenching of the hands. It is of great importance to notice these, as they occur at a period of the disease when art is all-powerful for their relief, while it rarely avails if deferred to a later period. The constitutional disorder, the pyrexia or symptomatic fever, that belongs to and characterizes inflammation—namely, heat of skin, a furred tongue, and, in the early stage, accelerated pulse—is never wanting. In short, the acute hydrocephalus of infants may be considered as analogous to the idiopathic fever of adults. It terminates quickly, and often fatally, from the more rapid and destructive course which inflammation of all kinds follows in very young subjects. So that we have no instances, in such, of fevers protracted to two or three weeks' duration. The disease, if violent, destroys life too quickly to allow of this; or else it terminates with serous accumulation within the skull. I have often remarked, that the older the child is that is attacked with symptoms of hydrocephalus, or rather inflammation of the brain (for this, in fact, is the only proper designation), the more strongly the disease resembles the fever of adults; and the more protracted also is its course.

This is a case that demands the most prompt and active treatment; first, for the purpose of preserving life, which is in immediate danger; and next, if the disease should not at once prove fatal, for the purpose of obviating a mode of termination which renders life scarcely worth possession—I mean, serous accumulation in the ventricles and between the membranes of the brain. Blood-letting is here the chief means of relief, and indeed the only one deserving confidence. Leeches, no doubt, have been often used with good effect; but it is proper you should know that they are generally inferior, in point of efficacy, to venesection. It is seldom difficult to open a vein in the arm of infants, so as to obtain the requisite quantity of blood,

which, at the age of three or four months, may be stated at from one to two or three ounces. Opening the jugular vein is in general a still easier operation, and probably more effectual. An objection exists to the application of leeches, which it is not always easy to avoid: I mean the terror they are apt to excite, and the continued mental irritation they occasion in fretful children; which tend to aggravate the disease. On this account it is generally better, and, I doubt not, quite as effectual, to apply the leeches to the feet as to the temples, as commonly practised.

Blistering is but a poor substitute for bleeding in these cases; and, indeed, is often highly objectionable, by the irritation it gives, both to the brain and general system.

I have watched very attentively the operation of mercury, both internally and externally applied, in numerous instances of the acute hydrocephalus, without feeling convinced of its utility. Cases, no doubt, frequently recover under its use; as many do with little aid of any kind from art: and upon the whole, I have seen more reason to doubt of, than to believe implicitly in, the efficacy of this remedy. But I have no doubt of its often proving injurious, by its deleterious operation on the mouth and alimentary canal; so much so, that I should think it quite unjustifiable to rely on it, to the exclusion of blood-letting, the beneficial operation of which admits of no question.

Of the use of Blood-letting in complicated Fevers.

By the term complicated fever, is to be understood the occurrence of some secondary inflammation during the course of any of the varieties of simple fever before described. There is no part in which this may not take place; but it more frequently happens in the thoracic and abdominal viscera, than in other parts. Such modifications, or secondary inflammations, do not always, or necessarily, add to the violence or danger of the primary disease; often, indeed, the reverse. The secondary inflammation sometimes appears to mitigate the violence of the brain affection, while it is itself rendered less active by the combination. The early occurrence, therefore, of diarrhœa or inflammation of the mucous membrane of the intestinal canal, or even of slight thoracic inflammation, in cases of idiopathic fever, is to be considered as rather favourable than otherwise. In such cases of complicated fever, blood-letting is seldom called for, and only where the secondary affection (pulmonic inflammation more especially) exists with considerable violence.

ON THE PATHOLOGY OF THE OVARIA.

To the Editor of the Medical Gazette.

Sir,

In the 17th volume of the Medical Gazette I communicated an interesting case of pregnancy of the fallopian tube: since then I have had repeated opportunities of observing various diseases of the ovaria, and have selected for insertion in your valuable periodical three cases, which, from their nature, I am induced to believe will be interesting to your readers. I shall not here enter into the physiology of the uterus and its appendages, since, indeed, these have been so fully explained by various authors on midwifery. However, it must be borne in mind that the several parts of the internal genitals of the female are peculiar to themselves, and perform functions different from, although in sympathy with, each other, and are severally liable to various diseases. My attention has been particularly drawn to diseases of the ovaria. In their function they are greatly analogous to the testes of the male. From their safe lodgement within the abdominal cavity, we should naturally presume they would not be so liable to disease, but experience teaches us the reverse. Their peculiar function of secreting the humores fecundi, and the extraordinary manner in which it is intermingled with the semen virile, will easily account for many obstructions of the natural functions of these parts. The internal genitals of the female are more amply supplied with blood than those in the male, the arteries are more capacious and lax than others, and these parts are consequently more liable to inflammation, both acute and chronic.

The following is a case of

Chronic Inflammation of the Left Ovarium.

In November 1831, I was requested to see a young woman of stout robust appearance, of light complexion, and sanguineous temperament, aged 23 years. She was labouring under severe pain, and an enlargement of the right side: these had existed for several months previous to the time of my seeing her, and many remedies had been ineffectually employed. The catamenial

discharge had been very irregular, and latterly had altogether ceased. She complained of a fulness and throbbing of the left side, with a feeling of stiffness extending to the thigh: great languor and debility; thirst and fever towards the evening; and she would frequently sit for a whole day without moving, unless urged to do so. On examination, the left side was considerably enlarged, the left iliac and a portion of the hypogastric regions being occupied by a tumor of a decidedly solid character, and about the size of a moderate melon. It appeared moveable, and gave her great pain when handled. On pressure over the lumbar vertebræ, great pain and uneasiness were occasioned, at times producing sickness. From the situation of the tumor, and the great derangement of the uterine system, I had no doubt of it being an enlargement of the left ovarium. This young woman continued under my care for several months, during which time the various preparations of iodine were given with great advantage, together with cupping, bleeding, and blistering; till the pain was entirely removed, the menstrual discharge re-established, and the enlargement considerably reduced. Some few months after this period my patient got married, and since then has borne her husband several healthy children, and she herself now enjoys prosperous health.

Remarks.—The case just detailed was one of simple enlargement of the ovarium from inflammation; and the sufferer was a young woman who had been brought up to hard work, frequently in the habit of lifting great weights, and more frequently exposed to heat and cold, which doubtless had much to do in exciting the disease. As I have before observed, there exists great sympathy between all the parts of generation; so we can more easily account for these parts being very liable to sympathetic inflammation. When we consider how many patients seek medical relief for an anomalous pain in the side, with other symptoms of general derangement of the system, it demands from us a more accurate observation as to the latent cause of these complaints. Inflammation once excited in the ovaries will put on a peculiar character, influenced mainly by the the habit and constitution of the body. For instance, we may have a simple case, like the one just related, or it may form an inflamed tumor, and run on to suppuration, or excite disease of a sarcomatous character, similar to the second case related in this paper, or a diseased action, which will produce encysted or ovarian dropsy.

We may have idiopathic inflammation of the ovaries excited independent of any similar condition of the uterus; and I believe the former case to have been one of this character. In all such cases the ligaments of the uterus on the affected side are greatly engorged with blood, as in inflammation in other parts of the body. This evidently indicates an antiphlogistic line of treatment; but young females, generally speaking, cannot stand any great loss of blood; it ought to he taken in small quantities, and at different times. In the treatment of the case I have related, I found the ioduret of iron a valuable remedy, and I have repeatedly had opportunities of using it in cases of emansio et suppressio mensium with great advantage; but it is proper to state, in the above case, both the hydriodate of potash and the tincture of iodine were extensively given.

Case II.—*Sarcoma of the Ovarium.*

The second case is one of sarcoma of the left ovarium, which occurred in a lady, aged 26 years. She was the mother of one child, and lost her husband about the time of its birth. She was a woman of dark complexion, of bilious temperament, and hypochondriacal. About two years and a half subsequent to his death, she was observed to decline in health. I was first requested to visit her professionally in September 1833. At that time she was suffering under violent pain of the left side, extending over the uterine region; the menstrual discharge had been irregular and painful; there was considerable fever, with sickness; and the bowels were costive. From an antiphlogistic line of treatment she recovered her usual state of health, and continued so till November 1835. At that time I was again consulted, in consequence of a relapse into her former complaint, but not in so acute form. The lower part of the abdomen was considerably enlarged, and occupied by a large tumor; the bowels were very costive, and there existed symptoms of hypochondriasis to a fearful extent. She suffered from fluor albus, and the catamenial discharge

had not been in efficient quantity. On examination, the body was found to be generally emaciated, and the abdominal cavity occupied by a large tumor: by a more careful observation, the enlargement was evidently that of the ovarium; it had already increased to a very considerable size, it was very irregular on its surface, and very solid to the touch. A few weeks after I was first called in, the symptoms of peritoneal inflammation became very acute, and this was soon attended with a liberal effusion of serum. The tumor continued to enlarge very rapidly, her health proportionally declining till the following April, when the breathing became very difficult from the great distension of the abdominal parietes. The abdomen at that time contained several pints of fluid in addition to the increased size of the solid tumor. It then became necessary, for temporary relief, to perform the operation of paracentesis abdominis. The relief from the operation was of very short duration, the solid enlargement increased more rapidly, and effusion in the peritoneal cavity again took place, but not in any considerable quantity. Her health kept gradually declining, while the disease progressed till the following May, when she expired completely exhausted.

I made the post-mortem examination sixteen hours after death. The body externally presented great attenuation; the parietes of the abdomen were remarkably thin, from the severe distension which they had undergone. On opening the cavity of the abdomen, the peritoneum was found much thickened with very extensive adhesions to the solid tumor, which occupied the hypogastric, right and left iliac, left lumbar, umbilical, and also a small portion of the right hypochondriac regions. On its anterior surface there existed two large abscesses in its walls. It was lobulated, presenting several small vesiculæ externally. On raising it, there appeared three large lobes, each of which presented several smaller ones. The posterior lobe was occupied by a large sac, which contained upwards of two quarts of thick oily fluid; the interior of this sac was very irregular, thickly coated with a soft substance like the brain, and the walls of this lobe, in several places, partook of the same character. The left or smaller lobe was also occupied by a sac containing upwards of a pint of similar fluid. The anterior and larger lobe was of a more solid character; this at different places was occupied by pellucid vesicles, some containing a drachm of fluid, others more, others less; but it consisted principally of a dark-brown substance, very soft in some places, having much the appearance of a brain that had run on to putrefaction. The ligamentum latum on the affected side was highly vascular, and the arteriæ uterinæ et spermaticæ were much larger than in a normal state. The body of the uterus was not half its natural size, and from the pressure of the tumor its cavity was nearly obliterated; and on the right side there scarcely remained a vestige of the ovarium. The bladder was much contracted, and contained about two ounces of urine. The whole of the bowels (with the exception of the rectum) were compressed into the epigastric and hypochondriac regions. There had existed severe inflammation of these viscera, which had caused them to be glued together with organized lymph. The other organs were in a healthy state.

Remarks.—This disease commenced with active inflammation of the left ovarium, which, from the peculiar state of the constitution of the patient, laid the foundation of the malady which terminated her existence. I have no doubt the disease continued in an insidious manner to develop itself from the time of the first attack, although there was no palpable enlargement of the ovarium to be felt. The patient's mental suffering, from the loss of her husband, and having been left in thrifty circumstances, were highly favourable to the advancement of disease. It is reasonable to presume, the whole of the proper structure of the body of the ovarium was involved in the inflammatory action; and being compounded as it is of vascular and cellular lobules gorged with a large quantity of fluid, will explain in some manner the tendency this organ has to the formation of heterologous masses of this kind. The nature of enlargements of the ovarium is generally very obscure: although, I believe, more frequently ovarian tumors consist of steatomatous deposits, yet their composition is subject to various changes and modifications. Diseases of the ovaria much influence the function of the uterus, but in various ways, as at one period of the disease the men-

strual discharge may be in superabundant quantities, and at irregular times; while at another stage of the disease it may be altogether absent, or small in quantity, and attended with pain.

In the treatment of diseases of the ovaria, medicines seem to possess but little or no influence; in this case, iodine and its various preparations were given in large quantities, and continued for some time without any good effect; subsequently the system was brought under the influence of mercury, but with no better advantage. I was surprised, on making the post-mortem examination, to find how easily the tumor might have been removed before the inflammatory symptoms came on, and which, no doubt, were excited by the rapid growth of the tumor. I believe the operation for extirpating the diseased mass would not only have been justifiable, but also very desirable; and if I had another case similar to this, where it was evident the disease would shortly terminate the patient's life, without any other hope but that of an operation, I should certainly recommend her to submit to it, and that at an early period of the attack, before any adhesion to the peritoneum had taken place from inflammation.

I believe, on the 25th of November, a few days after I had been first called in, when the enlargement did not much exceed the size of a child's head, it might have been removed safely and expeditiously, with some chance of success; but after the symptoms of peritoneal inflammation had once commenced, it would have been madness to have attempted an operation. The great objections to operations of this kind are, the danger of subsequent inflammation, the adherence of the tumor to the peritoneum, and the probability of the same kind of disease attacking some other part of the body; but almost the same objections may be raised against every operation for the removal of malignant diseases.

Case III.—*Ovarian Dropsy, attended with Pregnancy.*

The next case I have to relate is one of ovarian dropsy, which occurred in a married lady, aged 26 years, of a lively disposition, but very irritable; of a pale complexion, which had at times a dusky appearance. She is of middle stature, and very thin, and has had very delicate health ever since she left boarding-school. She has always been of a very costive habit of body, and obliged frequently to seek relief from aperient medicines; the stomach has always been extremely irritable, and would frequently eject her food as soon as taken, and at other times large quantities of bilious matter. In April 1834, she had a violent attack of inflammation of the liver; and although very actively treated at the commencement of the disease, she was a long time before she recovered her usual health. This happened about six weeks after her first confinement. The child was not nursed at the breast, owing to violent inflammation and ulceration of both nipples. About nine months after her confinement, and seven or eight months after the attack of hepatitis, she experienced a gradual increase in the size of her abdomen, which at first induced her to believe herself to be again pregnant; but owing to her not having the same symptoms as before, and the regular appearance of the menstrual discharge, induced her to ask the opinion of her friends, who made light of the matter, and insisted that she was pregnant. From this time several months elapsed, during which the body decreased in size, and she thought the enlargement considerably reduced. This, however, did not last long; the belly soon began to increase more rapidly than ever, with pain and uneasiness of the left side, the menses were more frequent and in much greater abundance than usual, which greatly affected the strength and spirits of the fair sufferer. From these circumstances I was consulted in February 1836. At that time the abdomen appeared very large, while the general appearance of the body was very spare. The bowels were costive, the urine scanty, depositing an abundance of lateritious sediment. There existed considerable pain around the lower part of the loins, with a fixed pain in the left side near the scrobiculis cordis. There had existed great pain of the abdomen, which rendered my patient unable to wear her accustomed clothes; still, however, her health had not been so bad as to prevent her attending to her usual avocations. On examination I was much surprised to find the body greatly distended with fluid, which seemed to occupy every part of

the abdominal cavity; pain was experienced on pressure over both the iliac regions, also over the right hypochondrium and scrobiculis cordis. On taking exercise, or using any exertion, great uneasiness and difficulty of breathing were experienced. The pulse was regular, about 76 in the minute; and the centre of the tongue was covered with a brown-coloured fur. At times she complained of vertigo, and was subject to sick or bilious headaches. Latterly her nights had been restless, and she was obliged to lay with her head elevated. She was treated medicinally with the various remedies usually employed in such cases, with but little or no advantage, till the following August, when she determined to resort to Cheltenham, as on a former occasion she had experienced relief from the chalybeate waters. However, she returned home after some weeks, much in the same state, as far as the disease was concerned, but her general health was improved. The disease kept gradually increasing, notwithstanding the various local and general remedies employed, till February 13th, 1837; when, from the great distension of the belly, difficulty of breathing, and repeated attacks of spasmodic pain, it became necessary to relieve the patient by the operation of paracentesis abdominis, when there were evacuated twenty-six pints of a thick ropy mucilaginous fluid. Subsequently to the operation, the same train of remedies were employed, the action of the skin became greatly increased, and the size of the abdomen kept diminishing. On examination, the right ovarium and the fundus of the uterus were distinctly to be felt, the former of which appeared very painful on pressure, and the latter about the size of a large powder-flask. The breasts became painful soon after the operation, the areolæ more dark in colour, and the nipples very sensitive and protubcrant. The patient now stated that she had not been regular for some months previous, and that at times she felt something beating in the lower part of her belly like a pulse. However, on a careful manual examination, I could not satisfy myself that she was pregnant. For some time after this, my patient's general health continued to improve, but the abdomen kept again gradually increasing in size. Owing to the great quantity of medicine which she had previously taken, she determined to allow nature to take her course.

On March the 6th my services were again requested, the signs of pregnancy being very unequivocal. I found her general appearance improved; but her size was very considerable, and of an unwieldly appearance. On examination, the lower part of the abdomen was occupied by the impregnated uterus, which had risen above the brim of the pelvis, and pressed towards the right side: the movements of the fœtus were very distinct: the upper and surrounding parts of the abdomen were greatly distended with fluid. However, as the patient did not suffer any severe pain, or other symptoms demanding interference, I recommended her to wait the result of her accouchement. For six weeks previous to this event, she was greatly troubled with a small miliary eruption on the abdomen, which, when she was warm, produced great irritation.

On July 11th she was safely delivered of a healthy female child. The labour was lingering and severe: the placenta was expelled naturally, and every thing appeared going on well till the following evening, when the nurse observed something bearing down, and on my visit named the circumstance to me; and also, that there existed a frequent and almost irresistible inclination to micturate. The after-pains were urgent, and attended with slight symptoms of pyrexia. I thought, from the quantity of fluid pressing upon the fundus of the uterus, it had become forced from its natural position; and, on examination, I found it in a complete state of prolapsion. I carefully reduced the uterus by means of a soft sponge smeared over with hog's lard, but found considerable difficulty in retaining it in its proper position. I gave the patient an opiate to quiet the system and allay irritation, and left her for the night.

On my visit the following morning, July 13th, I found that my patient had passed a restless night, and suffered much from after-pains, with constant sickness, the stomach rejecting every thing as soon as taken. The uterus was again prolapsed to a greater extent than before; the lochia scanty and pale; urine small in quantity, mixed with mucus; the tongue covered with a white fur; the pulse was increased in frequency to 96, and sharp. The patient

complained of a tightness round the waist, and a constant pain round the lower part of the loins. After several attempts, I succeeded in reducing the uterus as before, and retained it in its proper situation by means of a large sponge pessary.

Anodyne and saline medicines were given, and an aperient enema prescribed

On the following day the symptoms of pyrexia were less, the pain had abated, and the stomach had retained some milk. The pessary was forced down to the os externum, and the lochia had increased in quantity; the abdomen appeared to get larger and more tense; the child had been put to the breast several times, but the secretion of milk was scanty. The pessary was changed, and the remedies were persevered in, viz. calomel, squills, digitalis, and salines, till the 16th, five days after the accouchement, when the uterus became forced into the vagina, which condition was attended with urgent sickness. The abdomen continued daily to increase in size, and its parietes were extremely distended. The nights were passed without sleep, the breathing became harassed with a cough and expectoration of large quantities of thick mucus, and the patient was unable to move herself in bed. It therefore became necessary to perform the second operation of paracentesis abdominis on the 21st of July, ten days after delivery. Immediately subsequent to the operation the patient experienced considerable relief; the uterus retained its proper position without the aid of a pessary; there were no symptoms of fever; the skin became moistened by a profuse perspiration, and the bowels freely relieved. She refused to take medicines in consequence of the great irritability of the stomach, and every thing appeared going on well till the 26th of July, five days after the operation. During the evening she was taken with a shaking fit, which was followed by sickness and violent symptoms of pyrexia. On examination there was general pain over the whole abdomen, particularly over the liver and stomach, and also over the left iliac region, which was occupied by a sac containing about two quarts of fluid, and which was easily moveable. Quantities of bilious matter had been repeatedly vomited during the night, and the stomach refused to retain either fluids or solids. The patient complained of violent pricking pains within the whole abdominal cavity, particularly those portions which were occupied by the distended sac, liver, stomach, and spleen. The pulse was frequent, and small in volume, and the tongue was covered with a buff-coloured fur. The bowels were costive, and the urine scanty.

Sumat. haust. sal. in statu effervescentiæ 4ta quâque horâ.

℞ Hydr. Chloridi, gr. iij.; Extr. Opii, gr. viij.* ft. bol. h. s. sumend.

27th.—Has passed a restless night; the abdomen very painful on pressure; constant sickness; has retained nothing but a little milk on the stomach since last visit. The pulse 120, and very feeble; the tongue covered with a dark brown fur; the skin very hot and dry. The bowels have been moved once; the other symptoms as before.

Perstent remedia salina.—Fovus calidus abdomini applicetur.

℞ Hydr. Chloridi, gr. iss.; Pulv. Opii, gr. ss. ft. pil. 4ta quâque horâ capiat.

28th.—Much the same.

30th.—Much worse this morning; has had no sleep during the night; lies with the legs drawn up, and complains of the weight of the bed-clothes touching her body, although very lightly clothed. The bowels have been repeatedly acted on, and the motions have the appearance of thick green paint, and are extremely offensive; the abdomen has greatly increased in size; the pulse rapid, and very feeble; the fur covering the tongue is of a chocolate colour. The mouth is not affected by the calomel, and the expectoration of thick mucus continues, which amounts to about a pint in twelve hours.

Appl. Hirudines x. abdomini.—Enema simplex injiciatur.—Repet. alia.

31st.—Has had about two hours' sleep after three o'clock this morning, but was very restless and delirious the fore part of the night. The belly is not so painful on pressure, but she has been frequently harassed with sickness; she is extremely low this morning, and can scarcely answer questions.

Perstent omnia.

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* So in the MS.; but we presume the "Extr. Opii, gr. viij." to be a mistake.—Ed. Gaz.

Aug. 1st and 2d.—Has passed bad nights, and in no respects better.

Pergat.

3d.—Slight ptyalism; abdomen considerably distended, but not so painful on pressure; the excretions are unnatural, and very offensive; has perspired during the night; the expectoration continues in great abundance, with a slight cough.

Omit. pil.—Repetantur alia.

4th.—The bowels have been acted on seven or eight times since last visit; the ptyalism has greatly increased, and the salivary glands are swollen and painful. She complains of great pain and oppression in the chest, under the centre of the sternum, and has been harassed several times with sickness.

℞ Mist. Cretæ c. f. 3x.; Guttæ Nigræ, ℥iv. M. ft. haust. post sedes liquidas sumendus.
℞ Tr. Opii, ℥lx.; Tr. Catechu, f. 3iv.; Dec. Amyli, f. ℥iv. ft. enema statim injiciendum.
Gargarisma Sodæ Biboratis pro ore sæpe utendum.

5th.—Has passed a better night, but complains of pain in the mouth and jaws; the bowels have not been moved since last visit; sickness frequent; the tongue and mouth are covered with a thick white fur; expectorates large quantities of saliva, mixed with a thick purulent secretion from the bronchia. The abdomen continues of very large size, but is not so painful on pressure; the urine continues small in quantity, and very high coloured.

℞ Sodæ Bicarb. gr. xv.; Aquæ, f. 3x.; Sp. Æth. Nit. ℥xxx.; Sp. Ammon. Aromat. ℥xx. M. ft. haust. cum pulvere sequente, in statu effervescentiæ, sexta quâque horâ, sumendus.
℞ P. Acidi Tart. gr. x. fiat pulvis.
℞ Hydr. Chloridi, gr. ij.; Extr. Opii, gr. iij. ft. bol. h. s. sumend.

[From this time till the 19th little change occurred; some improvement was noticed on that day, after which the reports proceed.]

21st.—She continues to improve; passes better nights; and is refreshed from her sleep. The difficulty of deglutition is nearly subsided, and she feels a desire for food. The abdomen still continues a great size, and is painful on pressure; the bowels are acted on three or four times in the 24 hours, and the evacuations are more natural. The pain of the chest and the difficulty of breathing have entirely disappeared; and, in a word, she has improved in every respect.

From this time my patient became convalescent, and by careful nursing was able to sit up a short time daily. I frequently examined the abdomen, and found it keep gradually decreasing in size, as the health of the fair sufferer improved. The principal thing which now demanded attention was the state of the secerning organs. The bowels were kept regularly open; the kidneys acted more freely than they had done for some time; and the skin was frequently covered with profuse perspiration. In the beginning of September, as soon as she was able to be removed for a change of air, she paid a visit to some relations in Leicestershire, where she remained a fortnight. On her return home, I was truly astonished to find so great an improvement. The abdomen was quite reduced in size, her appetite had improved, and she had gained her strength surprisingly; however, she informed me that she frequently experienced darting pains in the left groin, and, if the bowels were confined, a general pricking pain within the abdominal cavity, particularly its lower part.

On the 11th day of October I made a manual examination for the purpose of ascertaining whether the whole of the fluid had been removed by absorption; and, to my gratification, I could not find a vestige of the large tumor which previously occupied the left iliac and hypogastric regions. The abdomen appeared rather fuller at its lower part than natural, but I could not detect any undulation on percussion.

August 1st, 1838.—I have frequently seen this patient, but she has had no return of the ovarian disease.

Remarks.—In offering any observations on the foregoing case, it will be necessary to call to mind the peculiar investments of the ovaria. They are covered externally with peritoneum, which is derived from the fundus and body of the uterus posteriorly, and from the ligamentum latum anteriorly. Within this we find them inclosed in another membrane, the tunica albuginea: in a healthy state, these two membranes are intimately connected with each other. On cutting through

the latter membrane, we come to the body of the ovarium itself.

When we take this view of the organ which frequently is the seat of dropsy, it is not surprising that we should most frequently have it encysted; still, however, this will be influenced by the tissues involved in, and forming the seat of, the disease. For there is every reason to believe that effusion may take place between the peritoneal investment and the tunica albuginea, altogether external to the proper substance of the ovarium. In this latter case we should expect to find the fluid contained in one sac; while if the ovarium itself become the seat of the disease, it would necessarily be contained in more than one sac.

There are several reasons which induce me to believe this case to have been a simple one of effusion of serum between the coverings proper to the ovarium. Firstly, because the fluid was contained in one sac, which was completely evacuated by the operation of tapping, and that at two different times. Secondly, from the uniformity of its circumference. Thirdly, if the body of the left ovarium had been involved in the disease, the right would have sympathized with it, and not have performed its proper function sufficiently for impregnation to have taken place. And fourthly, it seldom, if ever, happens, when there exists a disease of this character affecting the proper structure of the ovarium, that a cure is performed. But as far as the treatment of ovarian dropsy is concerned, it probably matters little as to the seat of the disease; medicines appeared in this, as in all similar cases, I have observed, to produce very little or no direct action on the diseased part. I believe the cure of this case to have been effected by severe inflammation of the sac containing the fluid, and the consequent absorption of the latter. There had existed for some short time previous to delivery symptoms which threatened a general and severe attack of inflammation of the whole abdominal cavity. I have no doubt the action of the proto-chloride of mercury was very beneficial, and greatly assisted the curative process: it improved all the secretions, particularly those of the liver and kidneys. The cough and other distressing symptoms of the lungs were only symptomatic, and caused by the great distension of the abdominal parietes. We should not have expected impregnation to have taken place with so great a disturbance of the internal genitals; and it is more surprising that abortion did not take place after the first tapping. It was not till after the operation I was aware of my patient being pregnant, as there had never been other symptoms of it than the absence of the menses, which had previously been very irregular. Almost immediately subsequent to the operation the signs of pregnancy became very evident; and notwithstanding the rapid manner the fluid again collected, I believe the full period of gestation to have been accomplished. The uterus never ascended much above the brim of the pelvis, and was compressed in a small compass, and to the right side. It will readily be seen that in the treatment of the above case I had only one object in view, which was not to interfere with the efforts nature herself made, but to assist them, by giving such remedies as appeared most consistent with that purpose.

After delivery the abdomen rapidly increased in magnitude, and it became impossible to confine the uterus in its proper position, but the operation immediately obviated this distress; and since then there has been no further symptoms of prolapsion of the uterus.

I am, sir,
Your very obedient servant,
WILLIAM ROBBS, M.R.C.S.

Grantham, August 1st, 1838.

INTERESTING MIDWIFERY CASE,

WITH REMARKS.

To the Editor of the Medical Gazette.

SIR,

SHOULD you deem the annexed case, with remarks, of sufficient interest to merit an insertion into your scientific and valuable journal, it is at your disposal.—I am, sir,

Your obedient servant,
JAMES PATERSON, M.D. &c.

Stevenson Street, Calton,
Glasgow, August 22, 1838.

Mrs. R. B. Tylefield, aged 29, about the middle size, and of a good constitution, menstruated on the 12th of January last, and in a few days afterwards became pregnant for the eighth time. Her first labour was comparatively easy, and only of three hours' duration from

the commencement of regular pains, but every labour since has been more difficult and tedious than the one preceding it. Her first three children were born alive, but the fourth required the use of the forceps, and was born dead. The fifth likewise required the forceps, though the child was remarkably small. It was born in a state of complete asphyxia, but by the persevering exertions of the medical attendant it ultimately recovered. I was employed to attend her in her sixth confinement, which occurred on the 26th of May, 1836. On carefully examining the dimensions of the pelvis at that time, I found that the conjugate or antero-posterior diameter did not exceed three inches; and after a most tedious and painful labour, it being impossible to deliver her in the natural way, or with the aid of forceps, I was obliged to have recourse to embryotomy; when she was delivered of a very large male child. She made an excellent recovery. I then informed her of her danger if she again became pregnant, as it was evident, from the history of her previous labours, that the pelvic bones were gradually encroaching on the capacity of the cavity, so as to render every succeeding confinement more difficult and dangerous. In the event of such an occurrence, however, I advised her, for her own safety, to have labour induced at such a period as it might be possible to give birth to a living child, and, at all events, with a better chance of a safe recovery for herself.

In a short time afterwards she became pregnant, and consulted several respectable practitioners, who all agreed in the propriety of inducing premature labour. By the evil suggestions of some ignorant and officious friends, however, she disregarded this advice, and allowed herself to go to the full period of gestation. A surgeon was engaged to attend her in her accouchement, who, having waited on the case for some days, found it impossible to deliver her in the natural way. A professional friend of his own was called in, and they agreed to perforate the head; which operation, with the extraction of the child, occupied them upwards of four hours. When it was accomplished, they found that, by the improper application or management of the instruments, they had extensively lacerated the bladder! For the treatment of this unfortunate accident she was afterwards sent to the Royal Infirmary; from which, in two months, she was dismissed relieved, but not cured, as she is still unable to retain her urine.

In May last she came to consult me, and I advised her, as formerly, to have labour induced in the seventh month of utero-gestation. At the urgent solicitations of herself and friends, I agreed to undertake the heavy responsibilities of the case, provided she would abide by my instructions, and submit to whatever treatment might be considered necessary or best suited to her situation.

On the 13th of June I made an examination per vaginam, and found that the pelvis was even more deformed than when I attended her in 1836, and that the outlet was considerably smaller. Owing to some unnatural adhesions at the superior part of the vagina, I could not satisfy myself as to the locality of the os uteri, but thought I felt it turned up behind the symphysis pubis.

On the 3d of July I again saw her; the abdomen was pretty large for the sixth month, and the motions of the child were very lively. She confined herself mostly to the house, on account of the constant draining of her urine.

On the 11th instant her bowels were freely evacuated by the pil. colocynth. comp., and on the following morning I saw her. She was in her usual good health, and felt no pains or uneasiness, as if labour pains were about to commence. I made a most careful examination to ascertain the state of the os uteri, in order correctly to observe the effect of the medicine about to be administered, and thought, as formerly, that I could detect it perfectly closed, and situated immediately behind the symphysis pubis, with the anterior lip attached to the posterior portion of the bladder, about half an inch beyond the termination of the vesico-vaginal fistula. On carrying the finger cautiously round the posterior part of the neck of the uterus, a feeling was communicated as if there had previously been a slight laceration of the mouth or neck of that organ, the adhesions presenting a sense of hardness to the touch.

At 10 o'clock A.M. she commenced taking every third hour two ounces of the following:—

℞ Secale Cornut. ℥ss.; Aq. Bull. ℥xxiv.; Syr. Simp. ℥j. M.

10 o'clock P.M.—During the day she was troubled with a heavy pressing down between the loins, and at the lower part of abdomen, similar to what she has usually experienced about the commencement of her labours; and now slight uterine pains occur at intervals of an hour. On examining, I readily found the os uteri, which was dilated to the extent of a sixpence; and likewise my former suspicions verified, as to the attachment of the anterior lip to the bladder. On carrying the finger gently round the inside of the os uteri, I felt the membranes distended with the liquor amnii, but could not make out the presentation.

13th, 11 o'clock A.M.—Had a restless night, with slight grinding pains, and a constant bearing down. Os uteri dilated to the size of half-a-crown.

Pergat.

10 o'clock P.M.—Has taken all her medicine. About one o'clock pains became more severe, and are now present every five or six minutes, with very good bearing down. Os uteri dilated to the extent of a crown-piece, but the child is so high that the presentation cannot yet be made out. In half an hour I gave a drachm of the pulv. secale infused in four ounces of boiling water, and at 11 o'clock the pains became considerably brisker, and the bearing down stronger; but at 12, midnight, they slackened much in point of strength, though not in frequency. The head had entered the cavity of the pelvis, and presented naturally; os uteri fully dilated. She was in good spirits, and not much exhausted; pulse 64, of good strength. At half-past 12 I repeated a drachm of the secale, as before, which, in twenty minutes, excited strong uterine contractions. The head descended to the outlet of the pelvis, and on rupturing the membranes, which protruded at the vulva, an immense quantity of liquor amnii was discharged. Pains now continued without intermission, with excellent bearing down, and the delivery was accomplished at a quarter past one o'clock on Tuesday morning, being exactly thirty-nine hours and fifteen minutes from the first exhibition of the secale cornutum.

The child, a male, was at first languid, but in a few minutes respiration was freely established, and I divided the umbilical cord. The placenta was removed in a quarter of an hour; there was no hæmorrhage. With the exception of her first labour she said this was the easiest and the shortest she had ever undergone.

14th, 11 o'clock A.M.—Since I left her she has slept for three hours, and finds herself remarkably well; is not troubled with after-pains; pulse 60, regular and good; lochial discharge free; has no complaint. During the early part of the morning the child became weak, moaned very frequently, and died before my hour of visiting.

15th. — Wednesday morning, still doing well, and requires no treatment; bowels free.

17th, 11 A.M.—Milk copiously secreted; never felt so well in so short a period after confinement.

21st.—Was out of bed the greater part of yesterday, and is now attending to her household affairs.

REMARKS.—This is probably one of the best cases on record illustrative of the power of the secale cornutum in promoting the induction of premature labour. In almost all the cases which have been published, where the secale has been used for this purpose, it has been combined with some other medicine, or given after other means had been employed, so that it is impossible to form a proper or correct estimate of its real medicinal virtues; and hence, I believe, originates the vast diversity of opinions which are entertained amongst medical practitioners respecting its properties; some extolling it as a boon to the obstetrical department of the profession, while others utterly discard it as unworthy of a place in the materia medica. I, for one, have had many opportunities of testing its value, and have exhibited it under almost every variety of circumstances where it was proper to be given, and after carefully observing the effects which it produced, and minutely examining the inferences drawn from the results, and likewise its effects in the above case, I have no hesitation in saying, that I regard it as the best and most powerful medicine we possess for exerting, as it were, a specific influence over the gravid uterus. When it was fresh, or well preserved, and properly prepared, I never found it fail in accomplishing the object for which it was administered; and of all the modes of giving it, I have observed that the infusion is the best, and if exhibited warm it gene-

rally operates sooner than when taken cold.

In Mrs. B.'s case there can be no doubt that the secale was the sole agent in producing labour. For months I looked forward to it with deep interest, as I had determined to give the secale a fair and candid trial, in order fully to satisfy myself with regard to its real efficacy. For this purpose I reduced half an ounce of it to a very fine powder, and having added twenty-four ounces of boiling water, it was allowed to infuse with a gentle heat for thirty-six hours; after which, one ounce of simple syrup was added, and the whole was carefully shaken every time the patient partook of it, so that it was taken in substance as well as in infusion. Here there was nothing combined that could possibly excite uterine action, and in making any examination to ascertain the progress she was making, the utmost care was observed not to detach the membranes, or to cause any irritation which might stimulate the uterus, or interfere with the action of the medicine. It affords, therefore, a most convincing and satisfactory proof that the secale cornutum, if properly administered, is of itself sufficient to accomplish the induction of premature labour, at all events, in the advanced stage of utero-gestation.

I am fully aware that very serious and plausible objections have been urged by many talented and well-meaning practitioners against the practice of inducing premature labour; but the necessity for it, in the present instance, was not a matter of choice, but an imperative and bounden duty; and was justified by every principle of humanity—by every principle of morality—as well as of science; and its propriety must, I think, be evident to every well-informed and experienced obstetrician. The previous history of the patient afforded a most melancholy proof that the capacity of the pelvis was becoming more and more diminished; and the stubborn fact stared me in the face, that her two last children had to be destroyed by the perforator before that deliverance could be obtained. The induction of labour, therefore, at a period when it was possible for the child to maintain a separate existence, and likewise, from its size, to pass the cavity and outlet of the pelvis, afforded the only chance which remained for the safety of the infant. It was undoubtedly easier and safer for the mother than embryotomy, as her last confinement has testified to her sad experience; and no rational being can maintain that it terminated worse for the child, because it was born alive; and though it only lived for a short time, yet this was not attributable either to the secale or to the operation of inducing parturition. I have frequently seen children, born at the full time, and under favourable circumstances, expire in the same way, within twenty-four hours of their birth.

In all such cases as the present, the only alternative left to save the offspring is the Cæsarian operation; but the dreadful fatality attending this operation, whenever it has been practised, in Great Britain or on the continent, will deter every conscientious man from performing it, when, by the induction of premature labour, or the destruction of the child, delivery can be procured *per vias naturales*.

The induction of premature labour appears to have been practised at a very early date, for it has been recommended by Paulus Egineta, and several ancient physicians, in cases of extreme contraction of the pelvis; but, in this country, the practice was first adopted about the middle of the last century, by Dr. Macauly, and other eminent practitioners in London, who decided on its utility, safety, and morality. From that period to the present day, the records of obstetrical science prove satisfactorily that the mother is not subjected to much greater danger by the induction of premature labour, when judiciously performed, than by spontaneous parturition at the full period of gestation.

In my opinion it may be laid down as an incontrovertible axiom in midwifery, that if the antero-posterior diameter of the pelvis be less than the space of three inches, and more than two and a half, the induction of labour at the seventh month of utero-gestation becomes an indispensible duty; and this conclusion is the more obvious when we consider the disproportionate size of the fœtal head at the expiration of the ninth month, when we naturally expect that parturition should occur. It is true that the practice has not generally been so fortunate for the child as it has been for the mother; but, all things duly considered, this result is of minor importance; for, in a natural or moral point

of view, the life of a fœtus cannot for a moment be compared in value with the life of the parent.

On reviewing the case, I have no regret in having acted as I did, though, from the contracted state of the pelvis, should she ever again require my assistance, I will consider it proper to induce labour at the very beginning of the seventh month; for, notwithstanding the small dimensions of the head, it required upwards of half an hour to pass the pelvic outlet, though the pains were remarkably strong; and I believe, had the induction of parturition been postponed for another fortnight, the use of the forceps would have been required. The ramus of the ischium encroaches considerably on the outlet of the pelvis, so as to render the birth of a child full grown at the ordinary period of gestation utterly impossible. My limits will not permit me to advert to the other operative methods which have been proposed and practised for the induction of parturition by the highest authorities in the profession, not only in cases of pelvic distortion, but in others entirely unconnected with it. I have conceived it my duty to submit this case to the consideration of my brethren in the obstetrical department of the profession, and more particularly so, as many talented practitioners, whom I esteem, deny that the secale cornutum itself can accomplish the induction of premature labour.

SOUNDS OF THE HEART.

To the Editor of the Medical Gazette.

Sir,

The following observations being the result of a careful and clinical post-mortem observation of disease, I offer them to your readers, hoping they may present a few points on which diagnosis may rest. Being extracted from a paper read before the Physical Society at Guy's, eight months ago, the heads of which were given in your report of their proceedings in February last, I shall be exempted from the charge of plagiarism, to which I might otherwise become obnoxious, after the publication of Dr. Williams's lecture a fortnight back. The part to which I allude is that in which he mentions a third sound as occasionally existing in the heart, and this the result of retarded valvular action. In both phenomenon and rationale it will be seen how much the accounts agree.

Physiologists, continental as well as English, have, during the last few years, assiduously endeavoured to unravel the physiology of the heart; and whilst at first sight but little advancement would appear to be made, the search, like that after the philosopher's stone, has much benefited science.

Facts founded upon physical and vital laws have been established, from which clear deductions may be drawn. The great bar to the correct appreciation of the phenomena, seems to have been the exclusiveness of the theories of the investigators, which prevented them from recognising more than one agent as the cause of a given effect. The theories of the sounds, as produced by motion of the blood, impulse against the ribs, or muscular contraction, are all supported by such incontrovertible facts, and bear so much the impress of truth, that the conviction is forced upon one, that the sounds are essentially compound. This, however, is the least common opinion entertained by the profession. That muscular contraction is attended with sound few will deny, none probably who have taken the trouble to place their heads sideways on a pillow, and bring into action the masseters and pterygoids, when a sound, resembling that of a large shell when placed close to the ear, will be heard. During the contraction of the heart, this cause of sound must be considered determinate.

The relation of the solids and fluids in the heart illustrates the proposition, that *fluids in motion, if obstructed, occasion sound*, the valves opposing the course of the blood in certain directions. In opposition to this, it has been objected by learned authorities, that the first sound of the heart does not at all resemble a sound so produced; the objection is hasty, as aneurismal tumors not unfrequently occasion sounds exactly resembling that during the systole of the heart, the distance of the disease from that organ preventing all possibility of transmission.

Further, the following interesting case demonstrates the fact, that the propulsion of fluids against their containing parietes will cause a knocking sound:—

T. Belfield, æt. 35, a tall, stout, muscular man, admitted into Guy's Hos-

pital, Oct. 10, 1836, under the care of Dr. Bright. General health, robust, has never suffered from rheumatism, but has been engaged in very laborious occupations. A twelvemonth ago, without any immediate cause, palpitation and dyspnœa came on, followed by slight anasarea, beating in the course of all the larger arteries of the body, and whizzing and ticking noises in the head. These symptoms persisted on admission, and angina pains not unfrequently occurred. Violent pulsation was perceptible over the whole arterial system, marked even in the ramifications of the temporal: the ear, when applied, distinguished a sound exactly resembling in character the first of the heart; in the radial, of the intensity of a healthy adult heart; in the brachial, resembling that of a very hypertrophic organ. A see-saw bruit audible at the junction of the first and second bones of the sternum.

The senses of touch and sight plainly indicate that the apex *strikes* the parietes; but the following case exhibits the non-essentiality of this in the production of the first sound:—

F. Broiler, æt. 50, admitted into Guy's Hospital, by Dr. Bright, in the commencement of 1836, the subject of cough, dyspnœa, and pain in the chest; an indurated tumor, of rounded form, peeped from between the second and third cartilages on the left side.

Dr. Bright diagnosed "malignant disease, which the stetheseope indicated to have extensively involved the lungs. The sounds of the heart, though subdued, were distinct and pure. He remained but a short time in the hospital, from which time till he died, including a period only of a few weeks, I attended him at his own house. I examined the body with the assistance of my friends, Mr. Brereton and Dr. Ridge, when the scalpel revealed medullary sarcoma, occupying more than half the thoracic cavity, obscuring from view the upper portion of the lungs and the whole of the heart, except a portion smaller than a shilling near the apex—a condition that must very effectually have interfered with any impulsion against the parietes*. Although doubting the essentiality of this cause, I cannot but believe that it gives intensity and finish to the sound.

"The above remarks are merely preparatory to, whatever its value may be, a new point of diagnosis, viz. a clicking sound in mitral valve disease. Although a frequent symptom, it is not invariable, but obtains in those cases where the curtains of the valve, though thickened, still allow of some freedom of motion. The new sound is always interposed between the first and second, perhaps somewhat nearer the first, though in character it resembles the second. It sometimes becomes rather indistinct, or entirely lost for a few beats, and then re-appears. It is most audible to the left of the sternum. Cadaveric inspection has now verified this diagnostic mark in three instances, (and in two since that period.) I rely upon it as decisive and expressive of no other lesion than mitral disease. The explanation which is offered to my mind is in accordance with the physiological points stated above, viz. that the diseased condition of valve interferes with the freedom of motion in the curtains, and consequently their natural sound, exalted probably by their want of elasticity, is retarded and not heard till the sound produced by the other phenomena has passed by."

Pure mitral valve disease, even when it goes on to the destruction of life, is but rarely attended with anasarea or ascites, and whether the tricuspid valve be refluent or not, the lungs bear the brunt of the distress, and become œdematous and apoplectic. I use the term *pure* mitral disease; for it is peculiar to lesions of the valve that they sometimes occur to such an extent that death occurs, without any lesion in the nutrition of the organ—an extremely rare circumstance in aortic valve disease. I possess the records of several such (mitral disease, without hypertrophy). Dr. Latham also mentions it in his Croonian Lectures; and I have heard Dr. Addison state such as the result of his experience.

The bellows-sound of mitral disease appears to me only to arise from regurgitation into the auricle from patency of the valve, or the opposition of part of the diseased apparatus to the passage of the blood into the aorta.

Statistics of pure mitral disease are not extensive enough to draw any very determined deductions as to the conditions of the pulse; but it appears to me that a contracted non-regurgitant

* To the above might be added, instances where the lung was interposed between the apex and parietes.

valve is more often attended with a frequent pulse, not intermitting, but irregular in power: no bellows-sound.

A regurgitant valve, on the other hand, appears to be attended with the most marked irregularities and intermissions, and with bellows-sound.

There is an anomaly connected with the bellows-sound, which does not appear to me easily explicable; why it should be heard nearer the axilla than the sternum, often almost in the dorsal region; yet there it most certainly is. The loudest bellows-sound I ever heard in a heart was in the case of a poor boy, admitted some months ago into the hospital, suffering from intense dyspnœa and palpitation. His father, it appeared, had tied him up by the heels, and flogged him, from which period he dated his illness. He died, and the tendons of the mitral valve were found extensively ruptured, allowing the curtains to float back into the auricle at each systole*. Here the bruit was scarcely appreciable in front, but very intense in the dorsal region. This is the second lesion of this kind that I have witnessed: the first occurred during exertion in a sailor, in whom the tendons were brittle from disease. Sudden collapse and speedy death were the consequences.

Before concluding, I will briefly mention a connexion, *not accidental*, of adherent pericardium with mitral valve disease. When I first witnessed these pathological conditions, I was inclined to consider them accidental; but a multiplication of cases evincing the same diseases, induced me to alter my opinion; so that now, if I see an adherent pericardium of any standing, especially if complicated with hypertrophy, and at the same time I know, from the history of the patient, that there has been continued exercises, or any other cause liable to induce activity of the circulation, I confidently anticipate a more or less diseased condition of the mitral valve; excessively so when the cardiac lesion is the cause of death. In cases, however, where perfect repose has been maintained or necessitated by the existence of phthisis or any other chronic disease, and where at the same time the sanguification has been imperfect, the lesion will be comparatively small. In support of the assertion that it is a mere coincidence, I adduce the following facts:—

1. The marked frequency of the connexion.

2. The greater aptitude of the aortic valves, under other circumstances to assume diseased action.

3. The impress which the pericardium bears of disease of more ancient date.

The occasional or even frequent coincidence of diseased aortic valves by no means disproves the fact; it would be marvellous indeed if, with the severe endocarditis that generally accompanies pericarditis, it did not sometimes occur.

I can offer no other explanation of this than that, with adhesion of the pericardium, the axis from which the blood is propelled from the ventricles is altered, and instead of impinging against the aortic orifice, is propelled against the mitral curtains. The same reason would serve to explain why the aortic valves escape.

I am, sir,
Your obedient servant,
ALFRED ASPLAND.

Hackney, Aug. 28, 1838.

* This was the more interesting, as several cases are recorded of a similar lesion by continental physicians from similar causes.

EXPERIMENTS ON ANIMAL MAGNETISM.

To the Editor of the Medical Gazette.

SIR,

IN again intruding myself before the public on the subject of animal magnetism, I do so not with the intention of recording my opinion on this debateable point, or to offer any theoretical reasons why I believe or disbelieve in the reality of the supposed phenomena, but simply to record a few facts with which, in the progress of my inquiries on this subject, I have become acquainted. I feel myself called on to lay these facts before the public, as they will tend in a measure to counteract the erroneous impressions which have been produced on the minds of many professional men by the publication of the reports which have lately appeared in the Lancet. Another reason which induced me to enter again on this subject is, the use which at some future period may be made of these reports, if allowed to go uncontradicted; for although I am convinced that the present existence of animal magnetism, in this country, is fast drawing to a close, yet the history of science

almost forbids us to hope that it might not again be considered as an object of inquiry worthy of beings in full possession of their rational powers. Should such an event be ever brought about by one of those unfortunate retrogradings which sometimes occur in the march of truth, the facts recorded in the Lancet may, if allowed to go uncorrected, be triumphantly appealed to as a mass of experimental evidence in itself conclusive of the truth of these phenomena.

The experiments I shall here bring forward are some that have been conducted at various intervals during the last eight months. They were first undertaken in order to convince myself, if possible, of the truth or fallacy of the wonderful phenomena said to be produced by magnetic influence; for although *à priori* reasoning would have led me at once to reject them as groundless, yet such was not the course which would have been in accordance with the rules of philosophical inquiry, by which an investigation of nature should be conducted. As the phenomena presented by the magnetized patients were constantly varying, and the means which were sufficient on one occasion to detect the fallacy of these appearances could not be again had recourse to when the patients were once aware that they were made use of for that purpose, I was neeessarily obliged to be constantly varying the experiments. I shall endeavour in the following communication to bring forward some experiments which evidently shew the fallacious nature of each of the phenomena supposed to be produced by magnetic influence. Many such have been performed, and these have always tended to support the conclusions deduced from those related; but owing to the artfulness of the subjects practised upon, and their consummate acting, it was often with the utmost difficulty that sources of fallacy were avoided. The earlier experiments were many of them performed by myself; but, owing to the blind fanaticism with which this subject has of late been pursued, I have for some time been prevented from personally conducting any: it is to the kindness of my friends, therefore, that I am indebted for many which I shall bring forward. I can, however, vouch for their accuracy, as they were all performed by gentlemen who have already distinguished themselves in their professional studies, and who, after a most careful investigation of the subject, have been led to conclusions which are, I believe, in accordance with my own. I must apologize for the detail which I shall in many instances be led into, but which I consider neccssary, in order to render the experiments at all valuable, to shew the precautions which were taken to avoid sources of fallacy.

The first experiments I shall bring forward will relate to the production of sleep, the most constant and simple phenomenon which is supposed to be the result of magnetic manipulations. The most striking experiment which I can relate on this point, is one which was performed by Mr. Taylor, the apothecary to the University College Hospital. The girl O'Key, sen., was introduced into Mr. Taylor's room, and sent to sleep after three minutes' manipulations. She was then awakened, and some persons, as had been previously arranged, came to inform Mr. Taylor that he was wanted in some other part of the hospital: he left the room, requesting the girl O'Key to remain in the chair until he returned, which he said he would do in the course of a few minutes. When he had been absent a short time, he sent word to say that he should be unable to return for half an hour, but wishing that the girl might remain where she was until he came back; instead, however, of waiting half an hour, he followed into the room, with noiseless steps, the person who announced his detention, and immediately commenced magnetizing the girl, who was unaware of his presence, and who was kept in conversation by other persons in the room. For twenty-five minutes were the magnetic manipulation continued, without the slightest effect being produced, although these manipulations had been sufficient to send her to sleep in three minutes when she was aware of their being performed on her. Mr. Taylor now pretended to have just entered the room, and commenced magnetizing her, so that she should be aware of it, when, in the course of five minutes, the usual effects manifested themselves, she being sent into a profound sleep. This, I think, is sufficient to prove to every person that the production of this magnetic phenomenon depends on the patient knowing that these effects are anticipated.

The next experiment will be sufficient

to show that the reverse of this is true—viz. that the magnetic phenomena take place when the party supposes they are expected, although no magnetic power is used. The girl was placed in a chair, in a small ward in the hospital; and we made her think that it was our intention to magnetize her through the door, the chair being placed as close to it as possible. A person in the room was told to mark the time at which we rapped at the door, and also to observe the moment at which any magnetic phenomena might show themselves; we then retired from the room, and in a short time gave the preconcerted signal at the door. Having done this, we immediately retired to a distance of twenty or thirty yards from the door, so as to preclude the possibility of any influence being transmitted from us to the person in the room. After a quarter of an hour had elapsed, we found, on entering the apartment, that the girl was in a most drowsy state, the individual who had remained with her saying, that she seemed on the point of going to sleep. Nor was this the only effect produced by this non-existing cause, for we were told that five minutes after we had rapped at the door, the girl's head had been suddenly drawn back; and she stated that she felt a sensation similar to that which she experienced when magnetized. The drowsy state in which the patient was found, was supposed, by the believers in magnetism, to be occasioned by her having received a small dose of magnetic influence before she entered the room, and which, in this instance, had been particularly tardy in its operation. I do not know if any reasons were attempted to be given for the sensations and movements of the head. I shall not insult your readers by attempting to show the natural deductions from this experiment, and many others which I could bring forward to support it. Those who attended the magnetic exhibitions which took place some time since at the University College Hospital, will remember that on one occasion, at which Lord Brougham was present, at the suggestion of his Lordship the girls were not magnetized immediately on their being introduced into the exhibition-room, the same means being made use of to attract their attention as if they were being operated on. This was a proceeding to which the young ladies were not accustomed, and to the surprise of many and the satisfaction of some, the girls went to sleep as soon when they were not as when they were magnetized.

This will conclude the experiments relating to the magnetic sleep. I think they will be quite sufficient to justify our concluding that this part of the science has not the slightest foundation. The next class of phenomena on which I endeavoured to arrive at some experimental conclusions, is that relating to what is called *traction*. I have but to refer to the reports of the Lancet for many instances of the effects of this extraordinary power by which the patient was drawn either to the right or left, upwards or downwards, at the will of the magnetizer. The following example will perhaps afford a clue to the explanation of the greater part of the experiments on this subject, without the necessity of admitting any magnetic power. The girl O'Key, sen. was placed in a chair, she being in a state in which she was susceptible to traction. Mr. Wood then went behind her in such a position that it was impossible she could *see* him. On his moving to the right, Miss O'Key certainly gave evident proofs of her susceptibility to his manipulations; on his going to the left, the susceptible young lady could not avoid being drawn over to that side also; on his going directly backwards, she also fell in that direction; and, in short, she invariably followed the movements of the magnetizer. As I thought, however, that it was not impossible that Miss O'Key might be aware of his movements without admitting the necessity of her having eyes in her poll, I proposed the following modification of the manner of proceeding:—Mr. Wood and myself were to go directly behind her chair: he was to go in one direction, making use of all the magnetic influence he possessed, whilst I went towards the other side, making rather more noise with my boots in moving than Mr. Wood, whom I had requested to move as lightly as possible. As I had anticipated, the noise with the boots had a more powerful effect than all the magnetic power which my opponent exerted. Four times out of five were the efforts of Mr. Wood perfectly unavailing to draw her towards him, as in every instance she fell directly towards me, or in the direction from which the greatest

noise procceded: once only did she appear in the slightest degree affected by that power by which not a moment before she had been influenced, and on this occasion she fell in the diagonal between us. In fairness to the supporters of animal magnetism, I must relate the explanation they offered as to the cause of the want of success of these experiments, although the respect I feel for some of those who brought it forward would certainly lead me to adopt a contrary course.

The cause of failure was stated to be following; viz., that I being rather taller than the gentleman who exercised his powers of traction in an opposite direction, my greater mass was sufficient to draw the girl in the direction in which I went; and thus a few pounds of flesh and bone were thought sufficient to neutralize the whole magnetic power of which Mr. W. was capable; for, be it remembered, that although this gentleman was exerting all his powers of traction, I was not making use of the slightest magnetic effort, with the exception of the noise with my boots. It would appear that in offering the above explanation, those who brought it forward had entirely forgotten one or two facts which do not at all fall in with the principle here advanced. Supposing the mere mass of matter to exert so powerful an influence on the persons experimented on, I would ask, how can we account for their being drawn backwards in the exhibition-room by the power of a single person, although there are perhaps hundreds assembled in front? Besides, it would appear from this explanation that the mental effort exerted by Mr. Wood could have had no effect, although it is the general opinion of magnetists that it is principally on this metaphysical cause that the magnetic phenomena depend.

Your obedient servant,

JAMES BLAKE.

Gosport, August 31st, 1838.

MESMERISM.

POSTSCRIPT TO MR. TERRY'S LETTER.

To the Editor of the Medical Gazette

SIR,

WHEN, on the evening of the 22d August, I had finished reading over my very long letter on the subject of mesmerism, I certainly thought that I had said at least enough, indeed I was sorry that my observations had run on to so troublesome a length; but the next morning, happening to be engaged in a case which kept me several hours in the house, where there was no little choice of books, and finding GAZETTE, No. 46, still in my pocket, I entertained myself by looking through Mr. Mayo's paper again. On this occasion I noticed several statements which I have not alluded to in my last, and I request the favour of you to allow me very briefly to draw your attention to them now, as a sort of postscript to that letter. The close alliance of the two articles as they stand in the GAZETTE, immediately following each other, the one notes of a "Lusus Naturæ," the other "Further Observations on Mesmerism," in which a sort of "Lusus Mentis" certainly must have a great deal to do, is, to say the least of it, a singular coincidence. Were the statements in the latter case as clear and capable of demonstration as are those in the former, each author would then deserve our thanks.

The quantity of power vested in a mesmerized substance is said to depend upon the vigour of body and health of the person who mesmerized it: CASE:—Several patients in the wards of the North London Hospital hold the sovereign, and Jane O'Key, all the while (and, be it remembered, always for these experiments) in the state of delirium, or that of somnambulism, perceives no effect from it, till at last a poor little girl, affected with ringworm, takes the sovereign; then Jane O'Key taking it from her, is affected with trance. Where, I would ask, are the Governors of this hospital? Have they, as we have at the Northampton General Infirmary, weekly house-visitors, whose duty, amongst other things, it is to see that the patients are not improperly disturbed, and a weekly committee to hear their report, &c. &c.? It is remarkable how very conspicuous a part the two O'Keys take in all the mesmeric performances; not only in the experiments tried upon them "propriis personis," but even in the wards of the hospital their presence seems to be indispensable —a "sine quâ non."

The following is curious. It is stated that copper, together with some other substances, is not mesmerizable at all; yet that a halfpenny, held in the hand,

is capable of possessing and imparting powerful mesmeric influence, and this by reason of the moisture attached to it from the perspiration of the hand which holds it. Wipe off the perspiration, the charm is gone; the coin has lost its power, it is but copper still. It is worthy of remark how much perspiring has to do with mesmerism. One would have thought that by this time the O'Keys had ceased to experience this effect; one would have thought, judging from analogy and common reasoning, that it was chiefly the young beginners who would "sweat at it." With so much mental and bodily emotion, is there no fear, sir, of that awful scourge, the sweating sickness, revisiting our land; which we are told "once prevailed with a mighty slaughter, and the description of it as tremendous as that of the plague of Athens?"

Another remarkable case. Pewter is not mesmerizable at all; but Elizabeth O'Key holds a pewter porringer into which half-a-dozen mesmerized sovereigns are put; in a few minutes the trance supervenes, and she is falling rapidly into coma; the sovereigns are hastily taken out of the porringer, and just in time to prevent the coma being perfected; so she recovered from incipient coma, "first into a state of trance, and then into her ordinary state of delirium."

Non-mesmerizable substances not inoperative upon mesmerized patients. Case:—E. O'Key is thrown into a state of delirium from holding a mesmerized sovereign in her hand; it is difficult—it is impossible—to withdraw the sovereign from her grasp, for a mesmeric cramp has come on, rapidly contracting the muscles of her hand; and so long as the sovereign is held, so long the cramp continues; it defies the efficacy of all common palliative or curative treatment; and *even pointing four fingers* at her, it is stated, in this instance has lost its effect; this closed, contracted hand will not release its hold. What is to be done? The difficulty is great, the dilemma alarming! Oh, happy ingenuity! Try a non-mesmerizable substance: insert, for instance, a tin extinguisher into the patient's hand; the muscles are relaxed, the sovereign is set at liberty, and all is well, so beautifully, so exactly is the influence of the mesmerized metal neutralized by the non-mesmerized substance. This potent tin extinguisher! Oh! that such power were imparted to my pen! for then it would be my greatest boast, first to lay open this mysterious tale, and then extinguish.

Common physical principles, it may be presumed, are not allowed to have any effect in these uncommon instances, or else there does not seem any thing very extraordinary in the mechanical power of a circular wedge or cone being applied to dilate or expand a circular, a sort of sphincter-muscular, contraction. Another instance of the influence of a non-mesmerizable substance is the following:—Jane O'Key, recovering from profound coma, is very drowsy, and common means fail to arouse her; press a bit of tin against her forehead, she is alive, awake, and cheerful; remove the tin, she is drowsy again; apply it again, she is quite animated. Truly Mr. Mayo may affirm that in this marvellous inquiry "wonder succeeds wonder"!

The tale of looking at a stone mantelshelf I alluded to in my last letter, but I omitted the following, which, to say the least of it, is not less marvellous. Let Jane O'Key (of course in her usual delirium) hold out both her hands; do you look intently for a minute at one hand; tell her to shut that hand, and squeeze it hard, the desired mesmeric effects are forthwith observed; let her shut the hand you have not looked at, and no effect is produced; the perspiration upon the hand you looked at became mesmerized. Now, sir, upon upon this one little incident I should like to stake and to try the reality and the veracity of the whole affair. If Jane O'Key, with her *two eyes* well blinded (I will not mind about the eyes in her fingers), will tell me half-a dozen times together which hand I have looked at, on which hand I have thought of (for I take it for granted it is one and the same thing, a very trifling attribute in comparison with the powers of clairvoyance), then I will attach some reality to mesmerism. I will then believe there is really something new under the sun. A lady, with whom I was conversing upon this subject a few days ago, considered me illiberal in expressing such entire disbelief of matters I had never taken the trouble to witness or to investigate, adding that "Probably you would not believe that the blind could read, unless the evidence of their asylums incontestibly proved the fact." The fallacy of the argument was, of course, conspicuous. I could readily comprehend

that refined and improved sense of touch capable in many instances of supplying in some degree the place of sight, but I had not yet learnt that seeing and feeling could be considered as the same thing; and Mr. Mayo, in his account of Transposition of Sensation, says that the power imparted to the fingers was more nearly allied to sight than to either of our other senses. He could hardly say so if the sense of touch were that which he alluded to. A blind gentleman, with whom I am very intimate, has frequently played a rubber at whist in my house with more quickness and accuracy than either of his competitors. His cards, which he carries with him, are so very minutely marked by the point of a needle, that though I have often sat by him, I have never observed the marks; yet with the utmost quickness he sorts and plays his cards, the other parties of course announcing what card they have put down. All this, sir, is perfectly comprehensible, and it has nothing whatever in common with the absurd and dangerous delusions I have quoted above. We all know that as the attributes of the mind are, by cultivation, capable of unlimited improvement, so the common bodily sensations admit of high refinement; and we cannot be too thankful that the loss of one sense is so very generally, in some degree, made up for by the improved condition of another.

It is particularly worthy of remark, that, after all, this new light, mesmerism, is not brought forward for its curative effects upon disease, but upon its speculative bearings; not as a branch of therapeutical but of physiological science (and that physiology, be it remembered, strongly tainted with materialism); and its highest interest is displayed in the phenomena of prevision, or prophecy, and the transposition of sensation. If the indulgence or gratification of certain speculative opinions respecting the nervous system is (as is here confessed) the chief purpose of the investigation, I must say that I think the parties concerned are unpardonable in carrying on those inquiries upon their fellow-creatures. All other physiologists, I believe, when indulging in physiological hypotheses, have invariably used the lower orders of creation for their experiments, the analogy of the nervous systems in the higher and lower classes of animals being near enough for all really useful purposes.

Mr. Mayo's paper concludes with an allusion to Homer, Shakspeare, and Scott: all the great observers of mankind and knowers of human nature have represented the prophetic spirit as occasionally exhibiting itself a little before death. With respect to these opinions, I believe them to be more elegantly classical and superstitious than substantially true; and I must further state, that during my time, which is now closely approaching to half a century, I have never known or met with any perfectly, or even imperfectly, authenticated instances of "communications strangely felt rather than made, of the time of the dissolution of an absent friend." Certainly I should think it probable, if I had met with such instances, that they might have something in common with the "source of the wonders of mesmerism."

I have the honour to be, sir,

Your obedient servant,

H. Terry,

Surgeon to the Northampton General Infirmary.

Northampton, August 24, 1838.

P.S.—I have delayed sending the above until I had seen the Gazette, No. 48. I perceive my letter announced for next week, and I am glad to find the number of openly avowed opponents to mesmerism rapidly increasing.

H. Terry.

August 30, 1838.

CALCULI IN BLADDER.

To the Editor of the Medical Gazette.

Sir,

If you think the following case worthy a corner in your valuable journal, by inserting it you will oblige

Your obedient servant,

J. F. Harding, M.R.C.S.

13, Spencer Street,
Northampton Square, Aug. 15, 1838.

In August 1837, I was consulted by Mr. W., a retired hair-dresser, æt. 81, he having suffered for some time with severe pain, arising, as he supposed, from gravel; but the various symptoms of which he complained convinced me he was labouring under the effects of cal-

culus in the bladder, of which I wished to be satisfied by sounding; but he strongly objected to any examination whatever, not even the passing of a catheter. Finding his determination to have no operation, even if I had discovered a calculus to exist in the bladder, I tested the urine, and found it decidedly acid.

I commenced giving him the liq. potassæ with hyoscyamus, alternately with soda and morphia, which greatly relieved him for some months; at the expiration of which period he again applied to me, the symptoms having again returned. I then administered the same remedies with the like results. But old age, with the malady from which he was suffering, gradually undermining his general health, he sunk about nine months from my first seeing him.

By the permission of his friends I was allowed a post-mortem examination. In the bladder I found, to my surprise, fifty calculi of various sizes, several of which were the shells of a large one which had been broken down either by being acted on by the alkaline remedies, or by friction, the bladder being much contracted, and the muscular fibres greatly thickened, but no ulceration or other disease of that viscus. The kidneys were atrophied, particularly the the left; at its fundus a large cyst, which communicated with its pelvis, prevented the urine escaping. The ureters were dilated and thinned. All the other viscera in a normal state.

Probably some of your readers would give an opinion as regards the calculi being so broken down—whether, as I have hinted, by the action of the alkaline medicines, or from the action of the contracted bladder, as the muscular bands were greatly hypertrophied. The calculi were so much acted on, as almost to leave no traces of the shells having been separated, appearing rather as separate calculi, consisting of phosphate of lime, and weighing about three ounces avoirdupois.

He had been a most active and temperate man. Even within two months of his decease he was enabled to ride or walk with but little inconvenience.

ON THE SECALE CORNUTUM AND COLCHICUM IN PARTURITION.

To the Editor of the Medical Gazette.

SIR,

I OBSERVE, in your last week's number, some remarks made by Messrs. Armstrong and Cory, in relation to the circumstances which should direct us in the employment of the secale cornutum. Possibly you will allow me briefly to state that which has fallen under my own observation, respecting the utility of the remedy in the cases alluded to by your correspondents; and this object I would wish to attain simply by stating *facts*, and trust, that should any "established rule," or weight of high authority, be violated by such a mode as I have adopted, I may be allowed to offer, in extenuation, an appeal more to a practical than theoretical disquisition on the subject. Not, then, to trespass long on the pages of your excellent journal, I am enabled to state, that in the last thirty cases of labour which I have attended, and where the orifice of the uterus has *not* been dilated to more than the circumference of a shilling, and where the pains were pretty regular in succession, the exhibition of the fresh powdered ergot of rye has had the most beneficial effects in forwarding the process of parturition. I have given this substance also where the secretion from the mucous membrane has sometimes been copious, and *vice versâ*, or in what is usually denominated a "*dry labour*;" in both of which it has been equally effective. I usually carry a bottle containing the secale with me, and at first give a tea-spoonful in some gruel, and repeat it (if necessary) after the expiration of twenty minutes, when *two* tea-spoonfuls are given; and, on a third dose becoming necessary, three tea-spoonfuls. It might be argued, *à priori*, that the large doses here prescribed might have a baneful tendency, rather than promote the desired end; the event, however, has proved to me the reverse is the case. The secale, in some instances, cannot be retained even when scruple doses only are given. When this occurs, it is useless to repeat it. Whether the effect of vomiting is to be overcome by exhibiting it in coffee, as Mr. Cory advises, I have had no means of determining.

I can add my humble testimony, likewise, in favour of its utility in active cases of hæmorrhage, as it occurs either as a consequence of parturition, or before impregnation has taken place, and there are no *general rules*, that I know of, to be enforced in such cases. In what is termed menorrhagia, in young unmarried women (a state dependent on causes often inexplicable), fifteen grains of the powder every three hours have been of more essential service than any remedy tried. With regard to the *modus operandi* of the secale, both Mr. Armstrong and Mr. Cory differ in opinion: the former gentleman considers the spasmodic contractions of the uterus are induced in a *secondary* manner; the latter stating his belief that the contrary takes place. It appears to me the contention on either side is more about words than substance. No evidence can be adduced to prove whether one muscle is primarily influenced more than another; and it is better to confess an ignorance, than attempt an explanation on such points, unless they can be tolerably well substantiated; and our present knowledge has not arrived at such a state of certainty as to enable us to speak positively on any subject which cannot be based on a surer foundation than mere conjecture or hypothetical reasoning. There are other articles in the materia medica to which the same train of reasoning applies with equal force.

Innovations of any kind upon our already most prolific catalogue of drugs may well be regarded with suspicion; yet when practical evidence can be brought forward to support them, an investigation into their merits is fairly due. Conceiving some slight analogy existed between rheumatism, *ligamentous* inflammation, and some of the characters or symptoms attendant on the immediate commencement of labour, (the uterus, a muscular organ, being probably at such a period in a state of inflammation also, however slight in degree,) I thought it possible that colchicum might be advantageously employed, in the same manner as the secale, in this latter state, and consequently made trial of it in four recent cases; eight grains of the powdered root were given at the first dose, and after waiting the usual time, twenty minutes, no effect having been produced on the patient, it was repeated, and within a few minutes the pains became very violent and forcing; indeed, the effects appeared the same as those usually produced by the ergot of rye; I say *appeared*, not being anxious to state that such a result was decidedly affected by the means employed; but the influence seemed, in some measure, plausible, more especially as the other three cases terminated equally satisfactory. The subject appears worthy of further inquiry; not, however, with a view of superseding the use of the other remedy, whose value cannot be too highly appreciated. Were I to venture to speculate on the *modus operandi* of the colchicum, I should hazard the opinion, or rather idea, that its primary effect consisted in disturbing the functions of the stomach in some peculiar, though unintelligible manner; and, knowing how great a sympathy exists between this organ and the uterus, that the same kind of action or influence was communicated to it, thus stimulating or inducing a greater disposition to contraction of its muscular parietes. Whether the secale acts on the same principle, I know not; but a question here naturally presents itself—how can colchicum, which is administered in rheumatic cases, and frequently relieves painful feelings or sensations in the parts affected, actually accomplish effects so completely opposed — namely, create increased painful contractions of the uterus? The phenomenon I do not pretend to solve, and therefore leave the matter to the consideration of your readers, premising, that however fanciful the idea may appear—that of associating rheumatism and the stages of parturition, yet I would wish to imply by such phraseology, that there is observable one leading feature in both—viz. pain of a peculiar and intermittent character; and hence my principal inducement in making trial of the colchicum.—I am, sir,

Your obedient servant,

J. B. Clutterbuck.

5, Charterhouse Square,
Sept. 4, 1838.

MEDICAL GAZETTE.

Saturday, September 8, 1838.

" Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

PROGRESS OF ANIMAL MAGNETISM.

THE papers published in the present and in some preceding numbers of our journal, will have shewn clearly enough that animal magnetism is running exactly its usual course. At first the slighter effects are produced, and witnesses of them doubt whether they are the result of some known cause or of some new influence; but greater wonders spring up, and then the investigations assume a more stringent character, previous fallacies are detected, and the influence of the imagination or of gross deception is proved in the whole series of observations.

As we expressed our opinions on the subject somewhat boldly before it began to be investigated in England, we are anxious to present our readers with some of the results of the evidence which has been since accumulated, that they may see how far we are borne out in the opinion which we formed from its previous history. We shall consider especially the statements which have been authenticated in our own pages. Now if the accounts lately given by those who are favourable to mesmerism are entirely and strictly true, not only are the phenomena they detail new and inexplicable, except as the results of some new agent, but they exceed in wonder any that have hitherto been exhibited. But here at once, in the question of their truth, is the issue joined: we find evidence sufficient to convince us that the observations have been incorrectly made, and that sources of fallacy have in no case been entirely excluded, without a proof being obtained that the conclusions previously drawn were hasty and unjust.

The precautions taken by the magnetizers to avoid deception, are always *asserted* but never *detailed*. Thus Mr. Mayo says (p. 773), " for the sake of brevity, I do not mention the precautions taken to prevent the patient, in this and in other experiments, *guessing when the effect is to take place and when it is not;*" but " *I am certain* the patient was not aware of the result which was expected to follow, and which did not fail to follow;" and others pledge their *certain belief* to the same effect. But in contradiction to their convictions, we have several plain and detailed accounts of precautions which they did not take, but which were sufficient to detect fallacies in the very same experiments as those from which they deduced the most positive and important conclusions. Let us take some examples.

It was supposed that one of the girls could detect the difference between magnetized and unmagnetized water; the former producing in her the state of sleep, the latter no effect. The experiment was made by dipping sticks in each kind of water, and passing them through the lips; and every trial seemed to confirm the belief that one produced sleep and the other not. But Mr. Blake (p. 645) thought he observed, that in the one case the stick was held carefully and anxiously between the lips, as if " results were evidently looked for;" while, when it had been dipped in unmagnetized water, " it was carelessly passed through the lips." He tested this belief, and " succeeded on many occasions in sending the girl to sleep when using unmagnetized water, and inserting the stick in her mouth in such a manner as if he expected results would be produced;" and, on the other hand, he found a " frequent

absence of any effect when magnetized water was applied in an indifferent manner."

However, an explanation was offered of this evident proof of the patient's *guessing* what was expected, with that marvellous facility of explaining away facts, which has so characterized one side of this inquiry. It was said that the sticks themselves might have become magnetic by being held in magnetic hands. But suppose they had, why then did they both before and after this explanation fail of producing magnetic effects? If the sticks were magnetized, then whatever water was used, sleep should have followed; but if they were not, then why did unmagnetized water often succeed, and magnetized water often fail, in producing sleep? The conclusion is inevitable; the sticks and the water were alike powerless, except as instruments from which to learn "the result which was expected to follow."

Again, it is said, *and the girl is told*, that she can discern a magnetized from an unmagnetized sovereign. Several are taken from a gentleman's pocket, and laid on a table (see p. 846). She took them up one after another, but none produced any effect. She was taken out of the room, and on her return several of the same sovereigns produced the trance, *though none of them had been touched*. An explanation as illogical as ever was attempted for this anomaly; and from this and similar experiments, though failures were frequent, a system of laws was made out in regard to the mesmeric influence on metals, and its communication from some of them to the body, and propounded in terms more definite and full of confidence than those in which the laws of any vital science yet known can be stated. One of these was, that nickel was peculiarly mesmerizable, and would produce more violent magnetic effects than any other metal; while lead, on the contrary was incapable of receiving the least magnetic influence; yet in our last number (p. 908) we published an account of experiments in which Mr. Wakley obtained the most violent magnetic effects with lead, while nickel was perfectly inert; and why? Because when lead was used the girl thought it was nickel, and when nickel was employed she believed it was lead. To explain some of the numerous failures which occurred in this part of the experiments, the influence of looking at an object is had recourse to. For example, the girl is magnetized by a sovereign which had never been touched. No inquiry is made to discover whether she *imagined* that it had been magnetized; but it is at once said that it had become magnetic by being looked at! and without attempting to explain why it was that all the others, which had been just as narrowly watched, had not become magnetic too; or why afterwards, when all eyes were just as intently fixed on every coin with which the experiment was made, none of these produced sleep—without further precaution, another code of laws is drawn up, relating to the conveyance of magnetic influence by emanation from the eyes.

Of course, the phenomena of clairvoyance must be exhibited by one of these girls, and before resigning his "fostering offices towards mesmerism," Mr. Mayo says, that he has "seen her, on three occasions, distinctly perceive some objects placed a few inches from her, through a sense more akin to vision than to any other, which has been suddenly developed in her hand." (P. 774.) He does not describe the phenomenon, but Mr. Wooldridge does; and after detailing a case in which the experiment seemed to succeed, he says (p. 848,) "we took another piece of

bread and placed it near the right, the non-seeing, hand, having previously touched the left hand with it, to make her believe it to be held near that. After a very short time the (left) hand began to move. We whispered, "she begins to see it;" presently she made a clutch, and of course found nothing. The experiment was repeated, in the same manner, three times more, and with the same results." Thus, then, the phenomenon of vision exercised by the skin, *does* occur according as the patient "*guesses when it is to take place and when it is not.*"

But let us pass from these, the higher departments of the art, to some of its more simple effects, in which the difficulty of detecting fallacies seemed to be greater. For instances of the magnetic sleep, its first and slightest effect, we need only refer to the experiments by Mr. Taylor and Mr. Blake (p. 940, of the present number) to prove clearly how that follows or not, according to the imagination; and to the case detailed by Mr. Blake (p. 647) of a gentleman who, under pretence of removing a pimple from her nose, magnetized the girl O'Key for a quarter of an hour, without producing the least effect, because she was only thinking of the pimple; yet both before and afterwards a single pass of the hand was sufficient to produce coma, provided the patient thought it ought to follow.

Now, we do not care to decide whether these girls are shameless impostors, or are themselves the dupes of their own imaginations; and we entirely acquit all those who have declared themselves in favour of mesmerism, of any intention to deceive others, or to promulgate what they do not entirely believe themselves; though we think that if, instead of pledging themselves to its truth, and engaging both their heads and hearts in increasing its seeming wonders, they had fairly and honestly investigated the sources of fallacy, which others have, in spite of them, detected, they would have arrived at very different conclusions. It is enough for us to find that the opinions which we gave just twelve months since, have been fully confirmed, and that there still "is not a single credible effect of animal magnetism which is not referable to the influence of agents long since known and acknowledged as imagination and imitation."

Indeed, the evidence by which this opinion is confirmed, is so exactly parallel to that on which it was based, that we must allude to some of the latter. Place, for instance, the following cases by the side of Mr. Taylor's and Mr. Blake's experiments; they are abstracted from the Report of the First Commission, of which Franklin, Lavoisier, and Bailly, were members. They took one of M. Deslon's best patients, and having bandaged her eyes, persuaded her that he was in the room, and magnetizing her; in three minutes the crisis was complete. They persuaded another that M. Deslon was magnetizing her on the opposite side of the room-door; in three minutes she fell into a complete crisis. A girl at Passy (at Franklin's house) had been often thrown into the state of crisis in three minutes, whenever she knew or thought she was being magnetized; and this, whatever obstacle was placed between her and the magnetizer. One day, while she was engaged in conversation, she was unconsciously magnetized through a paper partition for half an hour, and no effect whatever was produced. The magnetizer then came from behind the screen, and manipulated openly before her, and in twelve minutes she was in convulsions. They concluded, from these and many similar experiments which they instituted on purpose to determine the influence of

imagination and other sources of fallacy that "imagination without magnetizing produces convulsions—magnetism without imagination produces nothing." Such, too, was M. Bertrand's opinion, many of whose cases are the very analogues of those which we have recently published; and the same sentence may still serve as the basis for an honest and impartial opinion on all the evidence that has since been accumulated.

With regard to Mr. Wakley's experiments, we give him full credit for the manner in which they were conducted, and we perfectly concur in the inferences he draws from them, they being just what we have maintained with regard to animal magnetism since the subject was broached, twelve months ago. We cannot, however, concur on one point, and that is the turn he gives the subject with reference to the individuals who have busied themselves about it. He endeavours to create "a diversion" in Dr. Elliotson's favour by turning the laugh against Mr. Mayo. For ourselves, we really cannot venture an opinion as to which of these gentlemen appears in the more ludicrous position, and willingly leave that delicate point for others to decide. But there is an entirely different aspect in which the subject may be viewed, and that is with reference to the hospital and the school at which these magnetic exhibitions have been made. We ask, if the same thing had occurred elsewhere—if two young women had been kept for weeks and months in any other hospital in London, as subjects of private experiment and public exhibition, would our contemporary have failed to denounce such proceeding? Is there any language he would have deemed too strong in pouring forth the phials of his wrath? Would not an appeal have been made to the governors and to the public? Would it not have been used as an argument in favour of the immediate interference of the legislature in the management of our hospitals? We need not "pause for a reply:" every man of common sense knows and feels that it would have been so.

Then, again, as to the school of medicine. Suppose such a system of folly and humbug had been enacted at Guy's, or Bartholomew's, or St. George's, by way of instructing pupils in the practice of physic, would there have been any limit to the sarcasms of the honourable member, against the "neveys and noodles" of those institutions? Would not the cautions to young men and their friends against wasting their time and unsettling their minds by such pursuits, have been unceasing? Yet not a syllable of the kind has ever been breathed by him upon the subject—and why? On the contrary, the bubble has been blown to its present, or rather its recent magnitude, in great measure by the fostering breath of the Lancet, in which the proceedings at University College Hospital have been reported, with the most solemn gravity, as important contributions to medical science.

Magnetism has now lived a year in England: we scarcely expected that it would survive so long; and we cannot envy those who have nursed it to meet so ignominious a death as is now evidently impending. Once buried, may we not hope that any attempts to revive it will be treated as, according to M. Husson all communications to the Institute on the quadrature of the circle are,—put aside without examination?

GLASGOW EYE INFIRMARY.

Clinical Lectures by Dr. Mackenzie.

September 1st, 1838.

Lecture V.

Traumatic Ophthalmia.—Staphyloma of the Cornea and Iris.—Ophthalmitis Phlegmonosa.

As one of the most interesting cases now under treatment, I may select that of John

Fletcher (No. 8513). In him, we have an example of severe traumatic ophthalmia, ending in staphyloma of the cornea and iris, and latterly an instance of ophthalmitis phlegmonosa, or ocular phlegmon.

The cases most deserving of attention in a clinical course are not always the rarest, but often the most common; not always the most successfully treated, but often those which most severely tax the skill of the practitioner, or baffle perhaps his most careful attempts to alleviate the symptoms. This has been unfortunately the fact with regard to Fletcher. Not merely was the sight of the injured eye lost by the original accident, but a series of distressing consequences has followed, worthy of your earnest attention.

A stone-hewer, aged 73 years; he was taken on the list on the 19th May, 1838, with violent traumatic ophthalmia of right eye, the consequence of a stroke with a bit of whin-stone on the 15th. Nothing had been done for the eye before he came here. He had shewn it on the morning of the 19th to a quack, who thought it so bad that he would not touch it. There was much swelling and redness of the conjunctiva; the centre of the cornea was opaque, and a small quantity of pus is described as lying at the bottom of the anterior chamber; the vision of the eye was much impaired, and he had severe pain, especially towards the inner angle of the eye.

The treatment consisted in V.S., frequent leeching of the eyelids, blisters, mercurial purges, and calomel with opium.

On the 30th, the symptoms do not appear to have moderated, for the report states the pain to continue, and notices again the effusion of pus into the anterior chamber. V.S. was repeated, and the calomel with opium continued. He was afterwards leeched and purged, but seemingly with little or no effect, for on the 4th June the whole anterior chamber (cornea?) is reported to be filled with pus, and the pain to continue.

On the 6th, the pain is said to be relieved, but the cornea sloughy. On the 10th, the inflammation diminishes. He still continues to be leeched and blistered. On the 16th, the calomel with opium is stopped, and he is ordered two grains of sulphate of quina thrice a day, and this seems to have been continued till the 31st July.

The reports are not so minute as might be wished; but they suffice to shew that the violent inflammation which existed when he applied here on the 19th May abated after the cornea had become disorganized, and the eye reduced to a staphylomatous state.

When he came under my care, at the beginning of August, we found the cornea and iris completely staphylomatous, the staphyloma being rather of a conical form, and not very protuberant. He still complained greatly of pain in the eye. He was ordered to use the red precipitate salve every morning, the four grain solution of nitrate of silver was dropped on the eye once a day, and he had a collyrium with vinum belladonnæ.

You had an opportunity, then, of witnessing in this patient the rise and progress of a total staphyloma of the cornea and iris—a state which is never produced, except when the whole or a part (generally the greater part) of the cornea has been destroyed by abscess and ulceration, and the iris, protruding, has become covered by a pseudo-cornea, or opaque cicatrice, to which it is completely adherent. In fact, a *staphyloma corneæ et iridis*, as it is technically termed, is nothing else than a cicatrice, spread over the iris, and adhering to it, occupying the place and presenting somewhat of the form of the cornea, but totally opaque, and generally much thicker and denser than the natural cornea; often, indeed, of a consistence almost cartilaginous. A staphyloma of the cornea and iris is not a degeneration of the old cornea, not an opaque and thickened state of those textures which formerly were thin and transparent, but it is a new and morbid product, occupying the place of the cornea, which had been destroyed by abscess and ulcer. This holds, whether the staphyloma is partial or total, whether a part only or the whole cornea has been destroyed; and to the inner surface of the pseudo-cornea the iris is always adherent. A mere opacity of the cornea, in consequence of inflammation, never produces staphyloma. The iris sometimes adheres to the internal surface of the cornea, from inflammation; but neither does this *anterior synechia*, as it is termed, ever end in staphyloma, unless part of the cornea has actually given way and been destroyed. It is not adhesion of the iris to the cornea, but it is a covering up of the exposed iris by a new substance altogether, which lays the foundation for staphyloma, and constitutes the essential character of this disease. This is a point which has been illustrated in a masterly manner, and its truth established beyond controversy, by Mr. Wharton Jones, in a paper on Staphyloma, in the Medical Gazette, vol. xxi. p. 847.

In the state, then, of a total staphyloma of the cornea and iris, we found Fletcher's right eye in the beginning of August. He complained much more of pain than is general after a staphyloma has completely formed; and it occurred to me as not unlikely that the fragment of stone which

had produced the original injury, might still be within the ball of the eye, and cause the pain which produced so much distress. His own notion was that the splinter of whin-stone had gone into his eye, and had never been extracted. On the 16th I divided the staphyloma by a semilunar incision with the extraction knife, and ordered a warm poultice to be applied ever the eyelids, in the hope that if any fragment of stone were lodged within the eye, it would make its exit through the incision, and thus the suffering be relieved which our patient was labouring under.

17th—Says his eye is much easier; incision is keeping open, but nothing particular has been discharged from it.

Poultice continued.

18th.—On introducing a probe through incision, a quantity of thin fluid was discharged, but no foreign body.

20th.—The crystalline has just escaped through incision, followed by a considerable quantity of vitreous humour.

Poultice continued.

22d.—Yesterday a quantity of matter was evacuated through the cornea. Today the eye-ball is protruded, and the conjunctiva chemosed. The eyelids are of a dark-red colour and swollen, and he complains of great pain in the parts, with a feeling as if they were suppurating. Has had four grains of opium since the visit yesterday. Eight leeches were applied to the eyelids, and he had an ounce of castor-oil. The eye had now assumed the appearance of that violently inflamed state which is termed *ophthalmitis phlegmonosa*, or *ocular phlegmon*.

23d.—A better night. Swelling and protrusion of eye not abated: it presents a whitish colour on temporal side of sclerotica. A lancet being passed through this part, nothing was discharged but blood.

Poultice continued.

24th.—A pretty good night. Swelling of eye not reduced; it is discharging some thick white matter from the first incison.

25th. — Swelling of eye rather less; pulse 72.

26th.—Considerable purulent discharge from cornea. More pain and less sleep. Pulse 70, irregular.

Draught with 30 drops of Tr. Opii.
Poultice continued.

27th. — A good night. Ophthalmitis subsides. Considerable purulent discharge.

Draught and poultice continued.

28th.—An ounce of castor-oil. Draught and poultice continued.

29th.—Draught increased to 35 drops.

30th.—Another good night. Swelling of eye-ball rather less. A considerable discharge of pus from cornea. Can move the eye better.

Castor-oil repeated. Draught and poultice continued.

31st.—Symptoms of ophthalmitis continue to subside; tongue clean.

September 1st. — Swelling of eye-ball, conjunctiva, and eyelids, considerably reduced since yesterday.

Draught and poultice continued.

Such is the history of a case of ophthalmitis induced by opening a staphyloma. Such an inflammation of the eye is dangerous even to the life of the patient; and I rejoice, therefore, to think that Fletcher is now making daily progress towards recovery. From his age and other causes, and especially the open state of the eye, we did not require to use the same vigorous treatment which ophthalmitis generally demands.

Ophthalmitis, or ocular phlegmon, is a formidable disease, which attacks the whole of the eye-ball. When it affects an eye previously sound, it almost always produces destruction of vision, and not unfrequently it proves fatal. All the tissues, internal and external, are inflamed, in ophthalmitis: even the eyelids and the orbital cellular membrane are affected. The inflammation, however, of the internal tissues is the more intense, and hence the names *ophthalmia interna* and *retinitis*, which have been bestowed upon it.

The chief causes which have been observed to produce ophthalmitis are twofold, viz., external injuries and phlebitis.

1. For example, ophthalmitis sometimes occurs after the operations for cataract, and especially after extraction. Dr. Rognetta, who gives an excellent account of this disease (Lancette Française, 9 Fev. 1837,) says, he has seen it most frequently after depression. Other injuries of the eye, or of the orbital contents, may be followed by the same effect, of which examples are recorded by Percy, Guthrie, and others. It happens sometimes with such injuries, as with those of the fingers or hand, that a small prick is sufficient, in certain states of the general health, to provoke the most serious reaction.

2. Dr. Rognetta has altogether overlooked the absorption of pus in phlebitis, as one of the causes of ophthalmitis. Of the reality of this case there can be no doubt. I have seen it in erysipelas, or diffuse cellular inflammation. In one instance of this sort, which I visited along with two medical friends of this town, both eyes became affected, and the case ended in total blindness. In another in-

stance I saw this disease terminate fatally, under circumstances which led me to believe that it arose from uterine phlebitis, after parturition. Within a few weeks, two fatal cases occurred in 1836, one in the Royal Infirmary, under the care of Dr. Cowan; and the other in this institution, under that of Dr. Rainy. In Dr. Cowan's case the disease followed scarlatina; and in both, the suspicion was, that the cause was inflammation of the veins. Certainly, these two fatal cases did not arise from any external injury, but from some internal constitutional disturbance.

3. I have repeatedly seen staphylomatous eyes become affected with ophthalmitis; sometimes from a blow, in other cases from cold. This set of cases, as well as the first set, are not so dangerous as when the complaint arises from phlebitis, or from some hidden constitutional cause.

4. Morbillous ophthalmia, variolous ophthalmia, and the puro-mucous ophthalmiæ, are apt, though not very frequently, to terminate in a state in which the whole tissues of the eye are affected with phlegmonous reaction.

Such being some of the causes of ophthalmitis, we may (with Dr. Rognetta) distinguish three stages in its progress.

1. The stage of pyropsia, extending from the beginning of the disease till the epoch when the retina becomes insensible, and the eye ceases to perceive light, or to be photophobic. 2. The stage of suppuration, counting from the cessation of photophobia. 3. The stage of spontaneous rupture.

The disease begins slowly and insidiously in some cases; suddenly and severely in others. In constitutional cases, one eye or both may be affected. In traumatic cases it rarely, if ever, spreads to the eye which has not been injured. Sometimes it commences in the ocular conjunctiva; at other times it proceeds from the orbital tissues. In many cases, and especially when it springs from phlebitis, the retina appears to be the focus of the disease.

Pulsatory pain (exactly as in the most intense panaris or whitloe) is felt in the bottom of the eye and orbit, spreading to the forehead and temple, and accompanied with a sensation of burning heat, tension, and fulness, as if the eye could not be contained within the orbit.

The patient complains of photophobia to a great degree, with pyropsia, that is, flaming and shining vision. The smallest ray of light occasions the most severe feeling of intolerance. This symptom occurs chiefly in the commencement; by and by it totally subsides, the retina having become insensible from the effects of the inflammation on its substance, or from the compression arising from the fluids extravasated from the surrounding textures.

There is phlegmonous swelling of the eye, the intra-orbital tissues, and the eyelids. The eyeball is hard, and incompressible to touch, and is pushed almost completely out of the orbit. From the fibrous texture of the shell of the eyeball, we might think it incapable of extension; and no doubt it is from its yielding so little that the severity of the pain arises which attends ophthalmitis. Still it does yield, and the eyeball is distended and increased in volume and contents. Much less so, however, than its state of extreme protrusion, and its pressure against the eyelids, might lead you to suppose.

At first, the conjunctiva is rather œdematous than very red. The aqueous humour is sanguinolent; the iris changed in colour; the pupil contracted; the fundus oculi reddish or greenish. There is an involuntary fixedness of the eyeball, the state of pain and swelling rendering the common movements of the eye almost impossible.

The constitutional symptoms are of variable intensity; they are rigors, fever, insomnia, anxiety, delirium, sometimes convulsions.

Ophthalmitis has different terminations:—

1. It terminates in amaurosis, as it did in the case I have already referred to as arising from erysipelas of one of the upper extremities. The eye-ball may remain almost natural, or it may become atrophic. The capsule of the lens is left opaque, and the pupil small and adherent.

2. Ophthalmitis terminates in suppuration and rupture of the eye. The cornea infiltrates with pus, and becomes ulcerated. Only once have I seen the cornea actually affected with gangrene, and fall off in a distinct slough. That was a case which occurred in this infirmary, in a young man, in whom I passed a needle through the sclerotica, and gathering together the remains of a congenital cataract, pushed them through the pupil into the anterior chamber, and extracted them through a quarter section of the cornea. Dreadful inflammation and swelling followed the operation, which had been performed without the least difficulty, and with the smallest possible apparent irritation; and the result was gangrene of the cornea, which separated as a slough, resembling a bit of moistened white leather. Sloughing of the cornea is often spoken of by authors. Dr. Rognetta says he could cite cases of this sort by the hundred; but I have never seen gangrene of the cornea except in that one case. Onyx or abscess of the cornea, and destruction by ulcera-

tion, are confounded by many with gangrene.

3. Ophthalmitis terminates by death. This termination would certainly happen more frequently, if the eye did not burst spontaneously. The kind of relaxation which results from the bursting of the eye diminishes the violence of the inflammation, and the disease subsides, as does a whitloe treated by a deep incision. On the other hand, the inflammatory action, if unsubdued, is propagated to the brain or its membranes, and ends in fatal coma.

The danger of losing the eye, and of the disease spreading to the membranes of the brain, must render your prognosis always extremely guarded. According to the intensity of the symptoms, and their seeming tendency to such or such a termination, the friends must be warned of the dangerous, or very dangerous, state in which the patient is.

The symptoms of ophthalmitis may subside, as they have done in Fletcher, without any very severe measures; but, in general, the most prompt and energetic plan of cure is necessary.

In the first stage reiterated venesection from the arm is the chief remedy; and the vein should be re-opened as often as the hardness of the pulse points out the necessity for further depletion. Dr. Rognetta has great confidence in tartrate of antimony. He dissolves six grains in six ounces of water, with thirty drops of laudanum, and gives a table-spoonful every half-hour. Abstinence must be enjoined, and diluent drinks administered. Leeches are to be applied, or the neck and temples may be cupped and scarified. Cold compresses should be constantly kept over the eye-lids. Mustard baths of the feet are likely to be useful during the first stage, and even blistering the neck may be employed.

In the second stage, the products of inflammation are pent up within the eye-ball, and cannot be discharged. Paracentesis oculi, that is, puncturing the cornea, so as to evacuate the aqueous humour, or puncturing the sclerotica, if vision is judged to be extinct, is to be had recourse to. By puncturing the sclerotica there is perhaps very little discharged, but the tension is taken off, and the danger is set aside of the disease spreading to the brain. The form of the eye may be preserved by these means, and suppuration prevented. I have seen this treatment save apparently the life of the patient.

Should the disease go on to suppuration, the matter escapes by the aperture already made in the cornea, and the eye sinks; or if the wound of the cornea has healed, it is to be opened again. A warm poultice should be laid over the eye-lids.

During the second stage, the antiphlogistic treatment recommended for the first is still to be more or less adhered to.

In the third stage, the eye having been emptied, either spontaneously or by the knife, the parts remain for a time swollen and painful; but as the suppuration goes on, the swelling of the tissues fall, as you have seen in Fletcher, and the remains of the eye-ball shrink. Some slightly astringent fomentation may now be used. It is proper to examine the internal surface of the eye-lids from time to time, to see that no symblepharon forms, which might afterwards prevent the use of a glass eye.

It was my intention to have detailed to you several unpublished cases of ophthalmitis, as well as to have laid before you an account of the appearances on dissection in two fatal instances, the preparations belonging to which, with wax models of the same, are on the table before us; but our time having elapsed, we must defer this till another opportunity.

WESTMINSTER HOSPITAL.

Stone in the Bladder—New Staff Lateral Section—Inconvenience of the Staff, and danger of the operation.

Wm. Wenham, æt. 9, a healthy boy, though a native of London, was admitted into the hospital some time in the latter part of last year, having unambiguous symptoms of stone in the bladder. The irritation of this organ was unusually great, and the boy underwent a good deal of treatment before it was deemed expedient to subject him to lithotomy. Mr. Guthrie, whose patient he was, had a staff of a peculiar form constructed to suit the boy's urethra. This instrument resembled in figure a magnified boot-hook. This was not easy to be introduced into the bladder, but its advantages counterbalanced, in the mind of the surgeon, this inconvenience. When passed in the customary way, and retained in the urethra, its most salient angle made a considerable projection in the perineum.

After several postponements, the operation was fixed for the 17th February, 1838; when it was performed as follows, in the presence of the hospital surgeons, and a large company of medical practitioners and students:—

The patient was placed on the table, and secured in the usual way. The newly-invented staff was introduced with little difficulty. Mr. Guthrie made the ordinary incision in the perineum, but lower than usual, and thinking he had reached the groove of the staff, pushed forward his knife apparently along the groove to its termination.

The operator now at one effort impelled forward the staff and the scalpel, but the bladder was not penetrated. He next took the bistouri eaehé, and introdueed it as far as the stop in the groove, and, opening it, drew it out, making an ineision at the angle generally practised. No evidence of the bladder having been opened was afforded. An examination being now earefully made, Mr. Guthrie stated that the staff was lying betwcen the bladder and the reetum, resting against the prostate gland, whieh was small. Pereeiving now that the modified staff was too short, Mr. Guthrie brought it elose up to the pubes, and tried to impel it into the bladder; but it eould not be made to enter, the point eatehing in the side of the prostate, and raising up the bladder at each effort. The angular staff was then withdrawn, and one of ordinary shape introdueed with faeility. Mr. Guthrie having felt the stone with the beak, sueceeded in carrying the sealpel into the bladder, and some urine flowed out. The operator now requested his assistant to prevent the recurrence of a prolapsus ani, which had taken plaee to some extent, and enlarged the opening into the bladder with a bistoury, withdrawing the staff at the same time. A stone as large as a filbert was immediately removed; a second of nearly the same size was taken out; but a third, though readily seized, was too large to be extracted easily. Mr. Guthrie let go his hold, introduced his finger into the rectum, and tried to turn it, but nothing was apparently gained. He then took a pair of foreeps of small dimensions, but these proved useless, as well as two or three others. Ultimately the stone, which was oval-shaped, after a very slow and steady dilatation, was drawn out. Some wine was now given to the boy, who had been on the table for nearly an hour; the wound was dressed in the orthodox manner, and the patient put to bed.

Mr. Guthrie addressed the gentlemen present, and stated that he was now eonvineed that the staff he had invented was not adapted for ehildren; he had, however, suceeeded with it well in an operation on an adult, and he felt disposed to try it again. In this instance the staff was too short, and a miscalculation on his part of the depth of the perinæum had caused all the delay exeept in the extraction of the stone. No part was divided but what was proper and neeessary for the ends of the operation. On that day fortnight he would be glad to meet the gentlemen again, and he would then inform them of the result of the ease.

Feb. 20.—The child has not had a bad symptom since the 17th. Mr. Guthrie considers all danger to be over, except from any unhealthy suppuration that may arise between the bladder and the reetum.

24th.—The patient has had a little feverishness, whieh has been suecessfully combated with the usual salines. The urine flows through the wound, which looks healthy.

Mareh 3rd.—The patient is out of danger in all respeets. Mr. Guthrie delivered to-day a clinical leeture on the ease. He stated that he derived his idea of the new staff from a pieture in the possession of Mr. Carpue. In this is a representation of Frère Côme performing lithotomy, and the staff in his hand is of the same kind as that which he had reeently employed. No ease eould have proceeded more successfully than this boy's, notwithstanding the length of time taken up in the cutting and extraction. It had been stated that the peritoneum had been eut, and that the small intestines had deseended into the wound, but this was an error, as was proved by the non-oeeurrence of a single unpleasant symptom. What had given rise to the supposition was, the oeeurrence of a large prolapsus ani just at the time that the eut was made, and where the portion which eame down last looked like small intestine. This was at the time returned and retained in its plaee by Mr. White; the wound was healing, and the boy's health and spirits exeellent. The stones were of the ammonia-magnesian phosphate kind.

The patient has sinee been discharged well.

Another case of Stone successfully treated by Lithotomy.

David Welbourne, æt. 12, admitted 12th July, 1838, under Mr. Lynn. The father of the boy stated, that he had suffered with the symptoms he described for three years; these were indicative of ealculus, with urinary bladder. The patient, notwithstanding his diseasc, had a fine healthy appearance, and was a peasant from Hertfordshire. Upon being sounded, a stone was found in the *bas-fond* of the bladder. He was placed on middle diet; and the irritability of the bladder was pacified with doses of mild laxatives, and opiate injections administered per anum.

On the 5th August, Mr. Lynn and his colleagues thinking the patient sufficiently *acclimaté* in London and in the hospital, determined to extraet the stone by incision this day. Mr. Lynn, therefore, having the assistance of Mr. White, proceeded in the usual fashion, and made his ineision in a eool deliberate manner, shewing no desire to make his operation a match against time. The whole process was exceedingly well executed; three stones were extracted, the last with

much force; two were about as large as French beans, and one of the size and shape of a pigeon's egg flattened. The external coating of these stones consisted of lithic acid. The boy was put to bed, and a dose of castor oil was exhibited. The mode of dressing the wound in this case was a deviation from ordinary practice. A plug of oiled lint was inserted in the wound, and kept there for some hours, in order, as explained by Mr. White, to obviate the possibility of the occurrence of infiltration of urine. The presence of the lint prevented the urine from constantly dribbling from the bladder, and allowed time for a coat of lymph to be poured out over the surface of the wound, which was thus afforded a glased kind of protection for the divided cellular parts.

On the second day the water flowed through the penis. No bad symptoms had occurred.

On the third day the water chiefly ran away through the wound; he was doing very well.

On the tenth day some irritability of urinary organs occurred, and was in a few hours alleviated by opiate injections, and mild laxatives.

On the thirteenth day some fever had affected the boy; he was dosed with saline, aperient, and effervescent medicines, with complete success. The urine flowed at this time entirely through the penis.

Aug. 20th.—This day the boy is convalescing rapidly. A few drops of urine ooze through the wound, which is closing rapidly. The patient's appetite is good, and he sleeps well and enjoys himself.

Although these two cases are successful examples of lithotomy, the question naturally arises, why were they not treated with lithotripsy?

The reason assigned is, that children of that age cannot be restrained in a sufficiently steady position to admit of the manipulations of the "percuteur;" but it would appear, that if the boys could be held sufficiently firm to undergo the dangerous cuts of lithotomy (and the first boy was an hour on the table), the same precautions would be sufficient to save them harmless from the comparatively innocent manœuvres of the lithotriptist. It is well known that, in Paris, Leroy and Civiale frequently operate on children of a very tender age, and with unvarying success. Whether Baron Heurteloup, who so far exceeds them in manual dexterity, has performed any operations on children in this island, is not on record. It is much to be regretted that the Baron should allow his zeal for lithotrity, which, by his invention of the "percussor," he has really rendered a generally attainable art, to be supplanted by his ardour as a patentee. It is true that he has received but little encouragement from scientific men in this country, not a single distinction having been conferred upon him for his important services. Manufacturers of "dips," and concoctors of sugar-plums, are made knights and baronets; but the great improver of an art which subtracts most materially from the sum of human misery, is driven by the cold neglect of this country, and allured by Russian rubles, to divert his talents from their natural province, to the invention of machines destined to the destruction of his fellow men!

Dupuytren, who was a high practical authority, speaking of this operation (lithotripsy), says:—"Le traitement est assez court; mais il peut traîner beaucoup en longueur par différentes causes. C'est sans doute un malheur; mais il me semble, qu'après tout, un calculeux doit s'estimer très heureux d'acheter la cessation de ses souffrances et peut être la vie par quelques efforts de patience, et qu'au pis aller, il vaut mieux pour lui rester trois mois, entre les mains du lithotriteur le plus lent, que deux minutes sous le bistouri du lithotomiste le plus agile."

There can be no doubt that, if lithotripsy were entirely unsuccessful in its main object—the destruction of the stone—still its services have been most important in improving our means of diagnosticating calculous diseases.

After the operation last described was concluded, Mr. White stated he had invariably observed, that when violence was used in extracting the stone from the bladder through a small opening, the cases ended more fortunately than when they were easily and quickly extracted through a large orifice.

IDIOS.

FAILURE OF MESMERISM IN PARIS.

DURING the discussion which ensued on the report being made by the last commission appointed by the Royal Academy of Medicine in Paris (a translation of which appeared in the pages of this journal), M. Burdin, a member of the society, offered a prize of 3000 francs to any one who could produce a person capable of reading without the assistance of eyes, or of light, or of the touch. The decision was to be left in the hands of a commission to be appointed by the Academy; but only one candidate for the munificent premium appeared. A Doctor Pigeaire, of Montpellier, wrote that his daughter, a girl of 14 years old, could accomplish the feat,

when placed in the magnetic state by her mother. A commission, consisting of MM. Double, Husson, Chomel, Morreau, Louis, Dubois (d'Amiens), and Gerardin, was therefore at once appointed to inquire into the case, and Dr. Pigeaire eame up from Montpellier to Paris.

M. Burdin allowed, at the request of Dr. Pigeaire, that the book from which his blind-folded daughter was to read might be placed in the light, and that touching it might be permitted, so far as that sense was not made supplementary or subsidiary to sight; and he left it to the eommission to decide whether the bandage employed to blind-fold the patient with was satisfactorily capable of excluding all light. The father presented one to the eommission, with an offer of a thousand *sous* to any one who, in the normal state, eould read with it on. It was to eonsist of three pieces of black velvet; the eyes were to be padded over with cotton, and then covered below with a double band of fine linen. If this kind were satisfactory, they might themselves have another made, but it must be of the same form, lest a disturbance of the habit whieh the girl had fallen into might prevent the full development of the clairvoyant state, and of the same materials, because it had been found that velvet did not obstruct the passage of the mesmeric fluid. If, however, sueh another was made, it would be necessary, wrote Dr. Pigeaire, that his daughter should make first a trial of it, in in presence of the President of the Commission and the Secretary of the Aeademy; and if Madame Pigeaire was satisfied with it, it should then be delivered into the custody of the President, to be kept under lock and key till the commission met to make their decision. He proposed also that the eommission should be divided into two parts; that if a first experiment failed, four trials should be allowed in each series; but that if one experiment sueeeeded, the commission should for that time consider its mission ended. The lower edge of the bandage, he granted, should be glued with silk to the eheeks and sides of the nose; and the book was to be eovered with a flat pieee of glass, which must be allowed to lie quite steady, and to keep the book quite open.

A meeting was now fixed, and a bandage was offered by the eommission to M. Pigeaire, in accordanee with what M. Burdin required of them. But he refused it, saying that it was necessary there should be no spaee between the bandage and the eyes, which ought to be in immediate eontact. They therefore offered to let him pad it, so that it might exactly fit and come in contact with the eyes: he refused, and said that he had tried numerous descriptions of bandage for blindfolding his daughter in vain; that all had produced bad symptoms in her, when in the somnambulic state, except his own, which he now offered, and whieh was the only proper one to be used, and eould not be replaced. The commission then offered that she should wear the bandage which he presented, provided she were covered by a moveable veil, which should intervene between her face and the book she was to read from. This, however, he also deelined, on the ground that he had reason to believe that she read with the nerves of her face; and it was then suggested that a sereen should be adapted with two conical appendages which should permit the light refleeted from the book to fall on her cheeks, but should prevent it from coming to the lower edge of the bandage; but this condition he in like manner refused to submit to.

He answered further to the inquiries which were made, that she would begin to read in from a quarter of an hour to an hour and a half after the applieation of the bandage, or the commencement of the magnetie state; that it was necessary the book should be placed either on her knees or on the lath immediately before her; and that she must be allowed to place it as she liked, but that she could never read from it if it were held directly before her on a level with her eyes. They observed that when the bandage was put on over the girl's eyes, she used great exertions in moving the museles of her faee and lips, as if to displace it; but that, he said, was sometimes her habit when in the magnetie state, though she could see just as well when she did not do so as when she did. He added, that at one time she used to be able to read from a book which was shut up in a box; but when the eommissioners said they should deem it perfectly satisfactory if she would do that, he answered that he eould not be answerable for her possessing the same power now. He said, too, that it would throw her into eonvulsions if the fingers of one of the commissioners were placed on the lower edge of the bandage over her eyes while she was trying to read.

Hearing all this, the eommission deelined the inquiry, beeause M. Burdin's demands were not at all complied with, the bandage for blindfolding not being in their opinion sufficient for the purpose, and the position in which the book was to be placed being favourable to the transmission of light to the eyes under the lower edge of the bandage.

The report detailing the above proceedings, and from which our abstract of them is taken, was read before the Academy on the 14th ult., when M. Cornac, who had been present at the meeting betweeu the

commission and M. Pigeaire, said that Madame P. had, in his presence, placed the several pieces of velvet in succession over her daughter's eyes, and had then glued two broad bands of English taffetas from their lower edge to the cheeks, and two more by the sides of the nose, in the angles between it and the eyes. The girl being then magnetized, he took a volume of Malherbe's works from his pocket, and placed it before her in the manner required. She grew impatient, and appeared annoyed and harassed; at last she read slowly and with difficulty two couplets of an ode, all the time frequently putting her hands to the bandage, and working actively and strongly with the muscles of her cheeks and lips. The bandage was then removed from above downwards; and if, said M. Cornac, we could not see any gaps in it, we will not assert that there were none. He himself, however, could not distinguish light from darkness when it was put over his eyes; and M. Gerdy could only tell that distinction. M. Velpeau was able to see under its lower edge sufficiently to distinguish a card, the ace of diamonds. M. Dubois said that it was at the first glance evident that light could be admitted to the eyes under the edge of the bandage for blindfolding, and that there was ample confirmation of this in the refusal to permit it to be kept close by the fingers, or be covered with the screen.

FRACTURE OF THE PELVIS FROM A SLIGHT FALL—DISEASE OF THE RENAL CAPSULES.

M. Bouvier related the following case at the Royal Academy of Medicine, at its sitting on the 3d of July. Chassang, an inn-keeper, 71 years of age, was admitted in the beginning of April into the Hospice de Larochefoucault, for pains resembling those of rheumatism, which he had felt for some time in his lower limbs. On the night of the 23d, having drunk rather more than usual, he fell from his bed. From this time the pains seemed fixed in his left groin, which was painful on strong pressure, though the most minute examination could discover nothing more than a few glands slightly enlarged. He remained for three weeks unable to get up, and never afterwards walked without crutches: at the same time his strength gradually failed, and his appetite diminished.

On the 18th of June, there was general depression; the skin had a straw-yellow hue; and he lay on his back with his tongue dry, his pulse at 120, his cheeks slightly flushed; he had slight vomiting and diarrhœa, but neither cough nor expectoration.

On the 23d constant drowsiness, with occasional confusion of ideas. He died on the 26th.

At the post-mortem examination, ramollissement of the brain, with 4 or 5 ounces of limpid serous fluid under the arachnoid and in the ventricles; small collections of miliary tubercles in the upper lobes of both lungs, and especially of the right, at whose apex there was a small cavity; œdema, and slight pneumonia in the posterior and inferior lobes on both sides; and the following lesions of the pelvis and renal capsules:—a simple but complete fracture of the pelvis across the left ilium, from the ileo-pectineal eminence, through the middle of the acetabulum, to the spine of the ischium. The portions of bone were slightly moveable; the periosteum and adjacent cellular tissue were thickened and indurated in the neighbourhood of the fracture, and below them the bone was eroded and friable in several parts. Some lymphatic glands were similarly indurated; the bursa beneath the psoas injected, and containing a reddish fibrinous substance; the walls of the femur were of their normal thickness, and appeared very solid; the bones of the pelvis, with the exception of the alteration which had evidently followed the fracture, did not seem to have lost any of their consistence, so that if it were necessary to admit in this case, as in the former, (communicated a year since, and published in the *Bulletin*, vol. 1, page 912) a fragility of the bones consequent on advancing age, that change is probably less in the apparent structure of the osseous tissue than in some modification of its intimate nature, of which the cause must as yet be sought for.

The renal capsules formed large tumors, especially that of the left side; their surface was smooth and even, and so soft in some parts as to seem fluctuating; their substance was entirely converted into encephaloid matter, of which the greater part was softened and mixed with blood effused here and there into its interior. This alteration was the more remarkable as it seemed to be primary, no other organ in the body being similarly affected. The kidneys were normal.—*Bulletin de l'Académie Royale de Médecine, August* 15, 1838.

ACADEMY OF SCIENCES.

At the last annual public meeting of the Academy of Sciences, held at the Institute, the president, M. Becquerel, awarded the prize of experimental philosophy to M. B. Heine, of Wurzburg, for his researches into the regeneration of the osseous system.

STATISTICS OF INSANITY AT THE BICETRE.

It appears from a statistical account of the hospital of Bicêtre, that out of 800 insane persons now kept in it, 400 have been pronounced incurable. The mortality among the insane is one in seven; and the average age of those who have died has been found to be 19 years. The average is 50 for females, as ascertained at the Salpétrière. The average stay of the patients in the hospital is about three years. The months during which the greater number die, are the first five months of the year. The months in which the greatest number of patients are admitted are June and July. It is known that the human mind is subject to aberration at all ages from 10 to 89; but the age in which madness most frequently shews itself in men is from 30 to 39, and in women from 40 to 49.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO HAVE RECEIVED DIPLOMAS.

July, 1838.

D. Mathias, London.—Henry Waldron, London.—J. Bluett, Calcutta.—H. Coote, Bedford Place.—G. Shutter, Guildford Street.—F. Fox, Gloucester Place, New Road.—J. M. Stokes, Southampton.—C. W. C. Otway, Southwark.—A. Qualtrough, Isle of Man.—R. Askwith, Leeds.—T. Coe, Bury St. Edmunds.—H. J. Clare, Wicklow.—W. A. Janson, Seaham, Durham.—W. Bradford, Chepstow.—Forbes Barclay, Shetland.—L. B. Cobb, London.—E. Hoyland, Blackfriars Road.—T. R. Dyce, Aberdeen.—R. G. Butcher, A.—F. J. Mouat, London.—T. S. Burmann, Stratford-on-Avon.—T Tuomy Kanturk, Cork.—H. F. Waller, Feversham.—William Fletcher, Tean, Staffordshire.—R. G. Brewster.—F. Woodhouse, Westport, Mayo.—C. F. Edwards, Bedford.—J. Wordsworth, Rydal, Westmoreland.—E. Guest, Birmingham.—W. Highmore, Sherborne.—G. R. Wyatt, London.—E. Webster, Welden, Northamptonshire.—J. Goring, co. Tipperary.—G. Goodwin, Ashbourne, Derbyshire.—R. Alford, Taunton.—G. Southam, Manchester.—R. W. Sannemann, Hounslow.—J. W. Robinson, London.—F. R. Chadwick, Long Ashton.—Adolphus Collings, Guernsey.—C. Murphy, Seariff, co. Clare.—Denis J. Hynes, co. Galway.—Richard C. Elliott, Artillery.—Kester E. Knight, Calcutta.—J. G. Passmore, Bath.—J. B. Easton, Plymouth.—J. Kenyon, Liverpool.—A. R. Neville, Toronto, Canada.—J. H. Haire, R.N.—R. F. Wells, Highgate.—Michael M'Bride, A.—Richard Bligh, Calcutta.—N. E. Parker, St. Neotts.—John Coales, St. Albans.—J. Robinson, Dublin.—Thomas Hitchings, Oxford.

August, 1838.

R. Brandon, London.—P. Trantore, Guernsey.—C. G. Andrews, London.—H. Obre, Chelsea.—W. Wood, London.—J. Gledhill, Eddercliffs, Yorkshire.—J. Denton, R.N.—G. Dry, Oxford.—F. B. Curtis, London.—W. C. Nutt, Plumstock.—T. C. Grant.—F. M. Baker, Botley, Hants.—A. Farquar, Aberdeen.—W. H. Caddy, Torrington.—D. G. Croghan, Wexford.—F. B. Pritchard, Hambledon.—D. N. Wallace, Boyle.—J. Crook, Norwich.—E. Scholfield, Ashton-under-Line.—H. W. Hare, Yoxall.—L. Hare, Upper Gower Street.—J. H. Kent, Stanton.—J. Brade, Liverpool.—H. Hawes, Westbromwich.—Charles Simons, Gloucester.—W. Werren, South Brent.—J. H. Rogers, Newman Street.—J. Craig, Aberdeen.—A. Ingham, Turniff, Aberdeenshire.—J. Espinet, Trinidad.—B. N. Arnold, Birmingham.—W. Reeves, Carlisle.—J. Allen, Hammersmith.—D. Jordan, Castlebar.—J. Glasscott, New Ross, Wexford.—W. F. Babington, Bombay.—J. Colebrook, Petworth.—H. J. Hemingway, Dewsbury.—E. D. Batt, Brecon.—R. J. H. Scott, Devon.—J. E. Jones, Oakham.—J. Flagmore, South Molton.—G. B. Willis, Cambridge.—C. J. Philbrick, Colchester.—J. Williams, Stoke Newington.—W. W. Rawes, Stafford, Essex.—W. E. Grove, Cork.—Thomas W. Foster, Sheffield.—E. Howard, Portsmouth.—J. G. Brighton, Ombersley.—T. J. Welsh, Newport, Monmouth.—M. D. Hollins, Manchester.—A. A. Jack, Plymouth.—J. M. Gittings, Barbadoes.—A. Keyser, London.—H. Taylor, Devizes.—Ellis P. Kilson, Bath.—J. Clifford, Camberwell.—J. J. Adams, Finsbury Square.—John Simon, Bedford Street.—J. Salt, Betley, Staffordshire.—Thomas R. Dingley, Devonport.—C. Edwards, Armagh.—T. C. M'Evoy, Balbriggan, Dublin.—David Thomas, Llan Oils.—W. H. Pritchard, Newman Street.—Peter Taylor, Miramichi, N.B.—J. C. Beales, New York.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *Sept.* 4, 1838.

Abscess	4	Heart, diseased	1
Age and Debility	22	Hooping Cough	2
Apoplexy	7	Inflammation	13
Asthma	2	Bowels & Stomach	10
Cancer	2	Brain	4
Childbirth	3	Lungs and Pleura	1
Consumption	47	Insanity	7
Convulsions	33	Measles	6
Croup	5	Mortification	1
Dentition or Teething	5	Rheumatism	2
Diarrhœa	3	Small-pox	14
Dropsy	6	Spasms	1
Dropsy in the Brain	3	Thrush	4
Epilepsy	1	Unknown Causes	122
Fever	12		
Fever, Scarlet	3	Casualties	6
Fever, Typhus	11		

Increase of Burials, as compared with the preceding week . . . 35

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

August.	THERMOMETER.	BAROMETER.
Thursday . 30	from 41 to 67	30·08 to 30 07
Friday . . 31	44 70	30·08 30·05
September.		
Saturday . 1	47 69	30·02 Stat.
Sunday . . 2	48 68	30·13 30·08
Monday . . 3	49 69	30·08 30·13
Tuesday . . 4	38 68	29·94 29·76
Wednesday 5	54 70	29 61 29·44

Wind, S.W.

Except the 2nd and 4th, generally clear. A little rain on the 4th and 5th.

CHARLES HENRY ADAMS.

ERRATUM.—The paper on the "Origin of the Optic Nerves," in last number, page 894, was by Dr. KENNION, not "Kerrison."

WILSON & SON, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 15, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXIX.

Diseases of the Orifices and Valves of the Heart.—Anatomical Characters and Varieties.—Soft Thickening.—Tough Thickening; Contraction, &c.—Ossification.—Thickening with Ulceration, Vegetations, &c.—Atrophy of the Valves.—Prevalence of Disease on the left Side; its Causes.—Pathological History and Physical Signs.—Principles of Diagnosis of Valvular Disease.—Disease of Aortic Valves; Signs; General Symptoms.—Disease of Mitral Valve; Signs; General Symptoms.—Disease of Pulmonary and Tricuspid Valves.—Diseases of the Aorta.—General Dilatation; Signs.—Aneurisms; Symptoms and Signs.

We have seen a little of the effects which result from diseases of the fibres which move the blood; we have now to attend to the chief lesions of that mechanism by which this motion is directed and conducted—the orifices, with their valves and great arteries.

Diseases of the orifices and their valves are of very considerable variety, but those that interfere with the office of the parts which they affect may be reduced to two classes—those that more or less *obstruct* the current of the blood in its proper channel, and those that occasion it to take a *reversed* direction. The former, for brevity, I term *obstructive*, the latter, *regurgitant* lesions. You now know so well the natural structure and function of the several valves, that the description of the anatomical characters of a few of these lesions will enable you to understand their effects in interfering with the mechanism of the circulation.

The most common change in the valves is *thickening*, which presents itself chiefly under two forms:—1. A *softer* kind of *thickening*, in which the valves retain much of their pliability; 2. An *opaque thickening*, with more or less *induration*, so that the valves become less flexible and mobile than usual. The first occasionally affects the semilunar valves, but rarely the auricular. It appears to be the product of inflammation chiefly affecting the serous membranes of the valves, and is produced by organized lymph between their layers, or upon their exterior, from which it often may be separated. This deposit is generally seen less at the margins than at the middle and attached parts of the semilunar valves, chiefly on their ventricular sides, and occasionally forming a bond of *adhesion* between two adjoining valves, which are glued together as far as the corpora arantii, as you see in these drawings. This constitutes a form of slight obstructive disease of the arterial orifice. I have in a few cases seen the same deposit on the layers and cords of the auricular valves, here and there causing them to adhere, and forming a false membrane on their auricular surface.

The *opaque tough thickening* of the valves is the most common, and is frequently combined with the former. When occurring simply, there is a smoothness of the exterior, which, with the character of the thickening material, seems to indicate that it is chiefly between the serous layers of the valves, and probably arises from disease in the fibrous tissue, that forms the strengthening web of the valves. This

thickening commonly affects the semilunar valves to their very margins, and is sometimes seen extending into the ventricular lining, and perhaps forming there slight ridges where the bands of fibrous tissue cross the muscular walls. With the laminæ of the auricular valves it commonly involves their orifices and part of the auricular lining, and extending into the tendinous cords, which are irregularly thick and knotty, and their fleshy columns are likewise occasionally changed into fibrous cords. With this more rigid thickening there is often combined a *contraction* or *elongation* of some parts, or both, affecting different parts; and it is such changes of proportion that so commonly injure the proper action of the valves and orifices. Mere thickening, unless it be attended with great stiffness, will not materially injure the office of the valves; but changes in form and proportions necessarily must. Here you see several drawings illustrating these changes. In these affecting the mitral valve, you may perceive how the shortening of a few of the tendinous cords throws the rest into a loose state, in which they cannot draw the membranes of the valve smooth on each other; hence they form folds and chinks, through which the blood must regurgitate at each systole. Here, again, you see instances of portions of the valve and their cords elongated and enlarged, so that they may be forced backwards towards the orifice, and by keeping the valve partially open, also occasion regurgitation. Here again the two laminæ of the valve are thickened, contracted, and adherent, forming a sort of narrow funnel-shaped tube instead of a valve, both retarding the flow of blood from the auricle into the ventricle, and never closing against its reflux into the auricle. This forms both obstructive and regurgitant disease of the mitral orifice. In this manner the left auricular orifice is sometimes so contracted as not to admit a little finger; naturally it admits with ease two full-sized fingers. Here again, in the aortic valves, you see one of them contracted at its free margin, so that it could not meet its fellows, but left a chink through which the blood regurgitated with a murmur into the ventricle at each diastole. I have known the same effect to arise from this kind of disease preventing the equal dilatation of the valves. When a ventricle with its orifice is dilated, the semilunar valves are generally dilated with them, so that the proportions are preserved; but if there exist in one or more of these valves a rigid thickening that will not yield, the dilatation of the orifice renders them insufficient; they cease to close the orifice completely, and consequently regurgitation takes place through them. Again, an uncommon elongation of the free margin or attachment of one or two of the semilunar valves, so that these margins, instead of being closed against their fellows, become retroverted under them, constitutes another form of regurgitant disease of the semilunar valves, of which I shew you here several specimens. The valves appear to have given way generally at their attachments to the artery, perhaps from the yielding of its fibres and the formation of a slight pouch in it; but you see the effect is to destroy an attachment of two of the valves, which, instead of forming bags, are either retroverted, or contracted and thickened, on a line with the ventricular convexity of the other valve. In these cases probably rupture of some fibres may have at some time taken place, and accelerated the change; but it is always associated with more or less of the thickening of which I am now speaking.

To the thickening of the valves and orifices there is frequently added great induration and even ossification; and this alone may constitute disease, generally of the obstructive kind. As such, it often affects the aortic orifice, particularly at the attached portions of the valves, and at the curves of the artery, which form the boundary of the little recesses or sinuses into which they retire. If in either of these situations a rigid portion project into the current as it passes, it constitutes obstructive disease, and may cause a constant murmur with each pulse; or if the whole ring be rigid and unyielding, it may, under the circumstances of greatly increased action, constitute a constriction, the vibrating resistance of which, opposed to the current, may give a murmur. Sometimes the whole of the valve is so rigid that it must have opposed more seriously the passage of blood. In these drawings you see aortic valves which are ossified, and have adhered at their edges so as to leave only a little aperture at their middle, where their margins were loose and permitted the blood to pass.

The lesions which I have been describing are the most common forms of diseases of the valves; they are mostly of a chronic character, and in the greatest number of instances originate in rheumatic endocarditis. Their effects on the circulation and on other functions have been generally proportioned to their degree and character, the rapidity of their production, and to their complication with hypertrophy and dilatation, or other disease: hence they may vary infinitely; they may exist and produce cognizable signs, without obviously deranging the heart's action, or in an overt direct manner in-

juring the health; and they may cause the most distressing and dangerous symptoms, and sooner or later prove fatal.

There is another kind of thickening to which the valves are subject, accompanied with *softening*, *ulceration*, and often *rupture*. This is fortunately not very common; for it is a a terrible disease, destroying the valves, chiefly the aortic, in the course of a few weeks; and soon after proving fatal. This occasionally supervenes on older disease which has originated in rheumatism; but I have in several instances known it to arise independently of rheumatism, perhaps after a severe cold, or violent strain, especially in persons who have been addicted to spirits. The ruptured or ulcerated portions of the valves are found loaded with ragged, soft, fragile vegetations, more or less tinged with blood, and these are also sometimes seen adhering to adjacent parts where the endocardium is entire. The remaining parts of the valves are much thickened, and opaque yellowish white, with a pink hue; and pink patches are often seen in the aorta, with atheromatous thickening. I think that these changes may be viewed as the effects of acute inflammation affecting all the tissues of the valves. In these two drawings you see specimens of its ravages. In this, all three aortic valves have been completely broken up, and their torn margin fringed with thick vegetations hung down into the ventricle. In this, again, you see one of the valves has given way at its centre, and its margin, thickened and loaded with vegetations, lies like a cord across the mouth of the artery, whilst the lacerated attached portion is retroverted into the ventricle, and, with part of the lamina of the mitral valve near it, is covered with vegetations. In other cases I have seen smaller perforations, probably ulceration, both in the middle of the aortic and in the mitral valves; always fringed with vegetations. The smooth perforations so common at the free margin of the semilunar valves are of a different character, and not dependent on the same cause. I believe that in time the vegetations just described may become organized, forming the cartilaginous or fibrous little bodies which have been called warty excrescences, in which osseous matter is often found, and which generally have more recent vegetations attached to them.

Lastly, we have another class of valvular diseases that have been very commonly overlooked. I mean *atrophy* or *wasting* of the valves, by which their membranous portions may become *shortened* or *perforated*, and the tendinous cords withered and absorbed away. The semilunar valves, both aortic and pulmonary, present these in the most obvious manner in the oval perforations, at their free margins. You see in these drawings several specimens. The perforations are oval or rounded, with their edges quite smooth and thin, as is also the whole valve. In fact, although there may be partial deposits of false membrane on their ventricular surface, the valves are most commonly very thin and flaccid when they present these perforations; and in other parts there may be seen still thinner spots that are separations of the fibrous web, and are all but through the serous membrane also. Now so long as these perforations are confined to so much of the margin of the valve as closes against that of its fellow, they may not produce regurgitation; and this is very commonly the case. In the mitral valve, the wasting usually affects the posterior portion, the membrane of which is often annihilated by it, the cords being inserted directly into the auricular ring. The anterior lamina is also occasionally found much shortened, and without those fine thin expansions of membrane which commonly unite the cords to each other, below their insertion into the thicker part of the valve. It is pretty plain, that with this state of the valve, if there be not habitual regurgitation, inordinate action of the heart, or slightly disturbing circumstances, may induce it, especially if there be at the same time dilatation of the orifice. I have known a murmur produced by flatulent distension of the stomach, and by certain postures, which I have been inclined, for reasons to be explained afterwards, to refer to this kind of imperfection of the mitral valve. I have found these atrophied conditions of the valves in cases where there was no trace of previous inflammatory affections of the heart. In one case there was no other thoracic disease at all, the patient having died of fever; and attention was turned to the heart only in consequence of there having been many heart symptoms, with a constant blowing murmur below the left breast, during life. In some cases the smooth perforations have been found in valves thickened by inflammation, probably of more recent date.

With these several forms of valvular disease, especially those of an inflammatory origin, there are commonly combined various forms and degrees of hypertrophy and dilatation; the production of which may be more or less ascribed to and explained by the nature of the valvular disease. Thus hypertrophy with dilatation commonly affects a ventricle the orifices of which are partially obstructed, or permit regurgitation, either of which conditions tends unduly to irritate and distend it; and as the efforts of the

ventricle continue to be powerful or not, the hypertrophy or the dilatation will prevail.

You must have perceived from the descriptions already given, that the valves of the left side of the heart are far more commonly diseased than those of the right. Yet occasionally the latter do present the same changes as the left valves, but it is very rare that they are alone affected, or in a much greater degree than the left. Nor can you wonder at this when you consider the function, structure, and relations of the left side of the heart as compared with the right. Its function requires that it should be endowed with much greater strength, that it may propel the blood through the great circulation; and this greater strength exposes its parts to more violence from its own movements. Then, to bear this violence, its valves and orifices are furnished more abundantly with a strengthening fibrous tissue; yet this very tissue is obnoxious to inflammatory affections, which tend peculiarly to alter it. A tissue which protects by its strength cannot be endowed with high vitality; as its vitality is low, so is its natural reparatory power slow, and the hastening of this process by inflammation changes the nature and strength of the material. Hence there is deposited no longer the fine, even, transparent, capillary fibres, which only glisten into view with a silvery whiteness when they are bundled together in numbers, but an opaque, yellowish white, thick, tough material, partly fibrous and partly amorphous, possessing neither the strength nor the delicate flexibility of the original texture, and consequently injuring the apparatus by its bulk, its stiffness, or its liability to extension or contraction, laceration or rupture. Then the very perfectness of structure, that gives to the left ventricle a superiority in point of strength over the right, exposes it more to the effects of violence or excessive action. When the right ventricle becomes distended, you have seen that its auricular valve opens and permits a partial reflux; but the mitral valve of the left ventricle is made to close perfectly, and to bear the whole strain of the muscular fibres contracting on the blood. Again, the pulmonary valves are exposed to pressure from the tension of the pulmonary vessels only, which are exposed to little pressure but that of the expiratory forces; but the aortic valves receive the strain of the great arterial system, liable as this is to violent increase from the pressure of the muscles of the body, and of any sudden blow or impression on regions containing its larger branches. You will not wonder, then, that the valves of the left ventricle should be more frequently and more extensively diseased than those of the right. You have rather reason to wonder they suffer so little, and how they can stand so well and so long the strain to which they are exposed, particularly when disease has once begun in them. I believe that they do suffer much more frequently than is generally supposed, and that many of the circumstances which we noticed as causes of inordinate action of the heart, do often irritate, or mechanically strain and injure the membrane covering the valves, and excite a temporary inflammation in it. How else are we to account for the partial thickening so commonly seen, especially in the aortic valves, even where there had been no history of any complaint particularly affecting them? That such a thickening has been slight and harmless we may ascribe to the perpetual motion of the valves and sweep of the current, which generally prevent any accumulation of deposit that is not connected with an intense inflammation, or one affecting the subserous tissues. But occasionally circumstances may favour an accumulation; thickening and rigidity may take place; and thus disease will now and then begin in the valves, independently of rheumatism or any other common cause, and gradually infringe on the integrity of their function. The atrophous form of disease, however, which produces the thinning and oval perforations in the semilunar valves, and wasting of the membranous parts and tendons of the auricular, is commonly met with in either compartment of the heart. It was on the right side that I have seen it attain the greatest degree, having reduced the membranes of the tricuspid to mere fringes, and the margins of the pulmonary valves to a network of threads. But even such disease on this side of the heart produces less prominent symptoms than slighter lesions on the other.

We shall now consider the *pathological history* of diseases of the valves; and as a distinction between them can scarcely be made without a study of their *physical signs*, we will begin with this subject, which will supply us with further knowledge of their more essential pathology. I have before had occasion to mention to you that the phenomena called *murmurs* are often produced by the modifications which valvular disease occasions in the current of the blood. In order that they be produced there must be the requisite elements, sufficient force in the current, and such a modification as may constitute a vibrating resistance to it. You can understand that valvular disease may be present without forming these elements, and therefore without any attendant murmur, as when the current is too languid, or the resist-

ance too loose or yielding to cause vibrations; but in by far the greater proportion of cases, valvular disease does cause murmurs, either constantly, or under the circumstance of the current increased by exertion or other cause of excitement.

I hope that you now comprehend the principles of the production of sound too well to fall into the common error of supposing that the amount of a valvular lesion is indicated by the *loudness* or extraordinary character of the murmur. The loudest and most singular murmurs are the whistling, cooing, or musical, yet they are occasionally produced by comparatively slight lesions. The loudest musical murmur that I ever heard was produced by the retroversion of only the margin of one of the aortic valves, which you see in this drawing, the rest being in a pretty capable condition. The murmur was a loud cooing with the second sound; and the patient dying of fever, we had the opportunity of verifying the diagnosis which we had made of the disease. Again, in this case, where you see all the aortic valves entirely broken down by disease, the murmur, which was double, was obscure and distant.

The *quality* of the murmur is a better index to the character of the lesion, but it is by no means a sure one. Uniform and shrill murmurs, whether blowing or musical, generally indicate slighter lesions than the rough or grating and deep-toned, because they are commonly produced where there is no great looseness or irregularity of the aperture through which the current generates the sound; they are most frequently caused by regurgitation through small smooth chinks between the mitral or the aortic valves. The rough grating or sawing sounds generally indicate more extensive disease, not being produced (as it has been asserted by others, and stated in my treatise on their authority) necessarily by indurated or osseous irregularities, but by a larger current passing over a body which offers to it an irregularly vibrating resistance. Such murmurs most commonly occur in the aortic orifice. It was a remarkably rough double sawing murmur which accompanied the lesion of the aortic valves represented here*. One of them is broken through all but its margin, which is left a ragged cord across the mouth of the aorta; there were here plenty of soft vegetations, but no material induration. A rough grating with the first sound was produced in this other case also, where the aortic valves were perforated by atrophy, leaving mere threads at parts of their free margins, which, forming loops, might catch and flutter in the current*.

The more distinguishing circumstances in regard to murmurs are, however, their relation to the natural sounds or motions of the heart, and the points at which they can be best heard. Laennec and Bertin paid some attention to these points, but without obtaining any general results. I believe that Dr. Hope was the first who attempted a diagnosis between different valvular lesions by the apparent situation of their murmurs; but this attempt was not founded on correct acoustic principles, and the means of distinction announced by him have failed when tried by others. He asserted that the disease of a particular valve may be known by the murmur being heard best at a spot on the walls of the chest nearest to that valve, and by its being whizzing or deep-toned, according to the proximity or remoteness of the valve from the walls of the chest. But only look at the anatomical position of the different valves. All four sets are comprised within a space of a couple of inches, being placed, in regard to the front of the chest, one behind another rather than side by side. Then see how the lung intervenes between them and the front wall of the chest, and must obscure and confound the sounds from beyond it; and you will acquit me of the imputation of stupidity when I tell you that I have never been able, in so small a space, to discriminate between the murmurs generated at the different orifices. But there are principles of diagnosis in these murmurs, and if they be carefully and rationally studied, they will generally enable us to distinguish, as far as is useful, between the different kinds of valvular lesion. They are nearly such as I gave them in the last edition of my little treatise on Diseases of the Chest†; and I have since been enabled to confirm and correct them by the examination of very many cases before and after death.

In judging of the seat and cause of a cardiac murmur, you have to attend chiefly to the *period* of the heart's motions at which it occurs, and the *manner* in which it is *transmitted* to the surface. A mur-

* These and other illustrative drawings being too numerous to be introduced here, are reserved for a separate work. The notices of atrophy, polypous concretions, malignant diseases, textural transformations, malformations of the heart, and other subjects of minor import, are also omitted.

* In our experiments with India-rubber tubes, the best imitation which we could make of the sawing or rasping sounds, was produced by directing a strong current across rather loose threads or strings. This corresponds with the experience recorded in the lecture.

† Since the publication of this work in 1835, similar diagnostic rules have been proposed by other authors, English and French, without due acknowledgment.

mur accompanying only the first sound or impulse is necessarily caused by a current of blood *from* a ventricle; one accompanying, following, or replacing the second sound, must arise from a current into a ventricle. This simplifies the matter; but there are still two ventricles and two orifices to each; and how are the murmurs of these respectively to be distinguished? not by the different positions of these orifices certainly; they are too close together for that; but *by the different directions in which the sonorous currents spread the sounds*, and *the different manner in which they are transmitted to the walls of the chest*. Thus murmurs generated at the origin of the arteries will generally be more or less transmitted in the direction of the current along these arteries; and those produced in the auricular orifices will be conducted both by the current into the auricles, and by the tightened cords and fleshy columns to the apex of the heart, which is generally more or less in contact with the ribs. So far for the general principles: now let us consider briefly the special diagnosis of particular lesions.

Obstructive disease of the *aortic orifice* (observe, by *obstructive* I do not mean that the actual impediment is always much) is generally attended by a murmur with the first sound and impulse, heard in the region of the heart, along the upper half of the sternum, and in the right or both carotid arteries. The point where it is heard loudest will vary according to the position of the heart with regard to the lungs and walls of the chest. If it lie deep, and well covered with spongy lung, the murmur may be louder at the apex and in the right carotid than at any intermediate point, because the dense heart and the distended arteries convey the sound better than even a much shorter length of pulmonary tissue. In chests less deep, and especially where enlargement of the heart brings its base nearer to the sternum, the sound is loudest about the middle of this bone, or a little on either side of it; but still the more distinguishing character is, that it is heard above, in the direction of the innominata and carotid arteries, where sounds from the other valves can scarcely reach. The murmur is commonly like grating or sawing, and varies much in length and loudness in different cases. In some it lasts the whole period from the commencement of the systole to the occurrence of the second sound: in others it is a mere roughness or whizzing accompanying the natural sound, which may be pronounced *trrhub-dup* or *djub-dup*. It is very often accompanied by regurgitant disease of the aortic valves; and when this is not the case, the second sound (the *dup*) is frequently less clear than usual, which implies that the valves, from thickening, do not act freely.

Regurgitant disease of the *aortic orifice* produces a murmur with or instead of the second sound, by the sonorous gush of blood from the artery back into the ventricle. The place of greatest distinctness of this sound will also vary, as the former, according to the position of the heart and lungs: it is generally heard best at or near the middle of the sternum; but its more characteristic feature is, that it obscures or supplants the second sound or flap at the top of the sternum and in the carotid arteries, which is usually very distinct there. The second sound of the right or pulmonary valves can often still be heard about the left margin of the mid-sternum. This murmur is seldom so rough or grating as that of obstructive disease; for the current refluent from the artery is not so large or strong. It may, however, be very loud, and even musical, as in the case which I have lately mentioned to you; and if the valves cannot close at any period of the diastole, it may even last from pulse to pulse. More commonly, however, it dies away before the next pulse; and in slighter degrees of regurgitant disease, is very short, preceding and not supplanting the second sound. This is when the valves, before they are quite distended by the full recoil on them of the arterial column, permit a little jet of blood to grate between their margins, and make the second sound like *trrht* or *tzzt*, instead of *dup*. A little vegetation, or slight deposit of rough lymph on their margins, may cause this: I have often met with it both as a temporary and as a permanent phenomenon, when there was not much derangement of the heart's action.

Combined obstructive and regurgitant lesions of the *aortic orifice* produce the double, or to-and-fro sawing murmur, which is the succession of the forward and backward gushes of blood. They are by no means uncommon; in fact, the murmur, instead of the second sound, indicating regurgitation, seldom exists without a murmur with the first also, indicating partial obstruction. These lesions, when at all considerable, are accompanied by dilated hypertrophy of the left ventricle, which often takes an elongated form, bringing its impulse lower and more to the left than usual. When the regurgitation is very free, it increases the force of the diastolic collapse so much, that I have known it resemble a second impulse. It also not unfrequently stimulates the ventricle to a second contraction, constituting a reduplication of the pulse: the rhythm otherwise is not commonly much affected. But the pulse generally possesses a character that is remarkable. When the lesion is ob-

structive only, the pulse is generally hard and jarring; but when it is regurgitant also, each pulse, although momentarily hard and full, immediately recedes, which gives it a jerking or thrilling character. The same circumstance makes the pulsation of all the arteries *visible*, and sometimes *locomotive*,—a sign of lesions of the aortic valves first pointed out by Dr. Corrigan. I have seen it so extensive in an old man, that many arteries could be seen like worms under the skin, wriggling into tortuous lines at each pulse. The cause of this phenomenon is sufficiently intelligible. It proceeds from a defective equality of tension of the arterial coats. These vessels are distended at each pulse, and emptied the instant after, the aortic valves not, as usual, maintaining their tension; and if the coats of the arteries are defective in lateral elasticity, as in old people, they may admit each jerking jet of blood only by being elongated into a tortuous line. In its extreme degrees, and especially when existing in all states of the circulation, I think that this visible or moving pulsation of the arteries is pathognomonic of regurgitant disease of the aortic valves; but to a slight extent it may be observed in many cases of excited action of the heart, especially when there is a defective tension of the arteries, as after great losses of blood, and otherwise where there is a lax state of the vascular system. I should have pointed out to you a partial exhibition of this visible pulse as a sign of such a state of the arterial system. By raising the hand above the rest of the body, you will render *visible* the pulsations of the radial artery, which becomes partially emptied by gravitation, just as in case of aortic valvular disease the arteries in general become partially collapsed by regurgitation.

The general symptoms of disease of the aortic valves vary much according to its degree. When slight, it may occasion only more or less palpitation and shortness of breath on exertion, perhaps with a feeling of tightness or pain at the sternum, and other of the common symptoms of moderate disease of the heart. But when extensive, it is the most formidable kind of valvular disease, and generally proves fatal in the shortest time. I have known several cases run their course in a few weeks after the first complaint had been made of symptoms referred to the heart. It is probable that in some of these latent disease had been going on for some time in the valves, and was suddenly aggravated by mechanical violence or inflammation occurring, and to which alone the disease was ascribed. There then come on habitually increased action of the heart and dyspnœa, sometimes aggravated by fits of palpitation and orthopnœa, sometimes with angina, cough, and expectoration, often containing blood. There is commonly some anasarca, more rarely ascites and hydrothorax. Hæmoptysis and dropsical effusions have been too exclusively attributed to disease of the mitral valve and right side of the heart: I have seen them quite as frequently with severe aortic disease; in fact, where the left ventricle cannot effectually unload itself, whether from obstruction or regurgitation, there is just as much interruption to the circulation, pulmonary and venous, as if the mitral orifice or the right heart were diseased; and there is often a greater failure of the capillary circulation, from the impaired tension of the arteries. Among the effects of this, I may mention that regurgitant lesions of the aortic valves are often attended by a pallidity and emaciation, or in more acute cases by pasty puffiness of the integuments, with less colour than is commonly seen in mitral disease; in which the symptoms are more simply those of venous congestion. There is often, too, a restless irritability about the subjects of aortic disease, comporting well with the jerking character of the pulse; whereas with mitral disease it is not uncommon to see an unusual hebetude and torpor. But these distinctions are most uncertain.

Of the lesions of the *left auricular*, or *mitral valve*, we must now speak. *Obstructive disease of the mitral orifice* consists in its contraction, in the adhesion together of the laminæ of the valve and of their cords, or in bodies projecting into it. When this is capable of producing a murmur, it is at the time of the second sound, when the blood passes from the auricle to the ventricle at its diastole; but I believe that a murmur thus caused is of very rare occurrence; perhaps only in case of considerable contraction of the orifice with thickening of the walls of the ventricle, which gives them diastolic elasticity, and thus sucks the blood from the auricle with some force: such was the state of the parts in one out of the only two or three cases which I ever met with, in which the murmur with the second sound was not distinctly referrible to regurgitant disease of the aortic valves. Where such a murmur does exist, it would probably be best heard in the same situation as the familiar one of regurgitant disease of the mitral orifice; but its best distinction would be that it does not modify or supersede the second sound in the arteries, as regurgitant disease of the aortic valves does.

Regurgitant lesions of the mitral valve are attended by a murmur with the impulse and first sound, produced by a sonorous jet of blood through the imperfectly closed

valve at each systole. I have more than once adverted to the nicely-adjusted mechanism of this valve, and I have given you a sufficient number of examples of the lesions by which this mechanism may be deranged. They are very common; and my experience leads me to the conclusion, that of the instances of cardiac murmurs which present themselves in women and young persons below the age of twenty, five-sixths are caused by regurgitant disease of the mitral valve; whilst in older persons, and those of the other sex, they are more frequently caused by disease of the aortic orifice. Now where may this murmur be best heard? The mitral valve is situated about the cartilage of the fourth left rib near the sternum; but the spongy lung, and the right ventricle too, if it be dilated, intervenes between it and the walls of the chest; hence the murmur is seldom transmitted opposite to the valve so well as through the apex, to which the fleshy columns of the valves are attached, and which, at the time that the murmur is produced, is in close contact with the walls of the chest, somewhere between the fourth and seventh ribs below, or a little in front of the left mammilla. Here accordingly I have generally found mitral murmurs most distinct, being often quite superficial, and so loud as to eclipse the natural double sound; and this being still audible at the upper and lower ends and to the right of the sternum, and in the carotid arteries, where the murmur is inaudible or distant, completes the diagnosis. Great enlargement of the heart, or consolidation of the lung, sometimes transmits the murmur loud to walls higher up, in which case the dulness on percussion at the spot would explain the unusual circumstance. In a few instances I have heard a mitral murmur very loud and superficial about the third rib near the mammilla, and I have been led to suspect that it is in these cases transmitted by the dilated auricle which receives the refluent jet of blood. It is sometimes also very audible in the left back, and below the axilla. The character of mitral murmurs is generally blowing, but sometimes passing into a whistle, and more rarely grating. In some cases it accompanies the whole systolic act, and terminates with the second sound: in others it is confined to the end of the first sound, giving to it an additional vowel or a roughness, as we may express by the words *lōō-dŭp* or *lurr-dŭp;* or, if confined to the beginning, *rrhub-dŭp* or *jub-dŭp*. You may be amused at my new words; but I think that when your ears shall have become practiced, you will admit I have spoken truly the *language of the heart*. These murmurs are not always constant, especially when the action of the heart is irregular or unequal. We then often hear some of the beats with the murmur and others without it. Sometimes the murmur alternates with a double first sound, which I have compared to the footsteps of a cantering horse, and which I have conjectured to depend on a loose state of part of the mitral valve. When you once understand the principles of the production of these sounds, such varieties are intelligible enough.

As there is in most cases of considerable disease of the mitral valve more or less hypertrophy and dilatation of the heart, the signs of these conditions may be added to those of the valvular lesion. But if the regurgitation is free, it may much diminish the loud sound of dilatation, and the strong impulse of hypertrophy of the left ventricle, by removing the resistance offered by the closure of the mitral valve. Hence the tightening flap of the ventricular sound may be diminished as well as disguised by the murmur; and the strength of the impulse may be partially lost backwards into the auricle, and followed by a heavy diastolic collapse, when the blood flows in again from the distended auricle. When there is contraction of the mitral orifice there is usually more hypertrophy than dilatation in the left ventricle, with a strong impulse over it, and the blowing of regurgitation, which is scarcely ever absent, even in these cases. The other compartments are dilated, and may give, as usual, the short double sound.

The pulse varies very much in mitral disease. In many cases, particularly those in which there is constriction of the orifice, it is very irregular and unequal, exhibiting the different varieties which I have described under the head of Irregular Action of the Heart (see Lecture XXVI.) It is generally weak also, particularly where there is either much constriction or free regurgitation. Yet I have known cases in which the murmur at the apex, during life, and the appearances after death, shewed that the regurgitation must have been free, and the pulse was nevertheless strong and jerking, there being also dilated hypertrophy. And when the heart acts irregularly, the strong beats of the pulse are in some instances attended with a louder murmur at the heart than usual. In other instances again, where the pulse was intermittent, I have observed each intermission or weaker beat to be accompanied by a loud blowing at the apex, which was much less distinct in the other beats. This intermission was obviously from the force lost backwards in regurgitation, the cause of which was probably moveable, or subject to variation, as in the case of a partial looseness causing retro-

version, or of an excrescence between the laminæ.

The general symptoms of mitral disease, both obstructive and regurgitant, are, besides those of inordinate and irregular action, those especially of pulmonary and venous congestion. Sometimes the disease appears to be more pulmonary than cardiac, the patient being asthmatic in paroxysms, or suffering habitually from dyspnœa, with cough, and copious thin mucous expectoration sometimes containing blood. In other cases, especially those of a slighter kind, the lungs suffer little, and the chief symptoms arise from a congested state of the venous system; such are enlargement, pain, and deranged action of the liver, hæmorrhoids, scanty and disordered urine, ascites, hydrocele, and anasarca, congestive headaches, giddiness, &c. These occur in their greatest degree when the right ventricle is considerably diseased; but in a slight extent I have often met with them when the patient made little or no complaint of the heart, and when the chief sign of its disease was a constant murmur at the apex, indicating regurgitation through the mitral valve. When this defect does not obviously derange the action of the organ, it may still affect other organs through the circulation: continually putting back, at every beat, a little blood into the veins, may produce an accumulative effect in the different organs of the body, and none are so likely to suffer as those largely supplied with venous blood, and have been previously liable to any derangement. There are few cases of diseased heart, especially when this valve is affected, in which the functions of the abdominal viscera are not occasionally disordered; and I repeat, that this disorder is in many cases more prominent than the organic defect from which it has arisen. I cannot tell you how many patients I have had who call themselves, and have been called by their medical attendants, bilious, dyspeptic, and nervous, sometimes without a suspicion of a heart complaint, and in whom I have detected lesion of this organ, slight perhaps, but producing these effects by the constancy of its operation. Did time admit, I would give you many examples. In the worst forms of mitral disease, the conditions of pulmonary congestion and venous obstruction are combined; and then you see the terrible array of symptoms, which has rendered the name of organic disease of the heart so appalling, yet which does not occur in above one in a score of the cases where the structure of the organ is really diseased.

Of the signs of diseases of the *pulmonary semilunar valves* I know nothing from experience. They are so rare that I never met with a case in which I have rightly suspected its existence during life, nor in which I have found, after death, lesions clearly sufficient to produce distinctive signs. The only lesion that I have seen which might possibly have yielded a sign, was atrophy and perforation of the margins of the valves, the loops left by which, perhaps, might catch the current, and give a slight roughness or grating with the first sound. This would probably be audible at the middle of the sternum at its left margin opposite the valve, and perhaps below, over the right ventricle, but not at the top of the sternum or in the carotids; and as the pulmonary artery immediately plunges backwards under the arch of the aorta and into the lungs, it presents no course over which the sound could be traced. Speaking also *à priori*, of *regurgitant* lesions of this valve, if they could be distinguished at all, it would be by a murmur with the second sound heard at the middle of the sternum, and not in the arteries, and unaccompanied by the peculiarities of pulse which attend such a sound when connected with diseased aortic valves.

Of the *right auricular* or *tricuspid valve*, I can speak only as to its *regurgitant* lesions. We have before found that regurgitation through this valve is probably very common; it is chiefly known by pulsating swelling of the jugular veins, and is not commonly attended with any audible murmur. This, I think, may be explained by the laminæ of the valve being so yielding as not to resist the current firmly enough to produce sound, or if a slight sound be produced it is lost backwards. But in a few cases I have heard a deep blowing or grating murmur, distinct from the middle to the bottom of the sternum, in the epigastrium, and at the margin of the ribs close to it, not audible under the left breast, at the top of the sternum, nor in the carotid arteries, and accompanied by pulsating swelling of the jugulars, and signs of dilated hypertrophy of the right ventricle. This murmur I have been induced to ascribe to regurgitation through a tricuspid valve, thicker and more rigid than usual; and in two such cases I have found this condition of the valve after death: in one of them with some constriction of the orifice. Both these cases were combined with great dilatation and hypertrophy, especially affecting the right ventricle and auricle, and the symptoms had been the ordinary ones of extensive organic disease of the heart, affecting the lungs with unusual severity.

The different lesions of the valves and orifices which I have been describing may be variously combined; in which case their diagnosis will be more difficult, for

the positive characters of each are seldom so distinctive as to announce them clearly. Murmurs may be produced in two orifices at once—as by obstructive disease of the aortic and regurgitative disease of the mitral. The former might be recognised by the grating sound in the arteries, but the mitral murmur could not be distinguished from it, unless it were very different in character; as when it is whistling or whiffing, and quite superficial at the apex. On the ground of such differences I have several times founded diagnoses of lesions of both orifices, but I do not lay much stress on them. The physical diagnosis of valvular disease is in some instances very easy, but it is far from being so generally; and in the more complicated cases, accompanied by irregular action, without any definite or well-localized murmur, it often must be too conjectural to be depended on.

We have so little time left for the subject of *diseases of the aorta*, that I must dismiss it in a few words; having little to say that is new, and therefore referring you to the writings of recent authors on the subject, as you will find them epitomized in Dr. Copland's Dictionary.

The topics which we cannot entirely omit, are *dilatation* and *aneurisms* of the aorta, in the diagnosis of which I may, perhaps, be able to assist you. *General dilatation* of the ascending aorta and of its arch frequently accompanies disease of the left ventricle, especially that which affects the aortic valves. It may occur with very little apparent disease in the coats of the artery, and is then more uniform; but more commonly these are much altered, and then the dilatation is more irregular, and with a tendency to form lateral sacs, particularly near the attachment of the aortic valves and below the branching off of the innominata. The alterations of the coats consist in various deposits of opaque matter, differing in consistence and appearance, sometimes softer than the rest of the tissue, and easily separated from the serous coat; in other instances harder and tougher, involving the middle fibrous coat: in either case, there being occasionally osseous scales or concretions in them, and sometimes the whole calibre of the vessels being beset with all these deposits, broken up into the most rugged irregularities, as you see them represented in this picturesque engraving of Meckel's. But to do justice to the anatomical characters of these lesions would require a whole lecture; and, rather than describe them imperfectly, I forbear to dwell longer on them.

The aorta thus dilated, and wanting its proper regulating elasticity, receives the contents of the ventricle with more abruptness than usual; this circumstance, with the greater size of the vessel, increases the force and loudness of the pulsations at the upper part of the sternum, above it, and in the carotids. The character and sound of the pulsation will depend on the state of the walls of the artery and of its orifice. If the orifice be not contracted, nor the valves indurated, nor the ventricle much enlarged, nor the walls of the artery very rough or with projecting points, the pulsations will be simply unusually strong, with a double sound; the first sound being exaggerated by the impulse of the blood against the dilated walls of the artery, the second being that of the aortic valves simply transmitted. But if any of these conditions be present, they will constitute a resistance to the moving current of blood, and a murmur, more or less harsh and grating, will be the result, often with a thrill perceptible to the touch. To distinguish whether this murmur is produced in the orifice or in the rugged artery, we must return to the heart, and seek for the signs of obstructive disease of the aortic orifice. If they be not found, and the first sound in the arteries is deep, loud, and harsh, the probability is that it is produced in the artery; and even if there be signs of diseased aortic valves—if the murmur in the arteries be single and much louder, and more harsh than that heard at the middle of the sternum—there is still probably disease of the aorta, which transmits its own sound rather than those of the heart. Dilatation of the aorta above the orifice also increases the murmurs resulting from partial impediments in the orifice; and when the ventricle is also enlarged, the natural orifice becoming *relatively* narrow, may be the seat of a murmur without any real constriction.

Aneurism, partial or *saccular dilatation* of the ascending or bending portion of the aorta, or of the innominata or the other great arteries, may cause additional signs and symptoms, in consequence of the greater local extent of their tumor. When they are yet small, they may only increase the impulse and sound at the top of the sternum, and perhaps be accompanied with some general symptoms of disordered circulation; but as they increase in size, they press on the adjoining parts, and cause more or less disorder of their functions, and produce many physical signs. Thus their pulsating pressure on the pneumo-gastric nerves may sometimes, at an early stage, disorder the sensations of the lungs and stomach, causing feelings of slight dyspnœa, cough, nausea, or gastrodynia: its increasing bulk may compress—the veins—hence their distension, with lividity, congestion and œdema of the parts

from which they come, the head, neck, and upper extremities; the nerves — hence pain, numbness, and paralysis of the arms and hands; the œsophagus—hence difficulty of swallowing; the trachea or its branches—hence bronchitis, severe cough, and dyspnœa; the bones and cartilages—hence caries and absorption, and in case of the spine, perhaps, disturbance of its contents; and lastly, some of the other arteries—whence deficient pulse and circulation in the part to which it leads; occasionally one of the upper extremities. Some or other of these symptoms are generally present in the progress of aortic aneurisms; and their number and intensity may be generally explained by the particular position and size of the tumor.

The physical signs of aneurism of the upper part of the aorta are those of increased pulsation and sound, and, in advanced stages, those of a pulsating tumor presenting itself in various positions. The early increased pulsation may not differ from that of a generally dilated aorta, except, perhaps, that it is perceptible more at one spot above the clavicle, or that its sound is more to one side than towards the middle of the sternum. The sound may be like that of general dilatation, either simply double, like that of the heart, or single, attended with a grating or whizzing; the second sound of the heart being inaudible or obscure. The same remark will apply to more advanced aneurisms. They are commonly accompanied by a grating single or double sound at each pulse, but sometimes there is no grating; both the sounds are loud, and give one the idea that the heart is beating under the top of the sternum. In fact, that aneurisms shall produce a grating, there must be more than simply a sac smoothly communicating with the artery; the edges of the orifice must be abrupt and projecting, or contracted; or there must be some roughness or prominence in the artery near it; or there must be some other vent out of the sac, so that the current may not beat *in* merely, but *whiz through*. If the sac be very elastic, and the orifice small, there may possibly be a jet of blood to-and-fro, in and out of it, at each pulse and interval, causing a double whizzing; but I have never seen such a case. In most of the cases which I have seen, there has been at the top of the sternum a short grating sound, alternated with a duller, more prolonged sound, which I suppose to arise from pressure or constriction of some of the smaller arterial branches*. To distinguish such sounds from those of diseased aortic valves, you must have recourse to the same expedients as for the diagnosis of general dilatation; only, in case of aneurism, the sounds may be more local, and not so strongly transmitted to both carotids in the neck. When the aneurism reaches such a size as to form a tumor, perceptible above the sternum or clavicle, or between the upper ribs close to to the sternum, the only remaining difficulty is to distinguish it from other tumors: other tumors may produce the same symptoms and signs of pressure, and from the beating of the artery under them they may seem to be pulsating; nay, by compressing it or one of its branches, they may cause a grating or whizzing at each pulse, very like that of aneurism; and from the same cause they may disturb the action of the heart. But the pulsation of an aneurism may be generally distinguished by its greater extent, its distension in all directions, and sometimes by its softness; its sounds are generally much louder; and the pulsations of the arteries beyond, although occasionally unequal, are generally more frec than they would be if a solid tumor were pressing much on their exterior.

Aneurisms of the descending aorta may generally be known by an increased sound of pulsation, generally grating or whizzing, at some point along the left of the dorsal spine. As it advances and forms a tumor forwards, it may produce many other signs and symptoms, by pressing and displacing the lungs and the heart. If it increase backwards, it causes absorption of the ribs and vertebræ, producing paralysis and other symptoms of pressure on the spinal marrow, and presenting its pulsating tumor behind.

* This prolonged sound I now conclude to be seated in the veins: I have heard it in almost every case of substernal tumor that I have met with, aneurismal and other. It is generally a continuous droning note, swelling and rising at each pulse, like the *bruit de diable*, but not so loud. It is a valuable sign of partial venous obstruction.

LECTURES ON BLOOD-LETTING;

Delivered from time to time,
At the General Dispensary, Aldersgate Street.

By Henry Clutterbuck, M.D.
Formerly one of the Physicians to that Institution.

Lecture XIV.

I wish to add here, with regard to *complicated fever* (of which I spoke briefly in the last lecture), that the disposition of different structures to be affected by inflammation during the course of fever appears to

depend chiefly upon season and climate. Thus, in winter and in spring, and likewise in cold climates, inflammation is most apt to arise in the thorax, in the serous membranes, and ligamentous structures in general. Catarrh, pleurisy, peripneumony, and inflammation of the peritoneal covering of the liver, also acute rheumatism, generally prevail with great frequency at these seasons of the year, and often appear in combination with idiopathic fever (encephalitis). These commonly require pretty active blood-letting for their relief. On the other hand, the abdominal viscera are most apt to become inflamed during the course of fever at the end of summer and in autumn. Then it is that we frequently encounter diarrhœa, dysentery, and cholera, blended with manifest brain-affection or fever. Blood-letting is less frequently called for in these than in the spring diseases; though, whenever there is continued pain, tenderness to the touch, or active febrile symptoms, bleeding, to a moderate extent, can seldom be safely dispensed with. In hot climates fever is very generally combined with inflammation in the abdomen.

In such combinations of fever with abdominal inflammation the effect has sometimes been mistaken for the cause. This I believe to have been the case with respect to the inflamed and ulcerated state of the intestinal canal that is frequently found upon dissection after fever. M. Broussais, a French writer of considerable eminence, goes so far as to assert that this affection of the intestinal canal (which he terms *gastro-enterite*) is the primary and essential cause of fever; and he has been followed (somewhat servilely, as I think) by some of our own writers and teachers, both in his principles and language.

There is hardly any fever which, during a protracted course, does not sooner or later exhibit signs of intestinal irritation, consisting in inflammation of the mucous membrane of the canal. The general excitement that prevails throughout the vascular system in fever, with the disordered state of sensibility and irritability that always make a part of this disease, and which arises out of the morbid condition of the brain that is essential to fever, induces a general disposition to inflammation throughout the system; so that there are few protracted cases of fever in which, at some period of the disease, inflammation in one part or another does not arise, so as to render the disease complicated. The intestinal canal suffers as frequently, perhaps, as any other part in this way. But the chances of this being the case are very much increased by the general use of antimony, calomel, and other drastic purgatives, in the treatment of fever of late years; by which a constant irritation is kept up, and which, in such a state of system, readily passes into inflammation, with its consequences, ulceration, and sometimes perforation of the canal. That these appearances are only casual, is, I think, sufficiently proved by their taking place only after a long course of fever, and chiefly, if not solely, where purgatives of the class mentioned have been assiduously employed throughout the disease. Where no pains are taken beyond the use of enemata, or, at most, the mildest kind of aperients, to produce evacuation, I know, from abundant experience, that fever will often run its course, and that a protracted one *, without the occurrence of such intestinal mischief. But in all the cases mentioned, the disordered state of brain is perceived throughout, from the commencement to the termination. Nor is it conceivable, in fact, that such a state of intestines, when it really does take place, should give rise to the train of symptoms that occurs in fever, all of them being referrible to the brain. Intestinal inflammation often destroys life, but not in general by producing such symptoms as belong to proper or idiopathic fever, as I have just observed, therefore the effect seems to have been taken for the cause in this case; and if this be true, it should lead us to a more cautious use of purgatives in treating fever than is at present the case.

Of the Use of Blood-letting in the Specific Fevers.

Fevers are termed *specific* when they arise from peculiar causes only, and when, at the same time, they are characterized by a peculiar train of symptoms, and follow a particular course; in all which respects they differ widely from ordinary fever, whether simple or complicated.

There is a considerable number of these specific fevers, among which the most striking are small-pox, measles, chicken-pox, and scarlet fever; and it is not im-

* In an account of clinical lectures delivered at the Meath Hospital, in Dublin, Dr. Graves observes that an inflammatory state of the mucous membrane of the intestinal canal is frequently brought on by the use of purgatives, especially calomel, in fever, and which, he thinks, is often the cause of a fatal termination of the disease. If this be true, as I doubt not is the case, it is difficult to conceive that the tartarized antimony in solution, the use of which he strongly recommends (considering it to be possessed of a peculiar or specific power of reducing vascular action in fever, as well as other diseases), should be free from the objection which applies to calomel; for there are few more powerful irritants of the alimentary canal altogether than the tartarized antimony. As to its supposed sedative operation on the vascular system, I have only to say that I have seen no reason to think it possesses such a power.

probable that there are others with which we are unacquainted. They all owe their origin to the application of some animal poison, the first source of which is totally unknown, but which is capable of being regenerated in the human system under the disease, so as to produce the same in other individuals to whom it is applied. In all of them the brain-affection that belongs to fever in general is clearly manifest. They are very generally ushered in by the same train of symptoms as ordinary fever: chilly fits, succeeded by heat, sickness at stomach, headache, sleeplessness, and prostration of strength. Indeed, it is seldom possible to ascertain the nature of the disease that is about to take place till after the lapse of some days, nor till the distinguishing characters of the affection display themselves. Thus small-pox is rarely known till the eruption appears. Measles, again, cannot be distinguished at first from common catarrhal fever; and so of the rest.

Knowing nothing of the intrinsic nature of the virus in any of these cases, we have no direct means of counteracting it; nor are the effects it produces in any case, strictly speaking, remediable by art; they have each a determined course to run, which we have no power absolutely to prevent. Our efforts are necessarily limited, therefore, to palliation merely. In cases of ordinary mildness, little is required to be done beyond the passive treatment before described as applicable to fevers in general. Should, however, the febrile symptoms run unusually high, or signs of active inflammation present themselves in any important organ during the course of these specific fevers, blood-letting, as well as other anti-inflammatory means, may be required; but always to a limited extent. The case of measles is the most likely to require blood-letting, especially after the fever has declined, on account of the tendency to pulmonic inflammation that frequently follows.

Of the Use of Blood-letting in Periodical Fever.

Under this head are included both *intermittents* and *remittents;* the latter appearing to be merely imperfect and aggravated types of the former. You will think, perhaps, that I am stretching the point too far in classing intermittents and periodical fever in general with brain affections; thus assimilating them with ordinary continued fever. I think, however, there is ground for doing this, when the symptoms are accurately traced. Intermittents generally set out with the symptoms of continued fever; nor is it commonly till after the lapse of some days that the periodical character of the disease is clearly distinguished. At first the ordinary characters of fever only are seen. The disease begins by headache, cold extremities, shivering, and extreme prostration of strength; these are succeeded by a hot fit, during which the disease has the strongest resemblance to continued fever; and the same may be said of the paroxysms when repeated; so that it is only by observing the recurrence of these that we learn the real character of the affection. And it is not very uncommon for delirium to arise during the paroxysm; while the disease in many instances terminates in apoplexy, or paralysis, or other chronic brain-affections. Periodical fevers are liable, also, to the same complications as simple fever.

Notwithstanding this similarity of periodical to simple fever, there are yet important differences to be observed between them, as in respect to their origin, course, and treatment, so as almost to claim for the former the title of *specific*. The general, and probably the sole cause of periodical fevers, appears to be marsh miasmata, or something equivalent emanating from the soil, but the precise nature of which, chemically speaking, seems not to be known. Dr. Franklin, indeed, it is said, caught an ague by inspiring the gas contained in the air-bubbles that are seen floating on the surface of stagnant water in ditches. The gas contained in these bubbles is inflammable, and appears to be a compound of hydrogen with one or more of the solid combustibles; but I am not aware that an intermittent has ever been directly and purposely induced by inhaling any artificial combination of these substances.

There is considerable variety in the character and course of periodical fevers. Some are regularly intermittent, the paroxysms and intervals being distinctly marked. The duration of the paroxysms and the length of the intervals also differ considerably, but still with an approach to regularity. Thus we have the quotidian, the tertian, and the quartan types, but rarely other intervals. As, however, these differences affect but little, if at all, the treatment, it is needless to dwell longer upon them.

The remittent form of fever, as before remarked, appears to be merely an aggravated form of the disease. The same cause (marsh effluvia,) which in temperate climates generates intermittents of a mild character,—in hot and unhealthy ones, (as in the marshes of Holland, in the West Indies, the coast of Africa, and many others) gives rise to fever of the most fatal kind, but which still preserves, in greater or less degree, the tendency to observe periods. The nearer they approach, in this respect, to the common intermittent,

the less dangerous they are found to be, and the more under the influence of art. Fever of the remittent kind, as it appears in hot climates, is very generally accompanied by inflammation in the abdominal viscera, especially the liver; and jaundice often takes place in consequence. Thus is generated the Yellow Fever of the West Indies, of North America, and other parts.

With respect to the use of blood-letting in periodical fevers, the general opinion has been, that it is not only an unnecessary and useless remedy in intermitting fever, but in direct opposition to the nature of the disease. This opinion, in its extent, is liable to objection. Blood-letting is both a safe and useful agent in intermitting fever, under different circumstances; while it is not at all in opposition, as far as I understand the subject, to the intrinsic nature of the disease. It is pretty generally admitted, indeed, that in cases where the intermittent is complicated with some other active inflammation, the previous removal of this by appropriate means (of which blood-letting is one of the most effective) is essential to the cure of the intermittent itself. It is no less true, that where the paroxysms are irregular in their recurrence, and especially where marked febrile symptoms continue during the intervals, the disease will rarely yield to the usual remedies without the aid of blood-letting, or other debilitant means.

But it does not appear to be so generally understood, that in the simplest form of intermitting fever blood-letting is of itself capable of effecting the cure of the disease; yet such is the fact, as I could shew you by the recital of various instances that have occurred in the practice of this dispensary. I do not, however, advocate this practice as a necessary one, or as proper for general adoption. I believe, on the contrary, that the cure of intermittents, with the exceptions mentioned, is better effected by the cinchona, or other analogous remedies, than by blood-letting, which should be confined to the circumstances mentioned above. My object rather is, to call in question the soundness of the principle that has so connected intermitting fever with the notion of general debility, as to have, in some measure, deprived us of the benefit derivable from the use of a remedy which, when employed with discretion, is as safe as it is often really necessary.

The idea of debility constituting an essential part of the character of intermittent, is certainly not supported by the phenomena of the disease; for these present often unequivocal signs of the most violent arterial action; both local as respects the brain, and general as regards the whole system—such signs, in short, as are observed in active inflammation in general, and which are almost universally considered as sanctioning a liberal abstraction of blood. If you saw for the first time, and without a knowledge of previous circumstances, a tolerably robust person (and such are by no means exempt) labouring under the hot fit of an intermittent of no long standing; and if you attended only to the strong and full and frequent pulse, the great heat of skin, and the thickly-coated and white tongue, you would not hesitate to call such case an inflammatory fever; and if this general disorder were found to be accompanied by pain in the chest, cough, and difficult respiration, you would feel as little difficulty in designating it as a case of thoracic inflammation requiring a liberal loss of blood. Instead of the pulmonic symptoms, however, above described, you would, in the case of the intermittent, find the patient complaining of great heat and violent pain in the head, the arteries of which would be pulsating with great violence, while at the same time the proper functions of the brain, the sensorial functions, would be found in a state of disorder; the external senses, and even the intellect, disturbed, (the latter sometimes to the extent of delirium); and the muscular power (in direct opposition to the general vascular action) reduced. Do not such symptoms warrant our assigning a locality to the disease? and would not analogy seem to sanction the use of blood-letting in such a case? It is only from experience, then, that we learn that this remedy is not called for in the treatment of an ordinary intermittent; and this unexpected difference must be ascribed either to the specific or peculiar nature of the exciting cause, or to the peculiarity of the organ affected, as compared with others.

But in fact, much of the reluctance that is felt towards bleeding, in this, as well as many other cases, appears to rest on a mistaken notion of the mode of acting of this remedy; which is not by weakening the system altogether, nor by reducing vascular action, but upon the more general principle of *counter-irritation*, as I before endeavoured to explain to you; thus influencing and disturbing the diseased action, in common with the healthy actions of the system. And thus we are not surprised that the same remedy should prove beneficial in diseases of different, and even opposite natures (as we are accustomed to consider them). But while this is admitted, it is not required that we should resort to it (as a matter of indifference) in every case in which, by possibility, it

might prove advantageous. Where, for example, the disease is accompanied by great general debility, the cure should, if possible, be effected by means that are not of a debilitating nature, if such can be found. But still, as regards our present subject, the cure of intermittents, it is important to know, that although the disease, generally speaking, is best treated by the vegetable tonics, there are yet numerous instances in which blood-letting favours the operation of these remedies, and is even necessary in order to render them effectual.

With respect to the use of blood-letting for the cure of the aggravated form in which periodical fevers present themselves in hot climates, I have little to offer, having had no personal experience on the subject. We have, however, the concurrent testimony of many very able practitioners, in favour of it—among whom I may mention Rush, Chisholm, M'Lean, and Jackson. The opinions of the last of these writers, Dr. Jackson, are somewhat peculiar. Speaking of the effects of bleeding generally, in fever, he observes that it is not useful nor proper, in times of high excitement or universal action of the vascular system: on the contrary he says, "When the pulse is small, contracted, obstructed—even imperceptible; with a dry, withered, and impervious skin, or a skin greasy, damp, and clammy; a countenance livid; respiration oppressed; without local pain—the loss of 30 ounces of blood, or more, has often been unexpectedly followed by a development of the action of the vascular system," (p. 20); and he elsewhere says, "the quantity of blood drawn should not be measured by ounces, but by effects—*i. e.* by complete relief from the load of oppression." As this writer's opportunities of observing were great, we may safely credit his report. Such practice, however, would only seem applicable to the early stage of the disease. Some have advocated the use of mercury in these fevers, to the exclusion of blood-letting; but the evidence adduced in its favour is less decisive, and, indeed, rather equivocal, as it appears to rest much upon the maxim—"post hoc, ergo propter hoc." It is said, for instance, that when the use of mercury in the *yellow fever* was followed by copious salivation, the disease invariably yielded. But then it was observed at the same time, that, in the worst cases of the disease, mercury seemed totally inactive. So that the production of salivation, in any case, may be considered as merely a proof of the comparative mildness of the disease, and the recovery might possibly have been owing to this; for the disease was not always fatal, whatever treatment was pursued.

Besides the varieties of fever to which particular names have been affixed, as if they denoted specific differences, there are a large number of slighter attacks, which continue for a short period and then subside again, without the aid of art. These begin with shivering, followed by heat, pain of the limbs, and violent headache, and are usually termed "feverish colds," and the like. Confinement to bed for a few hours, with a simple sudorific regimen, in most instances brings them to a speedy termination. Yet these mild cases now and then, either from neglect or from constitutional tendencies, grow into a more serious form, so as even to assume at length a typhoid character, as it is called. This an early bleeding would have almost infallibly prevented. In cases where a known predisposition to fever (*encephalitis*) exists, prudence would justify the application of blood-letting, with a preventive view.

Of the Use of Blood-letting in Chronic Diseases of the Brain.

Besides the brain-affections that are attended by pyrexia, or a febrile state of system, as already noticed, there are various others of a chronic, or rather non-febrile character, which have their seats primarily in this organ.

The brain in its individual parts performs a variety of functions, each of which may be singly affected; or they may suffer in conjunction, and that variously. Thus there may be disordered sensation alone, or the voluntary power may be impaired or disturbed singly; and the same with regard to the mind; while two or more of these functions may be simultaneously affected, according to the particular seat and extent of the disease in the brain. The varieties particularly requiring notice here are the following:—Apoplexy, palsy, epilepsy (including hysteria), tetanus, chorea, and disordered intellect. To these may be added vertigo and headache, which, though often attendant on those just enumerated, may yet exist as independent cerebral affections, and therefore require notice. Now with regard to all of these I may repeat a former general observation, which is, that they either consist in actual inflammation of the brain, or are consequences of it, more or less remote. To this position you will find exceedingly few exceptions; and where this is not the case, they may be referred to excited vascular action—a kind of erethism — which, while it continues, disorders the functions of the part nearly as much as actual inflammation, though it is not so lasting.

Of apoplexy and its treatment.—Apoplexy, when complete, is a suspension of all the

sensorial or proper functions of the brain; there is neither sensation, voluntary motion, nor intellect. It is induced by a variety of causes, of which the chief, if not the sole, appear to be—1st, Mechanical pressure on the brain, produced either by depression of the cranium, or by extravasated blood or serous accumulation, or by tumors within the skull. 2dly, Inflammation of the brain in certain stages. 3dly, By arterial distension simply, the consequence of increased arterial action, which may be produced by certain emotions of mind—by external heat, especially if applied to the head itself—by alcohol, and narcotic substances in general. Now there is reason to believe that these causes all operate by impeding the cerebral circulation, in the manner formerly stated—that is, by pressure on the veins. The ultimate object in the treatment, therefore, is to restore the freedom of circulation in the brain; and this is to be done by the removal of the cause whenever it can be ascertained. A little reflection will shew you how far, and in what cases, blood-letting is capable of effecting this object. At present this remedy is used by far too indiscriminately, and is very often carried to a hurtful excess. Let us consider the causes *seriatim*.

It is plain that bleeding cannot replace a depressed portion of bone; and therefore mechanical aid must be resorted to for the purpose. The only use of blood-letting, in cases of this sort, is to ward off or remove inflammation. This object generally requires that the remedy should be used with tolerable freedom; but, of course, according to the circumstances of the case.

Extravasation of blood within the skull is probably the most frequent cause of apoplexy; and it is of importance to determine, as far as we can, when this is really the case. When the patient is attacked suddenly, falls down insensible, and continues so for twelve hours or more, it is highly probable that some blood-vessel has given way. Now this is the particular case to which I alluded, when I observed that blood-letting was used too indiscriminately, and sometimes carried to a hurtful excess. It is quite evident that bleeding from any part, whether the arm, neck, or temples, to whatever extent it be carried, cannot possibly remove the blood extravasated in the centre of the brain: this can only be effected by absorption, which is a slow process, and requires a certain degree of strength in the general system in order to its accomplishment. Bleeding, therefore, if employed to an unreasonable amount, is calculated rather to retard recovery. Blood-letting, in such cases, should be administered on a different principle, namely, for the purpose of diminishing the arterial action of the brain; because it is from the violence of this that the vessel becomes ruptured; and unless this action be reduced, it is probable that the hæmorrhage will go on till perhaps it proves fatal. For the purpose of reducing the arterial action of the brain the loss of only a moderate quantity of blood is required; at most, to the extent of twenty or thirty ounces. And as we cannot act immediately upon the cerebral arteries, we can only influence them through the medium of the general system; and this is best done by venesection in the ordinary way. You are not to go on with the evacuation till the stupor entirely disappears; for this may not take place for several hours, just as in a case of intoxication. You ought to stop as soon as the general circulation is decidedly weakened, which you can judge of by the pulse; for when this end is attained, the bleeding has done all it is capable of. The after-treatment consists in keeping the general circulation within moderate limits; this being the state of system the most conducive to ultimate recovery.

Apoplexy, again, may be occasioned by serous accumulation within the skull; but this is always the result of slow inflammation of the brain or its membranes. The apoplectic state, in this case, takes place slowly (lethargy), and is usually preceded by signs that portend the coming mischief; such as heat and pain in the head, suffusion of the eyes, and a febrile state of system altogether. This variety of apoplexy requires still more caution in the use of the lancet, on account of the protracted nature of the complaint altogether, and the state of general debility that usually attends it. Blood-letting, therefore, is only required in such cases where marks of inflammatory action appear. In all such cases, the evacuation must be limited in amount.

The existence of tumors within the skull, as a cause of apoplexy, can only be guessed at. Signs, more or less active, of long-continued inflammation, such as severe pain in the head, and general febrile symptoms, with more or less of disorder of the sensorial functions, are sufficient to lead to a suspicion that such is the case. The proper treatment, then, is that which is adapted to chronic inflammation—simple enough, though not often successful.

2. Where apoplexy originates in recent and active inflammation of the brain, as is sometimes the case, and which may be known by the usual signs of inflammation having preceded or accompanied the apoplectic state, such as pain in the head, a flushed face, and a coated tongue, our chief reliance is upon blood-letting, which should

be carried to a considerable extent. In vigorous subjects, and in the early stage of the disease, 20, 30, or 40 ounces of blood may be drawn with propriety; and this may be repeated, to a greater or less amount, as the circumstances may seem to indicate. But in an advanced stage of inflammation of the brain, whether it exist in the form of phrenitis or of idiopathic fever, great caution and reserve are necessary. The use of the remedy is then equivocal, and requires much judgment in its application. Where the disease is of a chronic kind, and has been preceded by lethargic symptoms, by vertigo, or by frequent headaches, the lancet must be used with great caution, because the probability is that the structure of the part has undergone a change, which bleeding cannot at once remove, while it may prove injurious if carried to any great extent. Small and repeated bleedings only are then proper, if only for the purpose of checking the further progress of the disease.

3dly. Apoplectic symptoms may be produced by arterial distension simply, without either extravasation or serous accumulation. This may arise from any cause that increases much the arterial action of the brain; such as external heat, especially when applied to the head itself; and the use of alcohol. We may conclude that the disease arises from arterial fulness merely, when the patient recovers his senses after a short time, as within an hour or two, while no other sign of oppression of the brain remains. Experience proves, however, that the stupor may disappear, in a considerable measure, although extravasation may actually have taken place, the parts gradually accommodating themselves to their new condition. In most such, paralysis, in greater or less degree, succeeds. When the apoplectic symptoms arise from temporary causes, such as the use of alcohol or other narcotic substances, or from the application of heat, or mental emotions, blood-letting may in most cases be dispensed with, as not being absolutely necessary; for in general the increased action of the cerebral arteries will subside after a short time, provided the cause be removed. But even supposing blood-letting to be employed, as the more safe practice, it can hardly be necessary to carry it further than to the extent of 20 ounces, even where the habit is strong, because this quantity is in general quite sufficient to reduce the arterial action within moderate and safe limits. In all cases of apoplexy that depend immediately upon arterial fulness in the brain, the application of cold to the head is an important auxiliary; as is purging also.

When apoplexy has once taken place, the tendency to a recurrence is so strong as to require great attention to all the circumstances that are capable of exciting the brain, in order to avoid them as far as possible, for upon this the safety of the patient turns. The chief of these are great mental application, emotions of the mind, and the use of intoxicating drinks; all of which should be studiously shunned. The occasional employment of blood-letting to a moderate extent, as well as the use of purgatives, are highly conducive to the same end.

ACCOUNT OF A SUPPLY OF FRESH VACCINE VIRUS FROM THE COW.

To the Editor of the Medical Gazette.

Sir,

If you consider the following account of a successful effort to procure a supply of genuine vaccine lymph directly from the cow, as likely to prove acceptable to your readers, it will afford me satisfaction to have provided you with it.

Allow me in the first place to premise, that having been engaged in vaccinating (at one time rather extensively) for thirty years, I have watched with regret a decided decline in the activity of the virus, and for many years I have been endeavouring in vain to renew the lymph from its original source. To many agricultural and other friends I have repeatedly expressed my willingness to go twenty or thirty miles to see a cow with the vaccine disease; and though I have occasionally heard of cows thus disordered, and once took some matter from an ulceration upon the teat of one, I have never been able to succeed in re-producing the disease in the human subject; or to see, until within the last month, a decided instance of the complaint. Whether others have been more fortunate in similar attempts, I know not.

The alterations in the vaccine affection which have appeared to me most marked, are, the smallness of the vesicle and its attendant areola; its rapid course; the absence of constitutional disturbance; the small quantity of lymph yielded by the vesicle; and, especially, the diminished activity of its infecting quality. Twenty years ago it was a matter of comparative indifference how long the lymph taken was

kept before it was used: after it had been preserved in a dried state for weeks, or even months, it almost certainly re-produced the disease. At the present time, virus a day or two old is very uncertain in its effects, and even fluid lymph often fails to communicate infection. On the diminished antivariolous power of the present stock of vaccine matter, I need make no remark; the public are too painfully aware of the fact.

Through the kindness of an acquaintance residing in Gloucestershire, who remembered my wish to procure some fresh vaccine virus from the cow, whenever an opportunity offered, a small quantity was sent to me on the 18th of August upon a piece of glass, said to have been taken from a cow; and also some taken from the hand of a person who had contracted sores by milking the diseased cows. I lost no time in investigating the source of this unexpected supply, and on the morning of the 21st ult. visited a farm-yard in which were about twenty-five cows, nearly all of which had been attacked with an eruption upon the teats during the four preceding weeks; the last of those thus affected had become so about a fortnight previous to my visit. Irregularly circular scabs existed upon the teats of many; in some the surfaces were raw from having been rubbed by recent milking: from none of the sores did any fluid exude, and most of them had been dressed with an ointment into the composition of which acetate of copper appeared to enter. The duration of the disease in the cows I ascertained to have been about a fortnight from the time that each had become affected. The hands of those who had been engaged in milking them presented sores in various stages; in one or two persons an eschar only remained; in others soreness still existed: in a boy of about thirteen years of age there was a large inflamed vesicle between the finger and thumb of the right hand, occupying all the space from the third joint of the finger to the second of the thumb: the skin was yellow at this part, as if a quantity of pus had been under it; such, however, was not the case; for from a small opening through the thickened and yellow cutiele the boy squeezed out a perfectly limpid fluid, with which I charged some glasses. This vesicle had made its appearance six days before, at which time he had felt ill.

The others before mentioned, who had been engaged in milking (all of them females) reported that they had felt very ill before they discovered the "gathering" on their fingers; one represented herself as having been so much indisposed with headache, pain in the back and loins, and general weakness, as to be apprehensive she was going to have a serious fever; all described the sores as extremely painful, and as having occasioned enlargement and tenderness in the axillary glands. The course of the disease in them, as in the cows, was about a fortnight; and though it was known to them that the cows had something the matter with their teats, several days elapsed before the milkers suspected their malady to depend upon the disorder in the animals: never having witnessed the complaint before in the cows, they were ignorant of the nature of the existing disease.

All the persons I have now spoken of had been vaccinated in infancy, one of them by Dr. Jenner.

I next inspected three children who had been inoculated by means of a needle, with lymph taken from one of those who had received the disease from the cows. One, a boy of fourteen years of age, who had gone through vaccination formerly, had four large, circular, mahogany-coloured scabs upon his arm, with the outer red line of an extensive areola still remaining. He was vaccinated about a fortnight before I saw him; the areola, I was informed, had appeared upon the eighth or ninth day, and he had been very unwell from the complaint.

The next was a child who had been vaccinated from one of the milkers only three days before; scarcely any inflammation or prominence were observable about the punctures, and I have since learned that the inoculation produced no effect.

The third case was a little girl, Jane ———, about five years old, who had never had the cow-pox, or small-pox. She had been vaccinated for the first time from the discharge taken from the hand of one of the milkers, during the activity of the complaint, on the 11th of August. I saw her, therefore, on the eleventh day of the disease. On one

arm were three large, fine, prominent, circular vesicles, flattened in the centre, and with some areola: on the other arm was one vesicle, much larger, and less circular. I was informed that for three or four days after she was vaccinated, it was difficult to decide if the infection had taken effect; and I have subsequently learned that the areola, which I saw on the eleventh day, continued to increase till the thirteenth day, and that the child had been "very poorly."

From this little girl I took a supply of lymph which was quite limpid, and flowed very freely.

I have not felt myself warranted in this account particularly to designate persons or places; but I am bound to acknowledge the great attention I met with from the inhabitants of the farm, and the facility afforded me of making every inquiry and investigation that I desired.

On my return to Bristol, I employed, as soon as was practicable, the lymph with which I was furnished. In this proceeding I was kindly aided by Mr. Wilson, and Mr. W. B. Carpenter, surgeons, of this city, who are much engaged in vaccinating. The matter I was possessed of was, a little from the teat of a cow, which one of the milkers had placed on a piece of glass for me, before I had seen the cows: some which I took from the boy who had caught the complaint by milking, and that from the child Jane, now vaccinated for the first time.

The matter from the cow produced no effect, though tried on several children; nor did that from the boy's hand. Of those vaccinated with the lymph from Jane, two only out of many were infected. One of these patients had one well-formed vesicle, the other had two. In both, the disease was late in coming on; in one of them no redness appeared at the base of the vesicle till the tenth day, and the areola was not fully formed till the thirteenth day. In this case, however, Sarah Owen's, each vesicle was very perfect, rising abruptly from the arm, its upper part almost overhanging the base; its surface was much flattened, and it yielded freely limpid fluid when punctured before the areola appeared. On the thirteenth day the child's body and extremities were covered with a rash, in patches, much elevated from the skin, and she was constitutionally indisposed. On the fifteenth day the surface of the vesicle was becoming brown, and the areola rash, and general indisposition, had disappeared.

From these two children many others were vaccinated; and now a second set has been inoculated from these last. In the majority of cases the vesicles have been inflamed round their base about the fourth or fifth day, and the areola has become extensive on the ninth. The areola usually continues for three or four days. In some cases it has been considerable on the eighth day. The vesicles are large, very well marked, and yield an abundant supply of clear lymph, and in every case there has been a good deal of constitutional disturbance. Some who have been vaccinated upon one arm with lymph taken on the eighth day from the other arm, have exhibited in the second vaccination a small vesicle surrounded with a miniature areola, appearing and subsiding with that upon the opposite arm.

It appears clear to me, that the new lymph is of a very active character; it so much resembles the original cow-pox in a more energetic form, that I feel no doubt of its anti-variolous properties: this, of course, must be decided by future experiments. I am happy thus far to be instrumental in propagating what I think promises to be a valuable renewal of genuine vaccine lymph. I have begun to re-vaccinate with this matter, some who had the cow-pox many years ago, and hope, ere long, to have the means of satisfactorily testing its protective character. Having sent some to Dr. Gregory, Physician to the Small-Pox Hospital, I hope he will soon be able to make a favourable report of it; and if any gentleman attached to a public institution for gratuitous vaccination is desirous of trying the new lymph, removed by so few degrees from its original source as it now is, it will afford me much pleasure to furnish him with a little of it.

Should I become possessed of any additional facts that would be interesting, either from my own observation or from that of others who have been employing lymph from the same source, I shall take the liberty of again addressing you.—I am, sir,

Your obedient servant,

J. B. Estlin.

Bristol, September 10, 1838.

EXPERIMENTS ON ANIMAL MAGNETISM.

By Mr. James Blake.

[Concluded from page 943.]

The next class of magnetic phenomena which I shall consider, are those which are said to be produced by the contact of a magnetized person with an unmagnetized one. It is alleged, that on bringing a person who is in a state of magnetic sleep into contact with one who is susceptible to the magnetic influence, sleep is immediately produced in this second person; and that on one being awoke, the other is awoke also. The sources of fallacy which cause these experiments so constantly to succeed, even when the persons could not see each other, are the following:—The noise made by the girls in falling when sent to sleep: the cessation of that constant volubility with which the young ladies were in the custom of amusing the by-standers; the evident anticipation of effects on the other girl when one was sent to sleep; all or any of which were quite sufficient to afford indications to the girls as to when magnetic phenomena ought to take place. In proof that these appearances were any thing but real, I shall bring forward the following facts. I have, on several occasions, succeeded in sending one of the girls to sleep, without the other girl, who had hold of her hand, being at all affected. This was done by not allowing the girl to fall down so as to make a noise, and by continually talking to her, requesting her to keep silent, &c.; when the other girl, not knowing that this silence was owing to the magnetic sleep having been induced, remained totally unaffected, giving her companion time to go to sleep and wake up again, without manifesting the slightest magnetic sympathy. The phenomenon of the awaking of one girl being followed by the awaking of the other, when they were in connexion, will receive elucidation from the following experiments:—There are many sources of fallacy to be avoided in investigating this phenomenon; and it is therefore one which, in the hands of the supporters of animal magnetism, is almost sure to succeed. These are, the indications afforded by the moving of the hand of the person who is awoke being sufficient to inform the person with whom she is in connexion that this event had taken place. On avoiding this source of error, it was found, that although the sight and touch might not now furnish any indications, yet that the audible sigh with which the *return to consciousness* (?) was accompanied, was sufficient evidence to the other person that this event had taken place, who immediately awoke accordingly. When a bandage was placed over the girls' ears, and the hands were held firmly in contact, so that no motion could take place between them, it was found that the powerful magnetic phenomenon of recovery from a state of *absolute insensibility* (?) could be produced in one girl, without the other evincing the slightest symptom of the propagation of the shock. In one instance, one of the girls was awoke, and her hand held against that of the other for five minutes; yet not the slightest effect was produced on the one who was sleeping, although she would have been instantly awakened had she been aware of the *extraordinary change* which had taken place in her partner: this is shewn by the sudden manner in which she recovered her consciousness, on the hand of the other girl, and the screen which was between them, being removed; an effect which, in mesmeric language, would be "very extraordinary," but which is very easily explained by any person who will use his common sense.

When both girls were asleep and had hold of hands, I often succeeded in awaking one by giving the arm of the other a sudden jerk, as if she had been suddenly awoke, although no magnetic effort had been exerted on either. Another proof of the acting of these phenomena produced by contact, is the fact that O'Key and her sister have often been sent to sleep by being placed in contact with Ann Ross; who, it would appear from her own confession, was shamming all the time, not being at all in a magnetic state.

Amongst the many curious methods by which the magnetic influence can be propagated, is that of blowing a person to sleep. It does not appear that the exact force with which the air should be expelled from the lungs, in order to produce this effect, has been ascertained. It must, however, be sent with sufficient force to enable the susceptible persons to feel it. It is, perhaps, one of the

most curious facts connected with this *science* (?), that air, when sent with the requisite degree of force from a pair of common bellows, possesses as much psychological power (for it would appear that it must be on something of this sort that the magnetic phenomena depend) as the air from the lungs of beings whose mental faculties are of the highest order. That this is the case is proved by a current of air from a pair of bellows producing the same state of sleep and insensibility as that sent from the lungs. Should the reality of these mesmeric phenomena ever be demonstrated, into what new and hitherto unexplored paths of inquiry will the metaphysician have to direct his investigations! and how interesting the results which may be anticipated for the science of mental philosophy, when we shall be able to trace the connexion between mind and matter through the medium of so simple an instrument as a pair of bellows!

I shall now bring forward an experiment on the power possessed by the elder O'Key, of seeing with her hands. The manner in which this optical power of the hands is shewn, is by placing them, when closed, on a table, and applying substances near the backs of them. As the girl was supposed to be asleep during the experiments on this subject, no precautions were considered necessary in order to prevent her seeing what was passing directly under her eyes. It having been doubted, however, that this sleep was real, and strong suspicions being entertained that she could command a small range of vision directly under her eyes with her proper visual organs, the following experiment was performed:— The girl's hands were placed on the table as usual; pieces of bread were placed near them, and were certainly perceived (whether by the hands or eyes will appear in the sequel), the hands moving about for a short time near the bread, and then seizing it. A screen was now placed between the eyes of the girl and the table. A piece of bread was placed within about a quarter of an inch of the right hand, one of the gentlemen present observing (in order to deceive her) that as the left hand could see the best, the bread should be presented to that newly discovered optical organ. From some unaccountable cause or other (the girl pretending to be perfectly insensible to all that was passing around her), no sooner heard these words been uttered, than the left hand began moving about, as if directing its particular attention to some object near it, although the only thing on the table was the piece of bread close to the right hand, which appeared to be in a complete state of amaurosis. The gentleman who was conducting the experiment was requested (with the intention of deceiving the unconscious young lady) to move the bread out of the way of the left hand, so that it might not be aware of its presence by touching it. The seeing hand, to prove that its power of discovering objects did not depend on the sense of touch, immediately made a snatch as if to seize some object. It was, however, disappointed, for though its optical powers were evidently of so high an order as to enable it to perceive *nothing*, yet its power of prehension was of a more material stamp. Fearing we might not have a repetition of this interesting exhibition, we accounted for the want of success of the hand in seizing the bread to its having been suddenly withdrawn at the moment she snatched at it. The *unconscious* young lady, apparently satisfied by this explanation, twice again gave us the same proof of the high power of vision possessed by the left hand, although the right hand, against which the object had all along been placed, was totally unable to perceive it.

The following is a specimen of the power of prophecy possessed by these magnetized females:—

Before leaving London, I was requested by one of Dr. Elliotson's clerks to affix my seal to a paper containing a prophecy which had been uttered by Jane O'Key, and which Dr. E. desired might be sealed up, and not opened until the time for its fulfilment should have passed. Being in town a fortnight after the circumstance, I was anxious to hear the nature of this prophecy, which I found to be as follows:— Miss O'Key stated "that a few hours after her prophetic utterance she should go to sleep, and remain so for 16 hours; that the should then awake with a headache, and be able to see with some other part of her body." As to her going to sleep and awaking with the headache, that was managed easily enough; but certainly, the proof brought forward of her seeing with some portion of her body on which the rays of light had not

yet produced any impression, is not sufficient to convince me of the fulfilment of this part of the prophecy. I shall, however, give it to your readers as it was told me, who will be able to judge for themselves. The only proof brought forward in support of this important event is this,—" that Miss Okey was seen sitting up in bed, by one of the nurses, brushing her hair, and with her foot in such a position as if she was looking at her head with her foot." It was therefore most philosophically inferred that she did see her head with her foot, this being the new part of the body which became endowed with visual faculties; and therefore the whole of the prophecy was fulfilled to the letter; and that there can be no doubt of Miss O'Key possessing the gift of prophecy when in the magnetic state,

I shall now conclude with a few remarks on some cases which have been presented to me by the supporters of animal magnetism, in which it would be difficult to suppose the effects produced to have been feigned. These cases are invested with a considerable degree of interest, not as tending to shew that there is any such thing as animal magnetism, but as affording instances of the power which the imagination possesses over the functions of the nervous system.

I should be taking up too much of your journal were I to enter into a lengthened detail on this subject; but every medical man must, on reflection, be aware of the great power exerted by the imagination over the functions of the body. We have many recorded cases of persons believing that they should die on a certain day, who have died on that day. We have one instance on record in which a man died, believing he was being bled to death, although he had not lost a drop of blood; and I would adduce many of these supposed magnetic cases as instances in which persons have been sent into a state of sleep, when they believed that they were to be sent into that state. But it will be argued, how can we account for the persons being again awakened by the transverse passes which are made, if they are in a state of insensibility, unless we admit these passes to be possessed of some peculiar power? Every person who has at all investigated the phenomena of sleep, must have come to the conclusion that, even in our most profound slumbers, we maintain a connexion with the external world, and are in reality aware of what is passing around us, although these impressions are not retained by the memory. This is exemplified in many familiar instances. The certainty with which some persons will awaken at an hour at which they may wish to arise, although not their usual time of awaking, is sufficient proof that we have during sleep a knowledge of the passage of time, and which, in many instances, would appear even more accurate than we possess when awake. The facility with which we are awoke by certain sounds, which we know are employed for the purpose of rousing us, although much louder noises of a different sort may be unheeded by us, tends to shew that we retain sufficient use of our faculties to distinguish between these different sounds.

In applying these remarks to the subject of animal magnetism, I would compare the transverse passes over the eye-brows to the noise which we may be expecting to awaken us. Insensibility to other things may be manifested, but when this movement is made, which it was before known is that which is to awaken the person, it is immediately perceived. That this is really the case is rendered highly probable by the consideration of those instances in which these transverse passes over the eye-brows have failed to produce any effect. Three of these have come under my own observation. The persons had been sent into a state of insensibility by the magnetic manipulations, from which they could not be awakened by any means that could be employed, the transverse passes over the eye-brows being unheeded, as if performed on any other part of the body. Now it is an interesting fact, that in all these cases it was the first time that the person had been sent to sleep; and in two of them, it is probable, the first time that they had heard of animal magnetism.

Under these circumstances I think we are justified in concluding, that the failure of the transverse passes was owing to the persons not knowing, before they were sent to sleep, that these passes were to be employed to awaken them.

I shall now conclude, and I sincerely hope for ever, these remarks on a subject the investigation of which I feel convinced can never be attended with

any advantage to science. I should not have entered into it to the extent I have done, had I not considered that, after the discovery of truth, the endeavour to correct error is the occupation upon which we can most usefully be employed.

Gosport, Sept. 6, 1838.

INVAGINATION OF CAPUT CŒCUM AND ILEUM.

To the Editor of the Medical Gazette.

Sir,

If you think the following case of intus-susception worthy of a place in your valuable journal, I shall feel obliged by its insertion.—I am, sir,

Your obedient servant,

H. Cunningham.

Camberwell, Sept. 7, 1838.

A. T., ætat. 9 months, a remarkably fine healthy male child, well in every respect from his birth, with the exception of a tendency towards relaxation of the bowels, was, on the morning of the 9th August, suddenly seized with a fit of screaming, which lasted for some time, and occurred again at intervals, accompanied with a stoppage of the fæcal discharge. The mother became alarmed, and applied for medical advice.

On examination of the little patient nothing peculiar presented itself; and as the child seemed perfectly at rest at the time, a few calomel and rhubarb powders were given, with the intention of opening the bowels. The mother shortly after returned to say that the powders had been rejected: the constipation continued; the child could suck but little at a time, and the milk, after remaining a little on the stomach, was thrown up again; that he still continued to scream violently at times, with intervals of rest.

On examination of the abdomen, a tumor deeply seated in the left iliac region could be distinctly felt, which I pronounced to be occasioned by invagination of the intestine; and a prognosis was given accordingly. An enema, consisting of castor oil, starch, and a few drops tinct. opii, was exhibited, in the hope of producing an evacuation, but without effect. The warm bath was tried almost to syncope, after which the child seemed relieved; but the efforts at stool returned, accompanied with a discharge of mucus tinged with blood.

10th, 8 A.M.—The child has been very restless during the night, with fits of screaming, tenesmus, and rejection of the milk, mixed with biliary secretion. The pulse was full, but soft; heat of skin natural; urine scanty; eyes heavy, with an inclination to sleep; respiration hurried; tympanitic distension of the abdomen. Five leeches were instantly applied over the seat of the tumor, which could now be but indistinctly felt, from the increased tumefaction of the abdomen. The leeches bled freely; the warm bath was repeated, with but a little alleviation of the sufferings. An enema was again exhibited, which instantly passed off on the removal of the pipe. During the day he seemed somewhat better; but towards night the straining again increased; nothing but blood and mucus stained the napkin, which resembled somewhat in appearance red-currant jelly, at times merely pure blood.

11th, 10 A.M.—The symptoms still continue; no fæcal evacuations; pulse full, quick, but soft; heat of skin natural; countenance not expressive of much pain; indeed, the child, to all appearance, seems quite well. No emaciation; but, on the contrary, the body is full, plump, and robust. The fluid ejected from the stomach assumed a yellow hue; he drank, for the first time, a little water on its being presented to him. The abdomen considerably distended; the eye sunk; but quite intelligent when spoken to. He still takes the breast, but cannot suck long at a time, which seems to bring on the tenesmus, with screaming; every thing taken into the stomach was almost immediately thrown up. Towards night he grew gradually worse; the sanguineous discharge from the rectum still followed every effort at stool, until death, which took place about 3 A.M. on the 12th, half an hour previous to which he seemed free from all pain, and, to use his mother's words, "gradually slept away."

Post-mortem examination 12 *hours after death.*—With the exception of the abdomen, which was somewhat more distended, the body presented exactly the same appearance as during life. An incision was made through the integuments, from the ensiform cartilage

down to the pubes: the cavity of the peritoneum being opened, the intestines burst forth, filled with flatus, much twisted, and of a deep yellow colour, from the nature of their contents—passing the hand into the left iliac region. Through the coats of the rectum could be felt a portion of the invaginated intestine projecting almost to the verge of the anus; a ligature being passed round the intestine above and below the tumor, the portion between was dissected out, which was about six inches in length, and of a dark brown colour: the inferior part was cut open, and the contents exposed: in the interior was the invaginated portion, almost of a blackish hue, in the centre of which a depression, not unlike the os tincæ, was discovered, which proved to be the ileo-cæcal opening (now perfectly closed): the inferior portion of the intestine being held, the superior was drawn out as though it were the finger of a glove doubled within itself, and it was now found that the caput cæcum, with a considerable portion of the ileum, had been forced up the ascending colon across the transverse and through the sigmoid flexure, low down into the rectum. The walls of the bowel being laid open, the mucous membrane was found to be softened, greatly swollen, and completely engorged with dark grumous blood, some of which could be wiped off with the handle of the scalpel: the portion of the mucous membrane lining the ileo-cæcal valve was particularly dark, and presented all the appearance of the intestine in strangulated hernia. The appendix vermiformis was also indurated, of a deep purple colour: the jejunum, duodenum, and stomach, were normal.

ATTENDANCE ON THE POOR.

To the Editor of the Medical Gazette.

Sir,

"The labourer is worthy of his hire" is an axiom which has continued from the primitive ages to the present time.

Labour is corporeal or mental, or it may be a combination of both: it has always been rewarded by an equivalent in produce or in money, and its amount is supposed to bear a fair proportion to the services performed.

The nominal value of articles of subsistence has regulated the price of corporeal or common labour, and mental labour has been estimated by the same equitable standard. As the sum paid to the agricultural or the mechanical labourer should be such as to enable him to provide for the support of life and the common wants of a family, so should the reward for the exercise of the mind be proportionate to the *kind* and the *degree* of labour, having reference to the means by which the individual had become qualified for the useful employment of his time in the services required. The eagerness of competition in all populous States will depress the nominal value of both kinds of labour to the lowest point consistent with human existence and the well-being of society.

The division of labour which obtains place in civilized communities, may be designated agricultural, mechanical, and professional. The latter can be subdivided into clerical, legal, literary, scientific, and medical.

Those who devote themselves to professional labour, of whatever kind, are held in the estimation of the public according to the extent of their respective attainments, and their measure of reward is adapted, or intended to be adapted, to the benefits which such attainments enable individuals to confer. Bishops and judges, barristers and physicians, and the higher class of surgeons, solicitors and surgeon-apothecaries, or general practitioners, seem to be parallels. Poets and painters, civil engineers and scientific men in the higher walks of art, hold a certain station, and the pecuniary reward of literary and scientific labour exceeds that which is paid for mere bodily exertion.

In the multifarious employments of men, some are occasionally overpaid, others underpaid; but all have a tendency to adjust themselves to an equitable standard. It cannot be denied, then, that public justice dictates the right of claim to remuneration in proportion to the extent of the public services required, and the education or qualification of individuals to render them capable of performing those services.

A correct view of these general principles seems to be necessary towards the right settlement of a question which has been much agitated lately; viz. "the medical relief of the poor." This im-

portant question should be discussed with candour and freedom, with temper and firmness. It will admit of no false colouring, no frivolous complaints, no timid subserviency to Poor Law commissioners, nor to local magistrates: it must be grappled with and carried upon its own merits, or lost by the supineness of its advocates.

There is, assuredly, a vast disproportion between the remuneration usually given to public *legal* servants and public *medical* servants*. The former are generally nominated as an affair of official patronage, without open competition or comparison of fitness; besides, a great many members of parliament are practitioners in various departments of the law—some of whom glide into lucrative appointments, or facilitate the advancement of other lawyers to situations of little labour, with considerable salaries, paid from the public purse, whilst the physicians and surgeons of most of the public institutions perform the duties *gratuitously;* they have no coadjutors, and few advocates, in parliament. It is the same in parochial appointments, to which these remarks chiefly apply. The vestry clerk is a *well-paid* official, whilst the physician or surgeon to the workhouse (in London) is not paid at all, or has a nominal salary, or such an annual sum as bears no reasonable comparison with his responsibility, his time devoted to the duties, and his station; but barristers, who are consulted respecting the parochial poor, are paid their fees. Physicians and surgeons of the numerous dispensaries in and near London, give up several hours, two or three times a week, and attend urgent cases *every* day *without fee or reward*; and the county surgeon, who visits the parochial poor in a wide district, a large portion of whose time is employed in his *public* service, receives little more than enough to pay the cost of his medicine, whilst the poor-law assistant commissioners, whose education has not been more expensive, nor his duties more arduous, receives 1,500*l.* a-year.

I do not question the justice of paying gentlemen for *legal services*, according to the nature and extent of their duties; I merely notice the disparity, under present regulations and usages, respecting *medical services*, and *contend that parochial communities should be compelled to make the reward of such services bear a just reference to the exercise of professional judgment, time, and medicines, employed and required for the benefit of the poor confided to their care.*

It is as important to the state that the health of the poor should be restored, as their right of settlement investigated: it cannot be denied that the good management of cases of surgery is of as much value to the community as the right decision of cases of bastardy; yet as things are now arranged, a miserable pittance is awarded by parish boards and poor law commissioners for one class of service, whilst *very large sums* are *annually* drawn from parochial funds, or county rates, for the other class; surely, then, even-handed justice calls for a revision of these matters.

I believe that the only proper mode of obtaining redress for such real grievances is by an appeal, or petition, to parliament, containing a plain statement of facts, respectfully asking (called praying) for the addition of whatever may be defective, or the correction of whatever may be imperfect, in all that concerns the *requisite* medical and surgical attendance on the parochial poor. The greatest care should be taken to prevent this question from becoming a subject of party discussion, and the management of it from falling into the hands of political brawlers.

Those gentlemen who have already formed a committee for the accomplishment of this purpose, would do well to obtain, or at least endeavour to obtain, the concurrence of the chartered medical bodies, in their appeal to parliament. There is a better spirit of conciliation prevalent in the councils of those bodies than existed a quarter of a century ago, when an important medical question, the Apothecaries' Bill, was publicly agitated; and there is an inclination, I hope, to assist in doing whatever is practicable for general improvement.

Parliament has already shewn a disposition to concede the reasonable claims of medical men, by allowing a recompense for *post-mortem* investigations and evidence before coroners' juries; it will listen, if respectfully addressed, to the complaints of those provincial practitioners who are extensively and usefully engaged in attendance on the poor, and not refuse to them that measure of justice which, upon due

* No allusion is intended to the army or navy.

consideration, they may be fairly entitled to demand.—I remain, sir,

Your obedient servant,

ROBERT MASTERS KERRISON, M.D.

New Burlington Street,
Sept. 3, 1838.

MEDICAL GAZETTE.

Saturday, September 15, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

RECENT EVIDENCE ON THE MEDICAL RELIEF OF THE POOR.

IN our late article upon this subject, the week before last, we observed that parish surgeons are, perhaps, the only class who never even ask for a salary amounting to one-fifth of the just recompense of their exertions. The natural consequence is, that what no one ventures to ask, no one dreams of offering; and it is a curious thing, that in the Reports already quoted*, as well as in several pamphlets on the Medical Relief of the Poor, now before us†, an infinitesimal remuneration is always spoken of as highly satisfactory, and perfectly delightful to all medical men in England, Wales, and the town of Berwick-upon-Tweed. No witness, no author, is ever carried so far by his enthusiasm as to compare the stipend of the medical officer with the lowest pay that could possibly be offered to an attorney or surveyor employed on parish business every day in the year. When the practitioners of Bridgewater were driven to struggle last year with shabbiness personified in the form of their guardians, they distinctly declared that they had no wish, individually or collectively, to receive any thing beyong a moderate remuneration for their attendance on the poor; and they followed up this declaration by an offer of their services on terms so immoderately low, that in almost any profession but ours it would have been thought a joke rather than a proposal. Thus they offered to take charge of the Union Workhouse, intended to contain three hundred persons, at fifty pounds per annum. An attorney's lowest fee for the most trifling service is, if we mistake not, three shillings and fourpence, and this, if daily repeated, will amount to 60*l.* 16*s.* 8*d.* a year. It is obvious, however, that no comparison of equality can be made between the slight and nominal service for which the man of parchment would charge 3*s.* 4*d.*, and the long and careful visit which ought daily to be paid to the twenty or thirty patients contained in the workhouse. Yet low as was the sum asked by the surgeons, the most parsimonious of boards thought it too high, and offered 30*l.*, or one shilling and sevenpence halfpenny per daily visit! Perhaps some one may here start an objection, and suggest that possibly the visits are not made daily, and therefore the price of each may rise to three, or four, or five shillings. Thus in that lamentable case, where a pauper labouring under influenza was made to work at a mill*, it appeared probable that the workhouse was not visited daily; and what happens in Surrey may take place in Somerset. Precisely; and this seeming objection is the other horn of the dilemma on which the starvationists stick.

* Forty-fourth, forty-fifth, and forty-sixth reports from the select Committee on the Poor-law Amendment Act; with the minutes of evidence, and appendixes (Medical Inquiry.) Ordered by the House of Commons to be printed, 20th and 22nd June, 1838.

† 1. The Report of the Poor-law Committee appointed by the Provincial and Medical Associaiton, &c. Second edition. London, 1837.

2. Medical Relief for the Labouring Classes, on the principle of Mutual Insurance. By H. W. Rumsey. London, 1837.

3. Facts connected with the Medical Relief of the Poor in the Bridgewater Union. Second edition. London, 1838.

* MEDICAL GAZETTE, vol. xxi. page 385.

If the medical man is an unwearied philanthropist, and in visiting his pauper patients consults only the dictates of humanity, the salary is a mere delusion, and will barely pay his drug-bill; if he is made up of more ordinary materials, and proportions his labours to his reward, the sick poor will be habitually neglected.

The fact is, there are two fallacies which the Poor-law Commissioners or their accomplices have constantly attempted to palm upon the public, and hitherto with considerable success. The first consists in the supposition that the most exquisite philanthropy is of every-day occurrence, so that a practitioner having undertaken to perform for thirty pounds a-year the duties of an office which would be underpaid with 200*l.*, is to be supposed to execute them with the same zeal as if he were substantially paid; the deficient 170*l.* being, as it were, supplied by the excess of his benevolence. That some cases of this kind are to be hoped for, it would be harsh to deny; but it would be absurd to assert that many are to be expected. This supposition, however, though we are reluctantly obliged to consider it as erroneous, is at any rate flattering in form; the other one is merely tricky, and analogous to the famous diversion of thimble-rig. The unfortunate practitioners are singly and separately persuaded, that if they will listen to the advice of the Commissioners who keep the table, each shall find some golden practice under his own thimble, whereas, in truth, the players all lose, while the thimble-riggers gain. In other words, practitioners are induced to suppose, that if they will underbid one another at the nod of their masters in Somerset House, the lowest bidder will get rid of his rivals, ousting them from their footing in his parish, and thus monopolizing the private practice. Now all this is mighty well for Mr. A., who thus succeeds in scaring away Mr. B.; or for Mr. Q., who can bear "no brother near the throne," and *out-work-houses* Dr. R.; or for any other letter of the alphabet over-jealous of the neighbouring character; but it is too clear that the whole set can gain nothing by the sport, and that after this Dutch auction, as before, they have no more than the whole private practice; saddled, however, in the former case, with the whole public practice undertaken at infinitesimal salaries. Even under the old system, the insane competition in our profession enabled parishes to engage their medical officers at nominal salaries; and we cannot expect any improvement as long as the colossal power wielded by the Commissioners allows them to summon their auxiliary doctors from London, to add to the ruinous competition already existing in the country. It is truly edifying to hear the Commissioners canting about medical men knowing their own interests, and its being impossible to scrutinize the motives which induce them to take such or such a situation, &c.; why, they might as well say that the Brighton coach-masters know their own interest when they take passengers down at five shillings a-piece. The real question in such cases is not, what are the profits, but whether the Dart or the Comet will be ruined first. To make the parallel perfect, however, we ought to imagine sundry Commissioners for the amendment of travelling inveighing against the extravagance of the fare, and setting a crazy hackney-coach to run to the same place for half-a-crown — with a famished coachman, a paralytic guard, and horses wrested from the knacker.

We are surprised, however, that the economic enthusiasm of the Triumvirate has never induced them to look farther than the surplus medical population of London, in their attempts to beat down country surgeons to water-gruel prices.

When wheat rises to eighty shillings the quarter, the ports are thrown open; how is it, then, that when your rustic practitioner asks nearly three shillings a-day for visiting only twenty or thirty patients, the affrighted commissioners do not solicit assistance from some region beyond the British seas? Denmark, for instance, is a wonderfully cheap country; the cost, as the political economists would say, of growing and raising a doctor in that part of the world, must be a mere trifle; his books, clothes, diet, and diploma, are probably at least fifty per cent. under any thing of the kind to be had in England. A salary of 15*l.* per annum would look tempting when translated into specie dollars; or, what would be still more economical, 10*l.* and a daily ration of food from a Union Workhouse — say specked potatoes and decoct. avenæ dilutissimum. The objection that the practitioners and patients would not understand each other, would be immediately got over, when it was considered how much this would diminish the number of applications; and to use a favourite expression of the advocates of the new law, the consulting a foreign doctor would be an admirable "test of destitution."

In the Reports of the Select Committee before us, the first witness examined is Dr. Kay, who, as our readers are aware, is an assistant Poor-law Commissioner. He is asked (p. 1), what is the qualification demanded of the medical practitioner?—to which he answers, that he must be an apothecary authorized to practise, or "a member of the Royal College of Surgeons, or a physician; in which case he cannot dispense his own medicines, without rendering himself liable to prosecutions at law."

We apprehend that in the latter part of this answer the exclusion is too sweeping. A Scotch graduate has unquestionably no right, as such, to supply his patients with medicines; but there can be little doubt that the right is possessed by all the members of the London College of Physicians; for the Apothecaries' Act guaranteed the privileges of the existing medical corporations, among which the College of Physicians assuredly possessed the power of dispensing medicines.

We may here remark, to the credit of our profession, that the medical districts in Norfolk and Suffolk, having been apportioned by Dr. Kay, are smaller than in other counties; the average size of each district in the former county being about four and a half miles square, and in the latter four miles; with an average population in Norfolk of 3057, and in Suffolk of 3284.

In Suffolk, nevertheless, one rural district contains upwards of 6800, and one town district, that of Ipswich, has 6900.

On being asked, however, what was the salary of the medical officer, and what was the cost per head in that district, Dr. Kay answered in each case that he did not know (p. 4.) What is stranger still, when so simple a question was put as "Can you state, in those unions where the population varies from 3000 to 4000, what is generally the amount of salary?" Dr. Kay, in a reply reaching to 41 long folio lines, gave nothing in answer to the question, excepting, forsooth, that "the salary in the unions, in relation to the population, would perhaps not afford the committee the best information from which to judge of what the amount of the remuneration is." It must be confessed that the faculty of answering questions categorically is a talent of itself, and worthy of all praise. This unanswered, but not unanswerable, question seems to us to contain the very cream of the whole inquiry; and why Dr. Kay evaded it in this place we are at a loss to know, for the first paper which he delivered to the committee (Appendix,

No. 1, p. 15) contains a near approximation to an answer. If we take the Mitford and Launditeh union, the first on the list, we find that it has a population of 27,694, and that the salaries of its medical officers amount to 500*l.* per annum; hence we may presume that the care of a district containing 3000 inhabitants is rewarded with about 55*l.* per annum. In the Wangford union, the fourth on the list, the population is 13,234, and the medieal officers receive 172*l.* per annum, so that he who superintends a district containing 3000 will receive something less than 40*l.* a year; while in the union constituted by the Great Yarmouth parish, a population of 21,115 is provided for by a medieal staff receiving 120*l.* a year, or at the rate of about 17*l.* per district of 3000. From 40*l.* to 50*l.*, however, may be considered as the average.

It does not require much argument to shew that the highest of these rates, being about 3s. a day, is absurdly small, and could never be considered a remuneration for attending six, eight, or a dozen patients daily, and providing them with medieines. Hence sueh districts at such salaries are only taken with the hope of keeping out or keeping down rival practitioners; and thus the destructive competition existing among medical men enables our economists to play them off against each other.

Some of Dr. Kay's suggestions for the improvement of the present system are very good as far as they go, but as he leaves this painful faet untouched, they are comparatively of minor importance. Thus he prefers a fixed rate of remuneration to the system of tender; and proposes that all who are paupers, and in actual reeeipt of out-door relief, should receive medical assistanee without the intervention of orders.

It is time to conelude these observations for the present; but we shall return to the subject on an early occasion.

GLASGOW EYE INFIRMARY.

Clinical Lectures by Dr. Mackenzie.

September 8th, 1838.

Ophthalmitis Phlegmonosa, or *Ocular Phlegmon.*

I now return, aeeording to the promise I gave in my last lecture, to the subject of Phlegmonous Ophthalmitis; or, as Professor Rosas, from the eircumstance of all the textures of the eye being involved in the disease, proposes to term it, *panophthalmitis.*

There are two kinds of phlegmonous ophthalmitis; the one is the consequence of direct injuries, such as the operations for eataract, and the other is the result of phlebitis. We may eall the one *traumatic*, and the other *phlebitic ophthalmitis.*

The ease of Fletcher, whieh I commented on in my last leeture, is an example of phlegmonous ophthalmitis, resulting from the incision of a staphylomatous eornea. I shall now direct your attention to another instance of the same sort of general and violent inflammation of the eye, also resulting from a surgical operation:—

Case I.—*Extraction of a Capsular Cataract—Phlegmonous Ophthalmitis—Gangrene and Sloughing of the External Laminæ of the Cornea.*

Arehibald Campbell (No. 2748,) aged 20 years, was admitted here on the 23rd of April, 1830: he squints inwards with the left eye; both pupils are above the medium size; the posterior chamber preternaturally large; the right iris tremulous. The right crystalline body seems redueed to a eoncave scale, of a ehalky appearanee; the left is in the same state, but the opacity is not so uniform as that in the right eye, the centre only being ehalky, while the circumferenee is more of an ash colour. Surrounding each of the eataracts there is a transparent ring (*cataracta cum zonula*,) espeeially round the left, through which transparent part he sees objeets with eonsiderable distinctness, and at a variety of distanees, but cannot see to read. Says he had good sight till he had cowpox, when about five years of age. Previously to his loss of sight, says he had a violent inflammation of the eyes. The left eye is the one with whieh he sees best, notwithstanding the squint. General health good.

24th.—A curved needle was introduced through the temporal side of the right sclerotiea, and brought in front of the cataraet, which was found to eonsist of the two hemispheres of the capsule, much thickened, and in contact with one another.

The anterior capsule was divided without much difficulty into two segments, and these were brought forward into contact with the cornea, along with the posterior capsule. About a fourth of the circumferenee of the cornea was then opened with the iris-sealpel, when the portions into which the anterior capsule had been divided were evaeuated with the aqueous humour. The needle was now withdrawn, and a delicate bent probe being employed to open the wound of the cornea, and lay hold of the posterior hemisphere of the capsule, this was readily extracted. The pupil immediately contraeted to a medium size, and remained clear.

25th.—Complained mueh of pain in the eye at eight o'clock last night, when eighteen ounces of blood were taken from his arm, and he had a ealomel and opium pill (2 gr. and 1 gr.) Pulse to day natural; considerable eonjunctival redness on temporal side of the eyeball; pupil of natural size; complains ehiefly of a feeling of sand in the eye. He had two ounees of sulphate of magnesia, and the calomel and opium pill was repeated at bed-time.

26th.—Again complained of considerable pain in the eye and head last night, when eight leeehes were applied to the temple. The conjunetiva to-day is still more vaseular, and is diseharging puriform mucus. Solution of nitrate of silver (2 gr. to ʒi.) A table spoonful every third hour of a solution of two grains of tartar emetic and twenty grains of tartaric acid, with one ounce of syrup in seven ounces of water; calomel and opium pill at bedtime; ʒj. sulphate of magnesia to-morrow morning; eye to be bathed with tepid milk and water.

27th.—Was again bled last night to the amount of eighteen ounees. The eye is intensely inflamed, and the cornea so hazy that the pupil cannot be distinetly seen. There is a eolleetion of matter at the lower edge of the eornea, probably in the anterior chamber; but the eye is so intolerant of light as not to stand suffieient examination to allow this to be aseertained. Twelve grains of ipeeacuan, twenty-four grains of sulphate of quina, and sixty grains of sugar, were direeted to be divided into twelve powders; and he was ordered to take one every third hour during the day. Two calomel and opium pills at bedtime; belladonna around the eye; solution of antimony, and solution of nitrate of silver, omitted.

28th.—Eye and head less acutely painful; intoleranee of light and epiphora somewhat diminished; tongue clean and moist; pulse slow; eornea still very muddy; colleetion of purulent fluid appears to be less; bowels open. Powders, pills, and belladonna, continued.

29th.—A blister ordered between the shoulders.

30th.—Upon the whole, feels easier. The cornea presents throughout a greenish-white appearance, entirely obscuring the iris. Two calomel and opium pills at bedtime, and one in the morning.

May 1st.—Solution of nitrate of silver repeated.

3d.—Twenty leeches round the right eye.

4th.—Bilious vomiting, and frequent bloody stools; edge of cornea appears a little clearer; pupil seems filled with pus. Powder and pills omitted. Ten grains of Dover's powder at bedtime. To have some beef tea.

5th.—Eyeball appears generally enlarged, and the eonjunctiva is chemosed. The point of a lancet was introduced between the lips of the incision, but nothing appeared to be discharged. A blister to right temple and behind the ear. Dover's powder thriee a-day.

6th.—Eyeball excessively hard, and mueh projected from the orbit; edge of cornea elearer; complains of frequent flushes and chills. Two stools. Cataraet needle passed through sclerotica, and a small quantity of bloody water discharged. Dover's powder eontinued.

7th.—Exophthalmia increases.

8th.—Lancet introduced through the selerotica at temporal side of eye, but only a little aqueous fluid with blood discharged.

10th.—Edge of cornea presents an appearance as if it was about to slough. Enlargement of eye continues.

12th.—Exophthalmia subsides; edge of cornea loosens, but slough does not seem to include its whole thickness. Tongue clean; pulse slow; two stools. Dover's powder continued.

13th.—Slough has separated from the cornea, leaving a floculent purulent surface. Swelling and protrusion of eyeball less.

19th.—Dover's powder at bedtime only.

June 4th.—Dismissed; the eye having subsided to its natural size; cornea opaque.

Campbell was not a plethorie subjeet, but rather thin, and had probably been in the way of being poorly fed, before eoming here.

There was nothing in the operation sufficient to account for the violent inflammation which followed, nor could we trace this consequence to any cause, external or internal. There was no exposure to eold, nor any intemperanee on his part. Are we to aseribe it to the admission of atmospherie air into his eye at the time of the operation, or must we content ourselves by attributing it to some unknown peculiarity of constitution?

The eyeball seems greatly enlarged in all cases of phlegmonous ophthalmitis; it looks as big as the fist in some of them. But, in fact, the eyeball is scarcely, if at all, enlarged. It is the protrusion of the eye from the orbit, along with the chemosis, which gives it the frightful appearance you saw in Fletcher, and which existed in a still greater degree in Campbell.

The eye-ball feels excessively hard in this disease; but on puncturing the sclerotica, little or nothing is discharged. Both the traumatic and the phlebitic cases agree in this, as well as in most of the other local symptoms. Puncturing the sclerotica, notwithstanding the smallness of any discharge which takes place, generally gives great relief, and the symptoms begin immediately to subside when this practice is adopted.

It is extremely probable that phlebitic ophthalmitis depends on inflammation of the capillary veins of the eye. Now, since traumatic ophthalmitis resembles the phlebitic so closely, may it not also consist in inflammation of the veins, excited by the injury?

The treatment in Campbell's case was sufficiently active, but it had no effect. Nothing seemed to check the disease; and this constitutes another point of resemblance between the two sets of cases, the traumatic and the phlebitic; for the latter are scarcely at all moderated even by the most active antiphlogistic treatment.

Phlebitic ophthalmitis has been observed under a variety of circumstances. It is a consequence of suppurative inflammation in some part of the venous system more or less remote from the eye, and this may be excited in different ways. For example,

1. Inflammation of a distant vein, induced by a wound, or by tying the vein, has been followed by the usual constitutional disturbance attendant on phlebitis, and amongst other secondary effects, by disorganizing inflammation of the eye. A case of this sort, which occurred in the practice of Mr. Earle, is recorded in the Medical Gazette, vol. ii. p. 284.

2. Suppurative inflammation of the uterine branches of the hypogastric veins, in puerperal women, which, spreading to the iliac and femoral veins, is apt to cause phlegmasia dolens, sometimes produces phlebitic ophthalmitis. Cases of this sort were recorded by Dr. Hall and Mr. Higginbottom, in the Medico-Chirurgical Transactions, vol. xiii.; but their phlebitic origin was first pointed out by Mr. Arnott.

3. Phlebitis occurring in erysipelas, or diffuse cellular inflammation, has been known to terminate in phlegmonous ophthalmitis.

4. Phlebitis arising in the course of typhus fever has ended in the same result.

I have little doubt that this termination follows in other cases of phlebitis; but the subject is comparatively a new one, and much remains to be done for its complete elucidation. In some of the cases I mean to lay before you, no marks of phlebitis may have been detected on dissection, and yet there can scarcely be a doubt that the cases were of phlebitic origin. In all cases of phlegmonous ophthalmitis, arising without direct injury, you should suspect this cause; examine carefully the different limbs, so that if there be any swelling, hardness, or pain, in any of the external veins, you may detect it, and ascertain also the state of the general health, and the history of the patient's previous ailments; so that if phlebitis had preceded the affection of the eye, you may not remain ignorant of so important a circumstance. In fatal cases of phlegmonous ophthalmitis the venous system should undergo the most careful scrutiny when the dead body comes to be examined.

Case II.—*Diffuse Cellular Inflammation of the Forearm.—Phlegmonous Ophthalmitis of both Eyes.—Recovery, with total Amaurosis.*

Mrs. L., aged 60 years, previously very healthy, became affected with whitloe at the point of the index finger of the right hand, about the end of November 1837. She said she had pricked the finger, and that it had afterwards been poisoned. She described the pain as shooting occasionally from the finger towards the shoulder. The finger was laid open, and a few drops of pus were discharged. In the course of a few days it was apparent that pus was lodged in the sheath of the flexor tendon, which was opened. This was soon followed by erysipelas over the whole forearm, ending in extensive suppuration. The forearm continued in a bad state for more than five weeks. It was repeatedly punctured, and disorganized cellular membrane was discharged by numerous orifices.

About the middle of January 1838 she began to complain of rheumatic pains in several of her joints, but without any swelling. She had never been subject to rheumatism, nor to synovitis. A few days after the occurrence, the erysipelas suddenly subsided, and she began to complain of dimness of sight, but without any pain in the eyes. The iris of each eye appeared inflamed, and the pupils were somewhat contracted and very hazy. In three or four days from the commencement of this affection of the eyes, there was a deposition of lymph observable at the lower part of each anterior chamber.

I visited Mrs. L., along with Mr. Max-

well and Mr. Moffatt, on the 26th of January. Both eyes were almost completely amaurotic. The capsule of each lens was opaque, and somewhat of a greenish hue. The pupils were irregular and contracted. These symptoms denoted the existence of inflammation; but their supervention had been unattended with pain, and in this respect the case differed from ordinary instances of iritis. Calomel and opium were given, so as to make the gums sore in a few days; leeches and blisters were applied; and extract of belladonna was used externally.

On the 6th of February, nine days from the time when her sight became impaired, the left eye was greatly protruded, and the conjunctiva so much chemosed as to overlap the greater part of the cornea. The swelling was of a pale red colour, and covered with a gelatinous, or lymphatic, pretty firm, exudation. The eye-ball was very tense. I punctured it with a lancet, through the sclerotica.

In about a week the swelling had fallen, so that the eye had retreated into its socket, and presented its natural size, but the rheumatic pains became much aggravated for several weeks. Vinum colchici, external rubefacients, &c., were liberally used, with little apparent benefit.

About the beginning of April, the right eye-ball became protruded in a similar way as the left had been two months before, but not to the same extent. It subsided in about the same time, under soothing applications. The left eye has since become atrophic. Both remain totally amaurotic.

In the months of June and July the rheumatic affection abated considerably, and the general health improved, but she felt so much weakness in her back, that she could not walk without assistance. Several of the lumbar vertebræ appear a little protruded.

In this case there were strong grounds for believing that the disease of the eyes was the consequence of some morbid poison, generated during the attack of diffuse cellular inflammation, and conveyed through the body by means of the circulation. A remarkable circumstance was the length of time which elapsed between the protrusion of the one eye and that of the other.

DR. BULARD.

This gentleman has been presented with a gold medal by the English colony at Smyrna, as a testimonial of their gratitude for the services he had rendered them during the prevalence of the plague, the city and vicinity being entirely free from that dreadful malady on the 16th July.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

Thursday, September 6.

Henry Mapleton, Exeter.—Frederick Augustus Laking, London.

WEEKLY ACCOUNT OF BURIALS.

From BILLS OF MORTALITY, *Sept.* 11, 1838.

Abscess	1	Fever	11
Age and Debility	16	Fever, Scarlet	3
Apoplexy	1	Hooping Cough	3
Asthma	2	Inflammation	5
Cancer	2	Bowels & Stomach	5
Childbirth	3	Brain	1
Consumption	26	Lungs and Pleura	3
Constipation of the Bowels	1	Influenza	2
Convulsions	14	Insanity	1
Croup	1	Miscarriage	1
Dentition or Teething	12	Mortification	1
Diarrhœa	2	Small-pox	12
Dropsy	4	Spasms	2
Dropsy in the Brain	6	Tumor	1
Dropsy in the Chest	1	Unknown Causes	51
Epilepsy	1	Casualties	6
Erysipelas	2		

Decrease of Burials, as compared with the preceding week 160

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N. Longitude* 0° 3′ 51″ W. *of Greenwich.*

September.	THERMOMETER.	BAROMETER.
Thursday . 6	from 55 to 64	29·28 to 29·27
Friday . . 7	56 65	29·31 29·35
Saturday . 8	50 60	29·52 29 85
Sunday . . 9	38 60	30·07 30·10
Monday . . 10	35,5 61	30·26 30·37
Tuesday . . 11	34 64	30·40 30·44
Wednesday 12	34 63	30 32 30 30

Wind, N. and S.W.

Generally clear, except the 6th and two following days, when rain fell: distant thunder, accompanied with rain, on the afternoon of the 27th.

Rain fallen, 375 of an inch.

CHARLES HENRY ADAMS.

NOTICE.

Mr. R. Gibson's communication shall appear, but our correspondents must grant us their indulgence at the present moment, as we have to wind up the volume.

ERRATA. — Mr. Clutterbuck on the Secale, &c. p. 947, col. 2, l. 8, *for* "affected," *read* "effected;" l. 9, *for* "influence," *read* "inference."

In Mr. Aspland's paper, in last week's Gazette, *for* "careful and clinical post-mortem," *read* "for careful clinical and post-mortem." Towards the end, *for* "to prove that this is a mere coincidence," *read* "to prove that this is *not* a mere coincidence."

WILSON & SON, Printers, 57, Skinner-st., London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, SEPTEMBER 22, 1838.

LECTURES

ON THE

PHYSIOLOGY AND DISEASES OF THE CHEST,

INCLUDING THE PRINCIPLES OF PHYSICAL AND GENERAL DIAGNOSIS,

Delivered during the Spring Seasons of 1836 and 1837,

At the Anatomical School, Kinnerton Street, near St. George's Hospital,

By Charles J. B. Williams, M.D. F.R.S.

Lecture XXX.

On the Prognosis and Treatment of Diseases of the Heart.—On the different Course of Cardiac Lesions.—On the Value of their Signs and Symptoms in directing the Treatment.—Practical Indications common to Diseases of the Heart.—Treatment of functional Disorders; increased Action; defective and irregular Action.—Treatment of Inflammations of the Heart.—Treatment of structural Diseases; Hypertrophy; Dilatation; Diseases of the Valves and Orifices.—Treatment of complex organic Disease of the Heart, and of its Symptoms and Effects.—Remedial Treatment.—Diet and Regimen.

This, our concluding lecture, is to be devoted to the consideration of the principles and application of the *treatment of diseases of the heart.* I am induced to embody my observations on the remedial measures applicable to the different affections which have occupied our attention in the preceding lectures, because I thus avoid repetitions and save time, and because also I shall be better enabled to bring before you the general principles of treatment which are applicable to all diseases of the heart, inasmuch as they all agree in producing more or less disorder in that spring of all the functions, the circulation of the blood.

If we look at the different varieties and degrees of diseases of the heart, even those which so much affect its structure that their nature cannot be a matter of any doubt, we find a wonderful variety in the extent of their effects in the system at large, and consequently in their tendencies and duration. In some cases serious and unequivocal symptoms of heart disease have soon ended in death; yet the lesions found have been comparatively small, and by no means *apparently* proportioned to the severe and fatal character of the affection. In other instances the symptoms have been more equivocal and complicated, less clearly referrible to the heart, being grouped with prominent affections of other organs; yet still, on death ensuing, the chief lesion is found to be in the heart, although even that may not be very extensive. In a third class of cases, and they are very numerous, there may have been the same symptoms as those which occurred in either of the former classes; their severity may have been such that the patients have been thought to be dying under them; yet they have recovered to a valetudinarian state, which has lasted for many months, and even years, sometimes much, sometimes little harassed by similar symptoms; occasionally again brought to death's door by exacerbations of these attacks, yet regaining afterwards health enough to enable them for years to discharge the duties of life; and when at length they die from this disease, or from some other induced or aggravated by it, there is found such an enormous extent of old structural disease, that it is a matter of astonishment to us that life could have been maintained so long and so well.

From witnessing many such cases and such results, I have learned two things in particular: first, that structural lesions of the heart are attended with immediate

danger in proportion more to the quickness of their production or of their increase than to their extent; and secondly, that if acute attacks or intercurrent disorders, which aggravate the permanent lesion, can be prevented or speedily removed, the fatal issue of even extensive organic diseases of the heart may be very commonly delayed for a considerable length of time. Inflammatory affections of the heart may in rare examples destroy life in their acute stage. More frequently they lay the foundation of lesions that proceed slowly and often in a latent manner, until suddenly developed by some new attack or cause of disorder: this may sometimes be accidental, but it is often consequent on the insidious operation of the latent lesion on some of the bodily functions; and were this discovered in time, it might often be prevented or counteracted. In occasional examples these diseases cause sudden death; and it is this circumstance chiefly that attaches an awful character to the whole class. But such instances are comparatively rare; and bear a very small proportion to the cases that we detect either during life or after death. Diseases of the heart are popularly looked on as hopeless cases; and to discover or distinguish them has been pronounced by high authorities in the profession to be the most difficult part of physical diagnosis. Yet, imperfect as our knowledge of this diagnosis is, it is sufficient to prove that diseases of the heart, so far from being always hopeless, not unfrequently exist very long under a mask of delicate health or some vague general disorder; and by detecting them when they are latent, and by defining them when their general symptoms are equivocal, physical diagnosis has removed from this class of diseases the character of such terrible fatality, and enables us to treat them at a period in which remedial and prophylactic measures may be of much avail.

I have said that diseases of the heart always produce more or less disorder of the circulation; but the most prominent symptoms of this disorder may be in parts remote from the heart, and may blind us as to their original source. Thus the excited circulation of pericarditis or hypertrophy sometimes manifests itself more in the brain than in the heart; and the retarded circulation of mitral disease often chiefly declares itself by derangement of the liver. The physical signs lead us back from these symptoms to their first cause, and guide us to shape our practice with a due reference to its character. But are we to disregard these general symptoms or secondary effects? Certainly not. They often become important parts of the whole disease; and with regard to the more chronic affections it may be said, that it is in great measure through these secondary effects that the balance of the functions is destroyed, complicated disease produced, the original lesion aggravated in return, and the malady becomes no longer local, but general, affecting first the functions, then the structures of several of the most important organs of the body. We must study disease in all its bearings: in its earliest history and supposed causes; for in these we may learn its origin, and be guided aright to investigate its nature. In its physical characters; for in these we find that of it which is essential and fixed. In its general symptoms and constitutional effects; for in these we find how and where the local disease is producing in the function or structure of other parts mischief that mainly contributes to injure the vital powers and to aggravate the original evil. And this study is as essential for the proper treatment as for the thorough knowledge of the disease. Thus in a case of pericarditis, if we neglect to inquire into the former history of the subject, we may fail to discover that there was long ago a severe rheumatic affection, followed perhaps by some symptoms of organic lesion which have only been more strongly developed by the present attack: and not knowing this, we may be led to treat the disease with dangerous vigour. Again, if we neglect in the same case carefully to examine the physical signs, we may not discover pericarditis to be present; but, guided by the prominent general symptoms, mistake it (as it has happened) for phrenitis, or bilious fever, bronchitis, influenza, or pleurisy, or gastritis, or some other quite different malady. Under this error the proper treatment may be withheld, and the patient's life lost, or limited by the formation of permanent organic disease. And if we neglect general symptoms, we lose the means of judging of the strength of the bodily powers, of the effects of the disease and of the remedies on them; and for want of this judgment, we can never rightly proportion or regulate the treatment.

Nor is attention to all the bearings of the disease less necessary in cases of more chronic or structural affections. These, whether their symptoms be distinct or not, if neglected, tend continually to derange the functions of other organs, and gradually to alter their structure, which increases and perpetuates constitutional disease, and reacts on and aggravates the original lesion. The lungs, the liver, the kidneys, the brain, and the mucous and serous membranes, all are liable to functional, and ultimately structural disease, from the disordered circulation caused by an imperfect heart; and if they are not watched and relieved before they mate-

rially suffer, their affections must hasten the failure and decay of the whole frame. The imperfection of the heart itself has also a tendency to increase, from the additional irritation and work thrown on it by its injured mechanism; and if not placed in circumstances calculated to soothe this irritation, and lighten this labour, its very exertions become more and more hurtful, and soon end in destroying life.

If, on the other hand, we are fully aware of the origin, character, and constitutional tendencies of a disease of the heart, we can often direct its treatment with remarkable success. In the case of inflammation, the application of antiphlogistic remedies with due relation to its seat and character,—and in that of structural disease, the adoption of means proper to lighten the labour of the organ, to allay its excitement, and to prevent the bad consequences resulting from long continued disorder in parts of the circulation, —are practical applications of this knowledge, the utility of which cannot be doubted. In the more moderate forms of structural disease in particular, in which the cardiac affection is eclipsed and unobserved amongst a variety of secondary symptoms, the knowledge of its existence may be of the greatest utility. The subjects of such disease, according to the effects which it manifests, are called "delicate," "of a weakly constitution," "nervous," "dyspeptic," "bilious," or "apoplectic," and if treated incautiously as such, without regard to the disease of the heart, may be seriously injured in consequence. Thus, for a supposed "general debility," are freely prescribed tonics and generous diet: for an apparent "nervousness," or "hypochondriasis," exciting amusements and active exercise: for a "liver complaint," reducing courses of mercury and of purgatives: for "blood to the head," or "tendency to apoplexy," venesection, cupping, and starvation: and all these measures, by further injuring the balance of the functions, more commonly aggravate than mitigate the original disease. Judicious practitioners, warned by experience, are led to treat these cases with more prudence and success, although still in ignorance of their real nature. Of such cases they will tell you, that although weak, they "do not bear tonics well," or although full and apoplectic, they "will not stand much reduction." The practical measures to which they have empirically groped, may be indicated to you by a clear and comprehensive study of the nature of the case; and in applying and modifying these measures you will always have the advantages and resources which rational and intelligible knowledge will supply above the rules of blind experience.

The treatment of diseases of the heart must, of course, greatly vary, inasmuch as they may be of very opposite characters; but there is one point of general agreement: the organ is in all cases taxed beyond its force, and whether from excess of irritability, or from defect of power, its task is too much for its well-being. A common indication, therefore, in all cases, is as much as possible to diminish this task, to lighten its labour. The greatest tranquillity of mind and body, without, at the same time, in the more chronic cases incurring the risk of torpor or stagnation; promoting by means suited to the nature of the case, the capillary circulation; easing the weight of the general circulating mass by the most favourable postures, varied from time to time to render gravitation an assisting agent; reducing the quantity of the mass which the heart has to propel, by a sparing use of liquid food; and at the same time correcting its quality, by cautiously promoting as much as possible the due balance of all the excreting functions; these are indications applicable to all kinds of cardiac disease, and in many constitute the chief heads of the treatment.

The treatment of *functional disorders* of the heart is a subject of great importance, because it is applicable also to cases of organic disease, which are liable to exacerbations by additional functional disorder. But this treatment must be varied not only according to the kind of disorder, but often also according to the nature of its cause. Thus *increased action*, arising from general plethora, is best relieved by diminishing the mass of blood by bloodletting, abstinence, and increasing the secretions. That from local plethora, or irregular distribution of blood, if possible, by directing it into its proper channels, and thus restoring the due balance of the circulation; but if the cause of the local congestion be an obstruction to the circulation which cannot be soon removed, or a suppressed secretion that cannot be readily restored, the congestion must be diminished by local or general bloodletting, rather than it should continue to oppress the important functions of the heart and lungs. So, also, when the increased action appears to be from an irritating quality of the blood, as in gouty and rheumatic habits, and in certain fevers, the use of colchicum, alkalies, nitre, and other sedative remedies which act on the secretions, is often found the best mode of relief. Again, inordinate action from extraneous irritation is best reduced by removing, if possible, the irritating cause, whether this be acidity, flatus, or indigestible matter in the stomach, fæces

or worms in the intestines, congestion or bilious concretions in the liver, an irritation of the uterus, or the like. Or if the cause be only slowly or not at all removable, we may employ means to lessen the sensibility of the nerves and of the heart to its effects, and these are the remedies indicated also where the increased action proceeds chiefly from an excessive irritability in this organ itself; whether this be idiopathic, or dependent on a structural lesion. Of these remedies there are two classes; narcotics or sedatives, the action of which is temporary; and tonics, which far less surely but more permanently lessen the irritability of organs, and at the same time augment their power. The sedatives that I have found most generally useful to allay palpitation and inordinate action of the heart, are camphor, hydrocyanic acid, hyoscyamus, belladonna, assafœtida, æther, and opium. The last is generally the most powerful, but its effect on the circulation being irregular, it is apt to disorder the secretions, and occasion subsequent congestions, which may continue to disturb the heart's action. By combining it with mercurial medicines, and ipecacuanha or colchicum, and following its operation by an aperient, these bad consequences may often be prevented. I have found digitalis to exercise far less control over the heart when primarily excited than when its action is quickened by disease of other organs; and as it appears to me, when it does act, to diminish the power of the heart as well as its irritability, I think that its use should be confined to those cases of perpetually increased action, in which a hurtful excess of strength is manifest in the pulse, as well as a frequency of the contractions. Of the other remedies named, I have found all occasionally beneficial, either singly or in combination; but I think that they are more so when given in a few large doses, to break as soon and as completely as possible the habit of morbid action, which, when established, is much more difficult to remove. Hydrocyanic acid must be excepted from this rule, for in large doses it acts like digitalis, inducing syncope; but in quantities of two or three minims it is often very effectual in quieting the palpitations caused by a disordered stomach. The tonic medicines generally best calculated to permanently lower the irritability of the heart, or of the nervous system through which it is excited, are those of a metallic nature, such as the subnitrate of bismuth, the sulphate and oxyde of zinc, the nitrate of silver, and the sulphate of copper. Whether these act directly on the nerves and heart, or only indirectly by modifying the condition of the alimentary canal, I cannot determine; but I have known a continued exhibition of each of them, in different instances, decidedly useful in relieving a tendency to nervous palpitation, even where this was combined with some amount of structural disease. Pure country air and cold bathing, or free sponging, with regular gentle exercise, are sometimes more effectual than medicines, in removing palpitation chiefly dependent on a mobility or nervous irritability of the heart. Such a condition may be accompanied by extremes in the action of the heart which is at one time excessive and at another defective, and is then a fit subject for the treatment recommended for weakness or defective action of the heart. On the other hand, if the excessive action be caused, attended, or followed by an inflammatory condition of the organ, evinced by heat of skin, continued pain or soreness, any of the physical signs of endocarditis or pericarditis, or of some new valvular lesion, the treatment should more or less consist of the remedies recommended for these inflammations. Further, it is to be noticed that inordinate action of the heart may produce and leave effects which may require separate treatment, such as pulmonary congestion, and even hæmorrhage and inflammation, bronchial spasm and excessive secretion; nausea and irritability of the stomach; congestion of the liver and bilious disorder; severe headache; swelling of the thyroid gland and tonsils, and other results of disturbed circulation.

The treatment of *defective action* of the heart, and of *irregular action* arising out of *weakness*, must also be guided according to its causes and nature. If it be of a transient kind, amounting or approaching to syncope, it may require only temporary measures, which are chiefly the exhibition of diffusible stimuli, such as ammoniacal, alcoholic, and ethereal medicines, or the production of strong impressions on sensitive parts, as by pungent vapours to the nose, dashing cold water on the face and chest, or by drinking a draught of it. Of these measures, none is so effectual as the dash of cold water; and if combined with warm applications to the extremities, it is the best resource where the fainting fits are so long as to be dangerous. It is not less efficacious in slighter cases; but it is not then generally eligible, for it is not a favourite remedy. I have known the bare mention of it arouse a lady from an apparent fainting fit, in which the action of the heart had really become very feeble: this shows what power the mind may exert over the heart; as in instances in which persons have been kept from fainting from loss of blood by strong mental excitement. The defective or irregular action of the heart induced by flatulent distension of

the stomach, or other results of indigestion, is best removed temporarily by ether, or strong carminatives, such as capsicum and the essential oils; and permanently by improving the state of the digestive organs. To guard against the returns of an habitual temporary defection of the heart's action, it is often proper, besides removing as much as possible their causes, to improve the tone and strength of the heart by tonic remedies, and these are also indicated in cases of more permanent or habitual weakness of the organ. Among the medicines generally classed as tonics, none seem to improve the tone of the muscular fibres of the heart so effectually as the preparations of iron, when they are well borne. But it is often very difficult to get a sufficient quantity into the system without disordering the stomach, and causing fever, headache, and other disturbance, which render hurtful the further operation of the medicine. Steel medicines should be begun in very small doses; both preceded and accompanied by occasional mild mercurial or saline aperients, to keep free the secretions, and to prevent any congestion or irritation in the intestinal canal. It is by fulfilling these conditions that weak chalybeate and saline waters sometimes succeed better than steel medicines. The carbonate of iron, too, is sometimes borne from its inertness, for whatever doses be taken, there can be conveyed into the system only the minute quantity that becomes dissolved by acid accidentally present in the alimentary canal; hence its action is very uncertain. The preparations of iron that I have generally found to agree best, are the ammoniated tincture, the tartarized iron, and the iodide, which in solution forms the hydriodate. The last has succeeded in my hands better than any other preparation; especially in chlorotic and scrofulous subjects, on whom it sometimes acts like a charm, restoring quietness and strength to their pulse, colour to their cheeks, vigour to their muscles, and regularity to their secretions and other functions. When iron is not borne, some benefit may be derived from bark, quinine, and other vegetable tonics; and in cases where paroxysms of irregular action occasionally occur in a heart habitually weak, the temporary use of æther, camphor, valerian, and the fœtid gums, and courses of the metallic preparations before noticed, of bismuth, zinc, and silver, sometimes prove beneficial. The heart and organs generally may often be much strengthened by the daily use of the shower-bath, or of very free sponging of the chest with cold salt water, or vinegar and water; by removing to a pure bracing air, and using frequent gentle exercise in it, and by great regularity and simplicity of living, with as generous a diet as the circumstances of the case will allow.

The treatment of *inflammations of the heart* is to be conducted on the same principles as that of inflammations in general, with this qualification, that unless in a stage almost too early to be called inflammation, we must not expect to produce by copious blood-letting that complete relief to the symptoms that often follows it in other inflammations. The irritation is here so much localized in the heart, that were inordinate action entirely reduced, it would leave the organ suffering from the equally hurtful extreme of defective action, from which the circulation would continue to be embarrassed, and the feelings of dyspnœa, faintness, and agitation kept up. Then succeeds the period of reaction, which more readily in the heart than in any other organ might bring back the inflammation, even when the body is suffering from extreme weakness and loss of blood. Whether this be the true view of the case or not, I have been convinced by repeated observations of different kinds of practice, that both pericarditis and endocarditis, and especially if connected with rheumatism, are most safely and effectually treated by *moderate general* blood-lettings, avoiding as much as possible sudden or full impressions on the circulation; and that local bleedings, free and repeated, should in all cases be employed as a measure of at least equal importance. The bowels having been freely opened, calomel and opium, as originally recommended by Dr. Robert Hamilton, should be given until the gums shew their effects. This valuable combination seems to be most effectual in large doses, twice or thrice in the day, as some of you have seen it prescribed by Dr. Chambers; and with it, or in the intervals, alkaline salines with ipecacuanha or antimony, and in rheumatic cases colchicum, may be given to allay the fever, to promote the secretions, and general irritation which prevails, before the blood-letting and the mercury have taken their full effect. As the disease becomes more chronic, the fever having abated, but signs of effusion of serum or of lymph remaining, with continued disorder of the heart's action, the local bleeding must be repeated, and the influence of the mercury renewed, from time to time, according to the state of the symptoms. For the febrifuge saline, I generally substitute at this stage an alkaline draught, with two or three grains of the hydriodate of potash, with a view to promote the absorption of the effused lymph; and under these means, combined with blistering or pustular irritation, the symptoms and signs have in some cases gradually disappeared, leaving the action and condition

of the heart perfectly normal. In other cases apparent health has been restored, but a murmur remains, a proof of permanent structural lesion, which, however, may be of trifling amount, and, if due care be taken, of no ulterior consequence.

The treatment of *structural diseases* of the heart must be adapted to the nature of the lesion as discovered by the physical signs, and of its constitutional effects, as manifested by the general symptoms. When these lesions are simple, the effects of the several kinds may be quite different, and the plan of treatment must be so likewise. But the lesions are much more commonly combined, and require in the same case mixed measures, which must be varied from time to time, according to the prevalence of local and general conditions of a different character. I will speak first shortly of the measures suited to the more simple lesions, and conclude with remarks on the general treatment of the more common and complicated forms of organic disease of the heart.

The treatment proper for *hypertrophy* of the heart will partake of the character of that suited to increased action, with a sthenic state of the vascular function from which it originates. Medicines, and a regimen calculated to reduce the action of the heart to a low but even standard, are those which are found to give most relief, and to retard and even arrest the progress of the disease. In severe cases, and to relieve a temporary congestion or increased irritation, occasional blood-letting is expedient, but not carried to a great extent, for it then may be succeeded by a hurtful reaction. Evacuations of other kinds are also useful; but the same rule of moderation should be applied to them. The diet should be spare, especially in liquids and stimulating food; but not starving, for this may produce cachectic and nervous disorders, and will injure other organs and functions more than it will reduce the action of the heart. Again, sedative medicines, such as digitalis, hydrocyanic acid, and colchicum, may sometimes be given with good effect; but not by producing sudden or strong impressions on the circulation, for these injure other functions at the time, and their effect is more than neutralized by the subsequent disorder and the irritations of reaction. Tranquillity, too, is highly desirable; but if this amount to a state of sluggish inaction, it will injure the functions of the capillary and secernent vessels more than it will quiet the action of the heart. It is always to be remembered, that hypertrophy is a disease affecting the slow and constant process of nutrition; and if it is to be arrested or removed at all (and I believe that moderate degrees both of hypertrophy and of dilatation are curable), it is by means which act constantly and slowly, and not by sudden or strong influences, or by any which continue to disturb the performance of any functions. The cautions against attempting fully or suddenly to reduce the action of an hypertrophied heart are especially applicable to the cases of fat persons, and those of a flabby fibre; for these can ill bear any abridgment of the strength of their circulation; and the embarrassed action which their heart sometimes exhibits, is often from want rather than excess of power.

In the treatment of *dilatation* of the heart, we have generally to use those measures which are suited to the temporary condition of defective or asthenic action from which it originates, and with which it is habitually combined; but its permanency makes it necessary that the more powerful of these resources should be used with discretion, moderation, and reserved rather for occasions of unusual depression or irregularity, than used habitually. But the treatment of dilatation generally should be of as strengthening and nourishing a character as the symptoms will bear; and if depletions or sedatives are necessary, they should be used only so far as to remove the congestions or allay the irritations arising from or accompanying the irregular or defective condition of the circulation. The metallic tonics, especially iron, if it can be made to agree, are often useful in improving the strength of the heart and diminishing its irritability; and its labour may be generally made easier, and its distension less, by using constant gentle means to keep the mass of the blood as small, and at the same time as rich, as possible. A nourishing diet carefully suited to the capacity of the digestive organs, avoiding that which unduly stimulates, and especially keeping the bulk of liquid at the smallest possible amount; preserving in sufficient activity the superficial circulation and its secretions by warm clothing, friction, gentle exercise, and occasionally, if necessary, by artificial heat, dry or moist; and the cautious preservation of the balance of the excretions by the preceding measures, aided, if need be, by appropriate mild medicines—these are the chief points indicated; and the more completely such a plan can be pursued, the better suited will the blood be in bulk to the strength of the heart, and in quality to improve the tone of its fibres. Occasional attacks of palpitation and asthma, and dropsical effusions, which occur when the preceding measures have been neglected, or have failed to bring relief, will require special treatment; but this must always be applied with the caution necessary with the feeble power of the

heart, which can bear no violent remedies, whether of a stimulating or of a reducing kind.

The treatment of *valvular lesions* must be also directed according to the manner in which it disorders the circulation, which varies from constitutional tendencies, as well as from the direct physical effect of the particular kind of lesion. Therefore it is not always but only frequently the case, that diseases of the mitral valve are especially accompanied with venous congestions which require treatment, in the lungs, the liver and bowels, and the head; and that diseases of the aortic valves are attended by more symptoms of arterial excitement throughout the system, requiring occasional moderate depletions and sedative remedies. In fact, scarcely this much can be said of the simple valvular lesions, but only as they commonly occur, combined with enlargement of the respective compartments. We shall therefore pass to the consideration of the general measures useful in the treatment of organic diseases of the heart, in the combined forms in which they generally present themselves.

I have before remarked, that considerable structural lesions of the heart are not incompatible with sufficient health and strength to support for years many of the duties, and to partake in many of the enjoyments of life; and that it is by their sudden development, or by their subsequent aggravation or speedy increase, in consequence of unfavourable circumstances, fresh inflammatory attacks, or of accidental injuries, that they frequently assume their distressing and destructive character. Now, although we can scarcely hope to cure the structural lesion, we may often mitigate its painful effects, and diminish its destructive tendency, by preventing or removing these aggravating attacks, and placing the patient in circumstances the most favourable for the maintenance of the balance of the circulation in its crippled state. The measures for accomplishing these objects may be divided into the *remedial* and the *regiminal*.

The chief *remedial* agents should be reserved as much as possible for times when an aggravation or increase of disease is perceived or may be expected; for if used continually, they may lose their effect, and they may interfere with the resources of nature, that may themselves gradually succeed in adapting the body to the imperfect state of the organ.

The exacerbation of the symptoms of a structural disease of the heart may be the result of mere *nervous* irritation, determined by mental excitement, gastric or intestinal disorder, or some such transitory cause. In this case a few doses of the antispasmodic remedies prescribed for nervous palpitation, may be sufficient, such as æther, camphor, valerian, or the fœtid gums. But even these should be combined or followed by an aperient to carry off the effects of the fit of disordered circulation; and in severe attacks of asthma, where there is extensive lesion in the mitral valve, as well as enlargement of the right ventricle, there may be left a congestion in the lungs, which is only to be gradually removed by expectoration, which may be promoted by the decoction of senega, with alkalies, ipecacuanha, and squill, or some other mild expectorant.

It will seldom happen, too, that a severe attack of palpitation, or troubled circulation, affects a diseased heart, without straining and irritating some part of the thickened or altered structure: hence commonly results a local inflammation, which, although slight, may be sufficient to cause a continued aggravation of the symptoms, or repeated returns of attacks of palpitation and asthma. So also subacute inflammations are often excited in a diseased heart by exposure to cold, bodily exertion, or mental excitement, the recurrence of rheumatism, or by other inflammatory attacks. It is by removing such inflammation, as well as the local congestions resulting from the unusually disturbed circulation, that local or a small general blood-letting, blistering, and the administration of a few doses of mercury and opium, prove so useful. The judicious application of these remedies, combined as far as it may be indicated with antispasmodics and anodynes, which more promptly allays the excitement and pain of the paroxysms of palpitation and asthma,—and constitutes the chief treatment of the exacerbations of structural diseases of the heart; and the earlier it can be applied, the more mischief it may prevent, and the longer may the imperfect mechanism be made to last. The mode and extent of depletion will be suggested by the circumstances of the case. It should scarcely ever be large. A moderate bleeding from the arm, or cupping from the chest, may be preferred where the signs and symptoms indicate much congestion of the lungs; but where the general weakness or irregular action of the heart renders the loss of blood a questionable measure, considerable benefit with less risk may sometimes be obtained by dry cupping freely employed on the chest and back, or in a less degree by deriving to the extremities by partial hot baths. In most cases, the loss of a small quantity of blood by cupping or leeches to the region of the heart will be sufficiently effectual. When

there is pain or fulness of the right hypochondrium, with an imperfect or disordered secretion of bile, and signs of hæmorrhoidal congestion, the blood-letting may be advantageously practised by leeches to the right side, or in smaller number near the anus. In females the disordered circulation not unfrequently interferes with the regularity of the uterine function; and if in these cases the natural relief cannot be obtained by the means usually employed, it may be imitated and sometimes induced, and the cardiac disease mitigated, by leeches applied to the groins or thighs. With lesions of the mitral valve and right side of the heart, these visceral congestions not unfrequently occur as the accumulated result of slight constant regurgitation, when there may have been no unusual disturbance of the circulation; and in this case, if treated early, they may yield to slighter measures, such as mild mercurial aperients; the occasional exhibition of which, to prevent the recurrence of "bilious attacks," or of hæmorrhoidal annoyances constitutes the best part of the medicinal treatment of many slight cases of disease of the heart, which are often not known to be such.

In the severer forms of structural lesion, the subacute inflammatory exacerbations may often need more than blood-letting; and here a few doses of calomel and Dover's powder, or analogous compounds of mercury with a narcotic and diaphoretic, are often of signal benefit. It is not always necessary to carry them to the extent of affecting the gums. A dose every night, for four or five days, carried off in the mornings by a mild salts and senna draught, will often suffice. But if the increased quickness, sharpness, or irregularity of pulse, palpitation or dyspnœa, with pain or inability to lie down, continue, calomel or blue pill should be given until the system is slightly affected. Under these circumstances, blisters are also often very beneficial; especially where there are signs of effusion into the pericardium or pleura, or considerable bronchial inflammation and secretion. A succession of them is often necessary, or a seton or issue may be substituted for them. In valvular disease, with inflammatory exacerbations referrible to the endocardium, I have found the tartar emetic solution a more effectual counter-irritant than blisters.

When the exacerbations have been in great measure relieved by these measures, their effect may be often kept up by milder means, which do not reduce the strength—such as colchicum, hydriodate and nitrate of potass, and the expressed juice or extract of taraxacum, which may ultimately be combined with, or exchanged for, some mild tonic—such as infusion of calumbo or cascarilla and nitric acid, with a little bitter or aromatic tincture. In these and all other circumstances, the state of the alimentary function should be carefully watched and its irregularities treated with appropriate remedies, which I need not specify. Nothing is more apt to irritate an injured heart, and to bring on attacks of palpitation or asthma, as disorder of the stomach and bowels.

But the exacerbations of organic diseases of the heart are sometimes the result of weakness and cachexy, rather than of inflammatory action; or if there be a little local mischief of this kind going on, the state of general debility and atony is such that nothing of the antiphlogistic kind beyond a blister, a mustard poultice, or some such slight local treatment, can be ventured on. In such cases, which, if severe, are the most hopeless, stimulants and tonics, as recommended for defective action of the heart, must be used; but from the permanent nature of the lesion, they are soon apt to lose their influence, and must be variously combined and changed, to maintain it as long as possible.

Dropsy, continued or often recurring in its various forms, when unattended with signs of considerable vascular power, is also an unfavourable symptom; for it generally implies either that there is permanent inadequacy of the heart to its task, or that the great secreting organs, the liver and the kidneys, have also become diseased. When dropsical effusions are the result of increased excitement of the heart, or of the supervention on it of an attack of bronchitis, or other occasional disease, they may be only temporary, being the result of an effort of the blood-vessels to diminish the bulk of their contents. But in any case, dropsical effusions may, by their pressure and bulk, interfere with functions, and therefore require special treatment. As long as relief can be obtained by diuretics, where the urine is not permanently albuminous, or by diaphoretics and mild purgatives, where it is, it is better to adhere to these remedies; but when they fail, which they too often do, recourse must be had to the more violent measures — hydragogue purgatives, acupuncture, and paracentesis. Of the diuretics for dropsies from disease of the heart, with considerably increased action, the saline diuretics, nitrate and acetate of potash, with infusion of digitalis or decoction of senega, have appeared to me to answer best. When the heart has less strength and more irregularity of action, particularly if accompanied with albuminous urine, the decoction of the pyrola umbellata, with tincture of cantharides

and sp. of nitric ether, is more suitable. Squill, with blue pill and a little sulphate of iron, as recommended by Dr. Abercromby, is also sometimes effectual, but it is very apt to disorder the stomach. In quite asthenic and cachectic cases, I have sometimes seen the tartarized iron prove a safe and effectual diuretic; and I have, in two or three instances, added a grain or two of the iodide of iron to a saline diuretic, with good effect. But the treatment of dropsy is too extensive a subject for our time; and although it do arise so commonly from disease of the heart, even when the kidneys and liver are also in a morbid state, I must refer you to books, as that of Dr. Seymour, and the article on Dropsy in Dr. Copland's Dictionary.

Although the chief medicinal treatment that is needed in structural disease of the heart, is that directed against its secondary effects, or the temporary exacerbations, yet I do not deem it useless sometimes to prescribe for the permanent lesion. It is true that we cannot remove this lesion, but we may make the heart and nerves less sensitive to its effects. We cannot remove the organic causes of epilepsy, but there can be no doubt that, under the use of nitrate of silver, the operation of this cause is diminished and the fits rendered less frequent. So by the use of the same remedy, or of the subnitrate of bismuth or the oxide of zinc, in organic disease of the heart in persons of a nervous temperament, I have seen a diminution of those occasional exacerbations which constitute the most harassing part of the complaint, and which are in part dependent on an extreme sensibility of the heart and nervous system; which feel the organic imperfection, and occasion vain and hurtful struggles against it. But this object is more commonly and surely obtained by means of regimen.

I have already said so much of the great importance of preserving a *balance* of the functions in structural diseases of the heart, that you will be prepared to expect that, in the few recommendations which it remains that I should make on the *regimen* proper for them, an equilibrium, or rather an activity of the most even, tranquil kind, will be the principle and aim of the whole plan.

The diet should generally be of as nourishing a quality as will suit the digestive organs, and will not produce plethora or inflammation. It is a great mistake to suppose that diseases of the heart are to be cured by starving. Except when inflammation is actually present, or in case of hypertrophy with continued increased action, or where pulmonary hæmorrhage or apoplexy is threatened, the diet should be one of moderation, but not abstemiousness. In most cases real strength in the organs of circulation, and in the muscles generally, is defective, whilst the irritability of the nervous system is undiminished. Now if tone and strength are to be given to the muscular system, and tranquillity to the nervous, by any mode of diet, it is by one of a nutritious character, with a due proportion of good animal food; and whilst it may be necessary to limit the proportion of the meat, and vary its mixture with less nutritious articles, especially farinaceous, according to the state of the digestive organs and the wants of the system, it is particularly important that in no case should the food, whether solid or liquid, be bulky in quantity. A distended stomach is apt, both mechanically and sympathetically, to excite or disorder the heart's action; and many are the individuals who suffer from palpitation under such an influence, when at other times they are scarcely sensible of it. The quantity of liquid food should also be limited, for a reason that I have already stated, to keep down the mass of the circulating fluid. It is very common for patients with diseased hearts to suffer severely after drinking too freely, although it be only of tea or water. The appetite for excessive quantities of drink is more the result of habit than of real want: by exercising a little self-denial in the first instance, the feelings of thirst gradually abate; and comfort and health are alike retained by drinking sparingly.

The expediency of avoiding active exertion is so obvious that I need not dwell on it; but there are particular kinds of exertion, which without appearing to be violent, greatly try the heart's powers. I would instance walking up stairs or up hill, or using even moderate exertion in a constrained posture. Against these the patient must be particularly cautioned. I have known great improvement ensue on a patient removing from a house and country in which he encountered stairs or hills at every turn, to those in which all was nearly on a level. This point is the more important, because moderate exercise is in most cases highly beneficial; and the more of this that can be taken in open healthy air, without inducing fatigue, the better for the strength and regularity of all the functions. This must be done in some cases only on foot; but it is desirable to vary the mode of exercise, and to those accustomed to ride, no motion is more suitable or salutary than the gentle pace of an easy horse. Where riding is not borne, carriage exercise is the only other resource; and it may well be combined with walking, but, when taken

alone, it is inferior to either of the others. Travelling by easy stages about a pleasant country is sometimes beneficial in many ways—by the amount of moderate exercise, the change of air, and the mental amusement, which it affords. In the intervals of exercise, which constitute the greater part of the day, it is very essential that the body should really be at *rest;* and by easy horizontal or inclined postures, on sofas, couches, or other contrivances, that the labour of the heart be aided as much as possible in propelling the blood to all parts. When the capacity for exercise fails, it is proper to assist the defective circulation, particularly in the extremities, by daily friction. In all cases this may prove salutary, by promoting the function of the extreme vessels and the skin. With the same object it is highly important to have a sufficiency of woollen clothing; and if the skin is habitually dry, its condition may be improved by warm salt water baths. In a relaxed state of the surface, sponging freely with tepid or cold salt water or vinegar, followed by friction, proves equally salutary.

Moral and mental quietude is most desirable, but here again not inactivity. According to the natural dispositions, various degrees of mental occupation and social intercourse are necessary; and so strongly is the action, and thence the structure, of the heart influenced by metaphysical causes, that there is no morbific circumstance more pernicious than violent mental passions, and no medicine, in chronic cases, so salutary as the calm enjoyment of agreeable social relations, and of amusing intellectual pursuits. We speak *practically* not less than *poetically*, when we place these passions in the human heart; and as we hear of many a whole heart rent and broken with grief and trouble, or burst with unbridled passion, we may well judge what havoc these feelings may cause in a heart already unsound. It is not often ours to "minister to a mind diseased, or cleanse the stuffed bosom of that perilous stuff that weighs upon the heart;" but we may often enlist the patient's understanding and well-grounded fears on our own side, when we counsel him sobriety in his sorrow and calmness in his care: on us may devolve the privilege and responsibility (and in no case does the conscientious physician, with his best exertions, more need divine help to direct his judgment aright) of advising and deciding with regard to important changes in the relations and occupations of his future life.

LECTURES ON BLOOD-LETTING;

Delivered from time to time,
At the General Dispensary, Aldersgate Street.

By Henry Clutterbuck, M.D.

Formerly one of the Physicians to that Institution.

Lecture XIV.

Of the use of Blood-letting in Palsy.

Palsy is usually defined, "a loss of feeling or of motion, or both, in any part of the body." It arises from the part being deprived of the nervous influence. The primary cause may be seated in the brain itself, in the spinal cord, or in the nerves proceeding from these. The greater number of palsies originate in the brain, and to these our attention at present will be confined, as most connected with our immediate subject—the use of blood-letting.

When the brain is forcibly pressed upon in any part, or rendered incapable, by disease, of performing its office, the parts in connexion with it, however remotely situated, may be paralyzed; that is, may be deprived of feeling or of motion, or both together: so that palsy is rather a symptom or consequence of disease, than a disease in itself. It is important to make this distinction, because, generally speaking, relief can only be given by attending to the immediate cause or source of the disease; applications to the paralyzed part itself being nearly useless. Thus, stimulating frictions to a paralyzed limb, or electricity (the application of which was formerly highly vaunted,) are, in reality, of little or no avail.

The ordinary form of cerebral palsy, or palsy originating in the brain, is commonly that of hemiplegia, where one side of the body, or a portion of it, is in a state of paralysis, while the other side is free. The affection of the brain which produces this is very generally seated on the side opposite to the side of the body that is paralyzed. This curious circumstance is explained by the fact of the decussation, or crossing, of the *corpora pyramidalia*, at the lower part of the medulla oblongata, where it joins the spinal cord. When the injury or disease is seated higher up in the medulla oblongata, where no decussation of fibres is observed, the paralysis takes place on the same side of the body, and not on the opposite, as in the former case. A more extensive affection of the brain may give rise to what is called paraplegia, where both sides of the body are paralyzed at once—a fact well known to Morgagni,

though treated of by Dr. Baillie, in his "Morbid Anatomy," as a new discovery.

The affection of the brain that gives rise to palsy is, in most cases, decidedly inflammatory in its origin. It is often, though not always, preceded by pains in the head, and is frequently accompanied by a febrile state of system, as indicated more particularly by fur on the tongue. This partial inflammatory state of the brain, which renders it for the time incapable of performing its office, so as to paralyze the parts that are under its influence, may quickly subside again, either spontaneously, or by the aid of antiphlogistic treatment; when, the part being restored to its functions, the paralysis ceases. But it is seldom that the termination is so favourable as this: the local inflammation in the brain may cease, but its effects, viz. tumefaction, induration, and the like, may remain. In this case the paralysis of parts continues, and often for life. And even where temporary recovery takes place, there is frequently, from time to time, a return of the inflammation in the brain, owing, perhaps, to some temporary excitement; when the paralysis is renewed, and generally in an aggravated and extended form.

The exciting cause of this affection of the brain is not always perceptible, the disease often taking place unexpectedly, especially in advanced life, and without any premonitory signs. In most instances, however, the foundation of it appears to be laid in excessive and long-continued excitement of the organ; as by habits of drinking, or severe mental application, especially if accompanied with anxiety. Palsy in many instances succeeds apoplexy, particularly when the latter is occasioned by extravasation of blood in the brain. In such cases the arterial fulness that occasioned the impeded or interrupted circulation through the organ (which is the immediate cause of the apoplectic state,) gradually ceasing by the decline of the arterial excitement, consciousness is restored, but the local and partial disease is left behind, and the paralysis only remains.

Either of the organs of sense may become paralyzed in the same manner; disease arising in the parts of the brain that are connected with those organs respectively. This commonly takes place slowly. It may, however, occur suddenly, a person awaking in the morning deprived of sight, without any defect in the eye itself; of which I have seen more than one example.

The treatment of paralytic affections of the kind now described is sufficiently simple; and sometimes as successful as could be wished; though, in the majority of instances, the disease, with more or less of mitigation, continues through life. It consists primarily in a local and partial affection of the brain, and produces its effect according to the particular seat and extent of the disease. In most instances, we are warranted by the symptoms in referring it to inflammation, but of a slow and inactive character. And when the disease ultimately proves fatal, such alterations of structure are found in the brain, as nothing but inflammation could produce. It is therefore reasonable to conclude from analogy, that the same applies to the disease in general.

We have to treat, therefore, either actually existing inflammation, or the consequences of it. The first point is, to determine whether inflammation still exists; and this is to be ascertained by the ordinary signs, such as heat and pain in the head, (though the latter is by no means essential) flushing of the face, and, as the least equivocal, a febrile state of system, of which a coated tongue is alone sufficient evidence. But even in the absence of unequivocal proofs of existing inflammation, it is proper, as the safest plan, provided the disease be recent, to assume the affirmative, as we know that inflammation may be going on in internal parts, without betraying itself by the usual signs; while there is generally sufficient reason to believe the disease to be founded originally in inflammation. But, of course, in proportion as the matter is doubtful, the greater is the caution with which we should proceed.

It being probable, therefore, that inflammation is still existing, (the disease being recent) the treatment should be such as is calculated to mitigate or subdue this. For the attainment of this object blood-letting is by far the most important, and, at the same time, the least equivocal means we possess. These, however, are not cases for a large abstraction of blood; small and repeated bleedings are alone proper; the quantities and times of drawing, being regulated, of course, by the circumstances of the individual case. And it is worth observing here, that feebleness of pulse, even with irregularity, when not combined with other proofs of general weakness of system, is not an absolute objection to loss of blood; for if this be employed to a moderate extent only, it is often followed by improvement both in the strength and regularity of the pulse. Numerous instances of this sort have come under my observation, in which, when they have occurred under favourable circumstances, as at a tolerably early period of life, the paralysis has been wholly and permanently relieved. In a still greater proportion of cases, by a similar mode of practice, the

disease has been kept at bay, as it were, for many years, and the patient has either ultimately recovered, or died at length of some other disease. I have at this time under my immediate charge two or three instances, where, for a very long period, the patient has been bled to the amount of six or eight ounces, once, and sometimes oftener, in a month; and always with the decided effect of warding off the threatening symptoms. This is done, too, without any loss of general strength, or other inconvenience, provided the appetite remain, (which it generally does) and a reasonable quantity of plain simple food, either animal or vegetable, be taken. The other parts of the antiphlogistic practice proper to be pursued, are too well known to call for particular notice. *Stimulants* of all kinds are to be avoided, with the exception, perhaps, of the ammonia, which may sometimes be useful for the purpose of equalizing the general circulation. A few years ago, the active principle of the nux vomica, the strychnine, was said to restore the lost power of the muscles and paralysis; but, if I might judge from the result of many ineffectual trials of this boasted specific, I should be apt to conclude that it no longer possessed such an extraordinary power. Little more can be said, I fear, in favour either of galvanism or electricity, when applied to the paralyzed muscles. These agents produce temporary muscular contractions; but the power and even the disposition to act are rather diminished by their continued use.

A mercurial course has been strongly recommended in these cases. In a few instances, I have employed this with apparent advantage, after the use of blood-letting in the manner above stated. But in many more, it has failed to produce any good effect on the disease, while it has injured the general health.

Of the Use of Blood-letting in Epilepsy.

Epilepsy is an affection of the brain, and when perfect, involves all the primary functions of the organ. So far it resembles apoplexy, from which it differs, however, in regard to the voluntary power, which in apoplexy is suspended, while in epilepsy the voluntary muscles are acting with preternatural and irregular violence (convulsions). There is a kindred alliance, nevertheless, between apoplexy and epilepsy, as, indeed, between all the affections of the brain. We find them at different times variously intermixed, and mutually convertible into one another, doubtless according as the local disease in the brain extends itself, or shifts its seat in any degree. Thus apoplexy, when severe, most commonly terminates in palsy; while, after repeated milder attacks, it often degenerates into epilepsy; and this, again, sometimes ends in apoplexy. In all of them the mental faculties are occasionally impaired or disordered; and, on the other hand, insanity is often accompanied by epilepsy or palsy, and terminates its career in apoplexy. From this consanguinity, as it were, between the different brain-affections, the treatment of them all is governed by nearly the same general principles.

Epilepsy is not always so complete as to correspond with Dr. Cullen's definition, viz. "*convulsio musculorum cum sopore*," for in many of the slighter cases consciousness remains.

Epilepsy, which, like palsy, is rather a symptom than a disease, is produced by various remote causes, one of which is recent and active inflammation of the brain. This is apt to occur in infants more especially, in whom *convulsions* (*i. e.* epilepsy) often take place as an immediate effect of cerebral inflammation. These convulsions sometimes prove quickly fatal; or they may terminate in serous accumulation (*hydrocephalus acutus.*) If the inflammation in such cases be quickly subdued by active antiphlogistic treatment, the epilepsy may not recur.

Epilepsy may be induced by disturbance of the brain from different causes without inflammation. Thus acute pain, wherever seated, and irritation of certain parts without actual pain, will be followed occasionally by epilepsy or convulsions, as in infants or others possessing great susceptibility. Disturbance of the cerebral circulation may have the same effect, as when the vascular action of the brain is much excited by the use of strong drinks; and it is remarkable that disturbance of the general circulation, by a large and sudden abstraction of blood from a vein, has sometimes the same effect. The effect in this case has been ascribed, though erroneously, to inanition simply, as if the quantity of blood in the vessels of the brain were diminished. Another powerful source of disturbance to the brain, producing epilepsy, is violent mental emotions, as terror, or even joy.

These causes are all uncertain in their operation, and require a certain degree of susceptibility or predisposition to give them effect. This predisposition may be constitutional, as in infants, and sometimes even in adults; but it more frequently arises out of that irritability which inflammation produces, and often leaves behind it, and which is generally accompanied by more or less of change of structure in the part. When epilepsy has subsisted long, it appears to be kept up, or

rather the predisposition to it, by such alteration of structure in the brain; as dissection has sufficiently proved.

The proper treatment of epilepsy consists, first, in removing or avoiding the exciting cause, when practicable; and, secondly, in lessening the predisposition, which consists in a morbid irritability of the brain; so that, if this be accomplished, the exciting causes may fail to produce their effect. The power we have to fulfil either of these indications is, however, very limited; and, in consequence, it is but in few instances that a cure is effected.

When epilepsy is recent, and the result of inflammation in the brain, the treatment is as simple as that of other inflammations. In such cases blood-letting may be considered among the most effective means of cure. If the disease has been of long standing, blood-letting is a very questionable remedy, and, indeed, generally fails to accomplish a cure; doubtless because the disposition to the return of the paroxysms is kept up by the change of structure in the brain, which art has little or no power over. Our object, then, is necessarily limited to the lessening the predisposition to the return of the paroxysms; but our efforts here are but little more successful. Time occasionally effects much in this way, the disease sometimes ceasing spontaneously, and appearing to wear itself out, and that although the morbid change of structure should remain, as it probably does. I have in some instances observed the disease to yield to a frequent repetition of small bleedings, but they have more frequently failed. In young and tolerably vigorous subjects, however, this plan is well worthy of a trial, as, should it not succeed, no inconvenience is likely to follow.

Narcotics of different kinds suggest themselves as not improbable remedies on these occasions, by their known power of influencing and altering the condition of the brain, the suffering organ. My own trials, in this way, have been limited to the use of opium; but I cannot say that the result in general has answered my wishes, although a temporary improvement has sometimes taken place from it. I question whether more can be said in favour of the other narcotics: the almost entire disuse of them at present, is a pretty good proof of their insufficiency. Mercury has always proved useless in my hands, as a remedy for epilepsy; and, indeed, has rather seemed to do harm, by increasing the general irritability of the system.

A variety of *tonics*, as they are called, (though it is far from certain that they operate upon the principle supposed,) have been employed in epilepsy, but with very uncertain advantage. Under this head have been included not only vegetable astringents, but a number of mineral substances likewise; as the preparations of iron, zinc, copper, silver, and even arsenic; but the general result has been the same. Even mesmerism itself, the ghost of a former age, has been pressed of late into the service—with what result, I need not state. With regard to the paroxysms of epilepsy, it is of no use to administer any remedy at the time; though they may sometimes be warded off (where they are foreseen) by an active stimulant; such as the ammonia simply, or combined with castor or valerian, as more medicinal in appearance.

Hysteria may be considered as a mild and imperfect variety of epilepsy, from which it differs chiefly in the disordered state of the alimentary canal, as evinced by flatulency, spasm, globus hystericus, vacillation of mind and feeling, often attended with convulsions. In mild cases of hysteria, consciousness remains; in the more severe ones, even in the same individual at different times, there is as entire an abolition of sense and intellect, and as violent convulsive movements, as in ordinary epilepsy; into which, indeed, it sometimes degenerates. It is less regular, and, in general, less frequent in its returns, than epilepsy; and it is induced by slighter causes, as mental emotions and irregularities in the uterine functions: hence the origin of the name assigned to this affection.

Hysteria consists in a disordered condition of the brain; as may be justly inferred from the symptoms of the disease. When, however, the trivial nature of these, on many occasions, is considered—the slight mental causes that induce it; and the long and uncertain intervals between the paroxysms; and also their speedy and entire termination in most cases, when the causes are no longer applied—there seems reason to believe that it is in general merely the effect of excited and disordered vascular action in the brain, unaccompanied by any disorganization.

Occurring, as hysteria does in most cases, in delicate and sensitive habits, with little general strength, there is rarely occasion to resort to blood-letting for its relief. It is of more importance to lessen, if it can be done, the predisposition, and which is to be effected by lessening the irritability or excitability of the brain: but our power to do this is very limited—cold bathing, both general and local, to the head, and the use of the vegetable tonics, are perhaps the most promising. The arterial system of the brain is in a highly-excitable state in many of these patients, who are subject, in consequence,

to frequent returns of violent throbbing headaches, which are brought on by the slightest causes of disturbance in the system. In many such cases I have found it useful to excite the action of the cerebral vessels in a gentle way, and at short but regular intervals; as by admistering small quantities of diluted alcohol, three or four times a-day; always choosing, however, those times when the headache is absent. Accustoming the vessels of the brain to this mild degree of excitement, appears to lessen their excitability in regard to other agents. Habitual dram-drinkers rarely suffer much from headache.

But although, generally speaking, blood-letting is not required for the relief of hysteria, this disease sometimes takes place in vigorous and sanguine females, especially where menstruation is suppressed; and also where it is accompanied with violent uterine pains and general febrile symptoms. In such cases, bleeding to a moderate extent is of unquestionable advantage. I have seen such cases terminate in fatal apoplexy, thus shewing the nature and origin of the disease.

Tetanus is a violent spasmodic or permanent contraction of the voluntary muscles. In extreme cases, the whole voluntary system is affected; in slighter cases, the spasms are confined chiefly to the muscles of the head and neck; as in what is called locked-jaw. Tetanus is probably a brain affection, as it affects the muscles that derive their nerves from within the cranium, as well as those that receive their nerves from the spinal cord. It is, however, confined to one only of the sensorial functions; neither sensation nor intellect partaking in the disease.

Tetanus is induced by external injuries of the brain; and therefore, probably, by disease also in this part; but if so, it does not, according to Mr. Hunter, leave behind it any visible traces of its existence. It is often brought on by irritation of remote parts, as the extremities; and that even without much pain. Some instances have been related of the cure of tetanus by blood-letting, but it has much oftener failed. Opium has been given in such quantities as, in other circumstances, would have destroyed life, and the patient has died notwithstanding—whether from the disease or the remedy is a problem not easily solved. The disease has often appeared to cease spontaneously, when all remedies have been laid aside. And it has also been observed that if tetanus continues for a week, it rarely proves fatal afterwards, if left to itself, though it may be long in going completely off.

Of Blood-letting in Chorea.

This is an involuntary agitation of the limbs, and generally of the muscles of the face also, giving rise to various odd and ridiculous gesticulations of the body and contortion of the features. It commonly is most conspicuous on one side, like hemiplegia. It is often attended also with some imbecility of mind. It appears most frequently in delicate subjects, though by no means exclusively so; so that there is no pretence for considering it, as is often done, as essentially connected with or dependent upon debility. It has been commonly treated by purgatives, with tonics in the intervals of purging. The disease in most cases subsides under this treatment; but seldom till after an interval of many months, so as to render it a matter of great uncertainty whether the remedies employed had really much to do with the cure. I mention the subject here for the purpose of observing that I have on many occasions, in the practice of this Dispensary, used small and repeated bleedings in this disease, with unequivocal and permanent advantage, and that where remedies of a different kind have failed. So that where there is much headache, as is often the case, with febrile symptoms, and where, at the same time, the habit is tolerably strong, I feel no hesitation in adopting this remedy, aiding it by occasional purgatives, with blisters *ad nucham*. If we may judge from the symptoms, both of body and mind, the disease appears clearly referrible to the brain, and to consist in a partial and chronic inflammation of this organ; very similar, indeed, to hemiplegia, into which it sometimes degenerates.

Of the Use of Blood-letting in Vertigo and Headache.

Vertigo is a frequent attendant, as well as premonitor, of the most dangerous brain affections. It often precedes the attack of apoplexy, and is therefore always to be treated with much attention. It commonly requires abstraction of blood in some way for its immediate relief, or, at least, in order to guard against the more serious results to which it is apt to lead. The use of the remedy is of course to be governed by all the circumstances and cautions that I have so often urged as necessary to be attended to.

Headache is a more equivocal affection, in regard to its treatment. It frequently precedes and accompanies brain affections of the most dangerous nature; but on many other occasions it appears to be of little moment in regard to its consequences, although highly distressing to the feelings of the patient. Nor is it always easy to discriminate between the lighter and the more dangerous forms of the disease. If the pain in the head be accompanied with vertigo, or any other disturbance in the

sensorial functions, it is always to be considered in a serious light.

In the treatment of simple headache, the general strength of the system is to be looked to. Of course, the obvious causes are to be removed as far as possible. If the habit is strong, an antiphlogistic treatment is proper, and, generally speaking, blood-letting, as a part of this. But in weak and delicate subjects, blood-letting may generally be dispensed with, in favour of other means. Speaking of hysteria, I remarked upon the great advantage occasionally derivable from the use of alcohol in small and repeated doses, as a remedy for headache. Opium, though it often excites headache, is yet powerful, at times, in relieving it. This remedy, however, generally does much more than is desirable; producing, for example, costiveness, loss of appetite, nervousness, as it is termed, and lowness of spirits, which more than counterbalance the benefit it is capable of affording.

Of the use of Blood-letting in Insanity.

Insanity, or mental derangement, philosophically considered, is a subject of the widest range, and involves a variety of questions, physical, metaphysical, and moral. But when looked upon in merely a medical point of view, and in relation to practice, it is perhaps one of the most simple.

It is hardly possible to give a strictly philosophical definition of insanity, though it is not difficult to furnish one that is adequate to professional, and even legal, purposes. It may be defined, then, a state of mind which disqualifies a person for the ordinary concerns of life, or which may render him dangerous to others. This definition, it must be admitted, is far from perfect. It may be said to include both idiocy (*dementia*) and infancy (*amentia*,) which can scarcely be said to be unsound states of mind. It might even be considered as including those temporary derangements of intellect that are produced by the use of intoxicating drugs and liquors, and also the delirium of fever. But, in truth, all definitions on such subjects are, in their nature, imperfect. The distinctions, however, that are necessary for practical purposes, may be always made without much difficulty.

Mental disorders have been classed together, by nosologists, under the general denomination of *vesaniæ*, and have been divided into genera and species, like the subjects of natural history. Thus, mania and melancholia have been considered as forming two distinct species of mental disease; and hypochondriasis, which is characterized by an unreasonable anxiety and erroneous judgment concerning one's own state of health, has been looked upon as a third species. Subdivisions, also, have been made according as one or another of the mental faculties happens to be disturbed. But there is no end to the possible variety of character occasionally displayed in insanity. The different faculties of the mind may be disordered singly, or in combination, and that variously. But, in fact, insanity or mental derangement is merely a symptom, and not a substantive disease. It is one of the signs of disordered brain, just as flatulency is a sign or symptom of disordered stomach. Insanity, therefore, in all its varieties, whether it assumes the character of mania, melancholia, or hypochondriasis, always implies a morbid condition of brain, this being the sole organ through which the phenomena of mind display themselves.

We know nothing, in fact, of mind, but as connected more or less with the brain; although of the nature of the connexion we know absolutely nothing. I think you will best understand the subject if I trace a case of insanity, from its commencement in phrenitis or simple inflammation of the brain, to its complete establishment in one of the forms of permanent mental derangement, as ordinarily met with in practice.

A person, for example, from excess in the use of intoxicating drinks—or from mental excitement, long watching, injuries of the head, or some less obvious cause, is attacked with symptoms indicating inflammation in the brain; such as pain and heat in the head, throbbing of the temporal and carotid arteries, flushing of the face and eyes, with a white tongue and other febrile symptoms. These are accompanied with, or soon followed by, delirium. Under favourable circumstances, and by the early adoption of active antiphlogistic measures, the symptoms are subdued—the febrile state subsides, sleep returns, and the delirium in a great measure disappears, but not entirely: the mind, perhaps, continues in a state of derangement for an indefinite length of time, but with no corresponding bodily disease; none, at least, that can be perceived. To this mental derangement, now become permanent, the terms mania, melancholia, or hypochondriasis, are severally applied, according as the patient happens to be violent in his conduct, or depressed in spirits, or occupied with unreasonable fears and apprehensions regarding his bodily health.

In a great proportion of cases, especially if it be a first attack, and no strong or hereditary predisposition exists, the mental disorder, after a time (seldom less than several months, or often longer), begins

gradually to decline, and at length wholly ceases. On other occasions, the insanity continues through life. In some cases, the maniacal symptoms are constant, varying from time to time in intensity, according as the brain is casually excited or disturbed by circumstances. At other times there are intervals, more or less protracted, of perfect sanity. In a word, the greatest possible diversity exists in all these points, no two cases being in all respects alike. Thus insanity, like epilepsy, has its paroxysms and its intervals; the former, as in epilepsy, sometimes arising without obvious cause, but frequently, also, from causes calculated to excite the arterial action of the brain; such as strong drinks, or mental emotions of any kind. But in all cases there appears to be a disposition to recurrence, and which trifling causes are sufficient to bring into action.

Now all this, physiologically considered, is easily understood by the analogy of other organs. The inflammation, in proportion to its duration, effects a change, greater or less, in the organization of the part; inducing, at the same time, a corresponding change in its vital properties, and, consequently, in the functions which it performs. The inflammatory action may subside; but the physical changes which the part has undergone may continue. These, like the induration and swelling of a gland, or any other external part, may slowly disappear; but till it does so, the function is not likely to be perfectly restored. If recovery should take place within a short period, as a few days, the mental disorder retains the name of delirium; but if it continue afterwards, and after the febrile symptoms have disappeared, it is called insanity.

Many writers consider insanity to be purely a mental affection, upon the ground that no uniform manifestations of bodily disease are discoverable after death; and also from the fact, that the passions of the mind are the great exciting cause of the malady. Others, again, contend for the corporeal origin of insanity, because, in their estimation, it would be to degrade the soul or mental principle, to suppose it to be, like matter, susceptible of change. Moral causes have, no doubt, a great influence in the production of insanity. The disease, it was remarked, was extremely prevalent during the violence of the French revolution. But corporeal causes, as injuries of the brain, and drunkenness, are equally productive sources of the malady. Pinel, a French physician particularly conversant with the subject of insanity, places the primary seat of the disease in the alimentary canal. But this is to confound cause with effect.

The opinion very generally entertained at present is, that insanity is a brain affection; and with great reason, since the brain is the only organ through which the mental function manifests itself. The unnatural state of sensibility, (another of the cerebral functions) goes to prove the same thing. An insensibility to external impressions is a marked character of insanity, in a great number of instances. The chief argument adduced against the idea of the brain being essentially the seat of disease in insanity, is that the hardness of the cerebral substance and the thickening of the membranes which are met with in old cases, are not to be observed in recent ones. This, however, only serves to shew, that the obvious changes of structure alluded to are not the immediate or proximate cause of the symptoms—a conclusion that is strengthened by the fact, that similar appearances are often found in other diseases of the brain, where no mental disorder existed. Such appearances are decisive in proving that disease was actually present in the brain at some former period.

Upon the whole, then, with regard to insanity as a topical affection of the brain, it may be concluded that the paroxysms are the result of temporary arterial excitement of the organ, while the disposition, or predisposition, is founded in a morbid change of structure, the result of inflammation, or of hereditary disease.

In the treatment of insanity, as of epilepsy, we have to look, first, to the exciting causes; and next, to the predisposition, with a view of lessening this as far as possible.

By avoiding the causes of the paroxysm, the vascular excitement in which this consists may subside, and the organ return by degrees to a quiet performance of its office. This object may sometimes be promoted by the employment of antiphlogistic means, proportioned to the symptoms and the state of the general strength. If there are marked febrile symptoms present, blood-letting may be properly resorted to for their relief; but not because the maniacal state happens to be violent.

The removal of the predisposition is not so easy a task. When, for example, there is disorganization of the brain (which appears to be the case in the generality of instances, and, indeed, in all where the disease has been of long duration), the case is hardly within the reach of art. So, also, where the disease is hereditary, it is not likely that much, if any thing, can be done for its relief.

In short, the whole management of the insane resolves itself into the removal of causes, when they can be ascertained; the reduction of inflammatory action where any signs of it are detected (and even if a

suspicion only of this exists, it is right to be guided by it to a limited extent); the placing the patient altogether in such circumstances that he may be as little as possible exposed to excitement, either physical or moral; and lastly, to occupy and amuse the mind as far as circumstances will allow of, using such a degree of restraint only as may be necessary for the accomplishment of these objects, and for his own personal safety, as well as that of others. By such treatment, the disease of the brain, out of which the insanity of mind arises, will, in a great proportion of instances (if the malady be recent), subside after a time, and the mental function be restored to its healthy state. This, however, will seldom be accomplished till after an interval of some months; during which, seclusion from friends and general society is advisable. Great regularity in regard to all the ordinary occupations of life is necessary; and also a certain degree of restraint; but without harshness on the part of the attendants. The insane, for the most part, are like children; requiring control, and submitting to it with great readiness, as long as it is exerted with mildness and discretion.

After what has been now stated, I scarcely need add that attempts to cure insanity by particular remedies, or particular management, are little less than absurd. Insanity, in short, is not to be cured, though it will subside spontaneously, or from very simple management, in a large proportion of cases. It is to be always considered as a symptom merely of a morbid condition of brain, and as consisting either in disorganization, or in a morbid irritability of the organ, which renders it liable to be disturbed in its functions by any cause of excitement, particularly that of increased arterial action in the brain itself.

The employment of blood-letting, therefore, merely because the delirium is violent—the use of opium or other narcotics for the purpose of procuring sleep, where this is deficient—and, in short, all other violent means of impressing the system—are almost sure to prove injurious, while they are incapable of accomplishing the end desired.

The distinction of insanity into mania, monomania, melancholia, hypochondriasis, &c., may require some attention as regards the moral treatment and general management of the patient, but is of no importance in a medical point of view. The maniacal or furious madman does not, because such is the character of his insanity, require blood-letting or other debilitating remedies; and still less does the melancholic require stimulants; for the state of melancholia, with its depression of spirits, is often accompanied by high arterial action both in the brain and in the general system; calling for active antiphlogistic measures. In short, in insanity, as in all other diseases, it is to corporeal symptoms that we are to look for our indications of cure.

ON CATARACT,

WITH A CASE;

Being the Substance of a Lecture delivered at the Blenheim Street School of Medicine and Dispensary, August 15th.

By Thomas King,
Surgeon to the Dispensary.

I am inclined to consider the lens rather as an unorganized than an organized body; no trace of organization has ever been found in it; no vessels have ever been injected; and, what is more, it seems to have no solid connexion with the rest of the eye. Some anatomists admit that it adheres to the membrane surrounding it; but I believe the general opinion to be, that it is surrounded by the humor Morgagni. This want of solid connexion is perhaps the greatest proof that any part so circumstanced is not organized. The hair and the nails, with which the lens has some analogy, and which no one suspects of being organized, are more intimately connected with the body than it: they are united, the one to its bulb, and the other to the skin, by pretty strong adhesion. However, the explanation I have to give, as to the mode of formation of cataract and the degree of gravity of some of its principal forms, will equally apply, whether we consider the lens as organized, or view it as an unorganized body. The manner in which anatomists describe the capsule I do not consider to be strictly correct. The posterior layer is spoken of, as if it were distinct, and could be separated from the hyaloid membrane. The truth appears to me to be, that these two are so intimately blended, that, as far as regards an interference with them by an operation, it is as if there existed but one. I do not think they ever can be shewn to be separate and distinct membranes—that one can be removed from the other. When, therefore, the membranous layer behind the capsule, be it single or be it double, is divided or re-

moved by an operation, it necessarily follows, that the vitreous humor is let loose, because some of its cells are thus laid open in the place corresponding to the lens. It is on this account that operators so often lose a portion or the whole of the vitreous humour, in the operation by extraction; especially when they attempt to remove any opacity behind the lens. Now, whether the lens be unorganized, and nourished, as I presume, by a kind of imbibition of the humor Morgagni, or whether it be organized and receive vessels for its nutrition, I think the mode of action of the great cause of by far the greater number of true cataracts can be explained. In the former case it would be by a diminution, by a want of supply of the proper quantity of the fluid; and in the latter, by an alteration of the minute vessels, such as to interfere with the circulation in them. We know that a considerable quantity of water enters into the composition of the lens, and is absolutely necessary for its transparency. A diminution in that quantity must necessarily be followed by a diminution of transparency, by a certain degree of opacity, whether the fluid be furnished by imbibition or through the medium of vessels.

Age may be said to be the great cause of true cataract. The most remarkable change induced by age is a diminution of the fluids. Every part of the body may be said to become more dry, more rigid, and less elastic and transparent by its progress. Hence the aptness of the vessels to harden and ossify; and, certainly, the effect of the change in question is likely to be first produced in parts remote from the centre, and of delicate structure. The remoter the part from the centre, the sooner is it liable to be affected. Thus, the anterior layer of the capsule, being further from the centre than the posterior, *cæteris paribus*, more readily becomes opaque than the posterior layer; and the alteration that produces it, being proportionately less, it follows, that it constitutes a kind of cataract much less serious than the opacity of the posterior layer. It belongs to the first degree of deterioration the eye and its vessels undergo by the change in question. Of course, it is also far less serious, because it can be destroyed or removed without injury of the hyaloid membrane behind it. The lens usually becomes opaque before its capsule, because, in my opinion, its means of nutrition are less direct than those of its membrane. The minimum of alteration in a membrane is often evinced only in the matter it secretes; and, in this case, it needs only an alteration in the quantity of the humor Morgagni to change the state of the lens, so that, when, by the progress of age, the quantity of the humor Morgagni tends to diminish, the lens tends to become opaque. The effect of this diminution would be first produced in the centre of the lens, and gradually thence towards its surface; because the fluid first acts on the superficial layers, and must traverse and supply these before reaching the central ones. There may be enough for the former, but not enough for the latter. Now, the manner in which lenticular cataract forms, confirms this explanation. The opacity begins in the centre, and gradually extends to the circumference. The mode of action of the cause of cataract, establishes, then, as regards the gravity of the disease, three degrees. The minimum is that in which the lens is opaque, the next that in which the anterior layer of the capsule is affected, and the maximum is that in which the posterior layer is opaque. In the operation I have devised for cataract, I introduce the needle as low down as possible; and I have also explained at what distance from the margin of the cornea it should be introduced. My directions are suggested by the anatomy of the parts concerned. The most complex and delicate part of the eye I believe to be that between the iris and the vitreous humour: it comprehends the corpus ciliare, with the minute structures or appearances which have been described under the names of the zonula Zinnii, the orbiculus capsulo-ciliaris, and the annulus albidus. I contend, that the needle should be introduced between the termination of the retina and the ciliary processes—that is, at rather a full eighth of an inch from the circumference of the cornea. If we go farther back than this, we traverse the retina, and interfere with a good deal of the vitreous humor; if we come more forward, we are in danger of injuring the iris. Surgeons have given the caution not to approach the iris too closely, but they have not pointed out the inconvenience of introducing the needle far back; in fact, they assign no definite posterior limit to the space it

may traverse. My directions, with regard to the place at which the needle should be introduced, are given, first, for the purpose of avoiding the iris and more important parts of the corpus ciliare; and secondly, with a view to interfere as little as possible with the vitreous humor.

I make it enter the lower part of the eye. This has many advantages. The needle prepares a way and a place for the lens to pass through, and lodge in. The opaque body falls into these by its own weight. As soon as the anterior layer of the capsule is divided, it is only necessary, in most cases, to withdraw the needle: the lens follows it as it is being withdrawn. It is clear, then, that the vitreous humor is disturbed only in one place, either by the instrument or the lens; whereas, by the other plans of operating by depression, it is disturbed in several: first, where the needle is passed on to the pupil; secondly, in the place where the lens is lodged; and thirdly, it is broken down in the intervening space, by the passage of the needle, moved from one to the other, to displace the lens from the axis of vision. It is evident, the more the cells of the vitreous humor are broken down, the more impaired must they be. Heal up, in some way or other, they do, but not without cicatrices; and these, I have reason to believe, often form an impediment to vision afterwards—a kind of secondary cataract. Even, in the operation by reclination, there is more injury done to the vitreous humor, and I believe to the eye, than by the simple plan of depression which I propose. I cannot help thinking it a great disadvantage, to leave the lens in such immediate vicinity with the axis of vision as it is left, after reclination.

In deciding on the comparative value of the different operations proposed for cataract, we have been too apt to overlook the great consideration, which, above all, should influence our judgment—namely, the state, the degree of perfection in which the eye is left after them, as an organ of vision. It appears to me to be left more perfect, or with less injury, by the simple plan I propose than by any other. The needle is introduced at a defined place and fixed distance from the cornea, determined by considerations of safety, where no important part is injured.

The passage of the needle makes a way for the lens to pass through, and a lodge for it to lie in, where, indeed, it falls almost by its own weight, and from which it cannot easily rise again; because the vitreous humor, from having suffered little or no disturbance, keeps it in its place. It is applicable to all cases of lenticular and capsulo-lenticular cataract. Where the anterior layer of the capsule is opaque, it is to be reduced to shreds, which may be passed into the anterior, or left in the posterior chamber of the eye, to be absorbed. The posterior layer, when opaque, is to be treated in like manner, after the lens has fallen into its lodge. In this last case, the operation I propose will surely be preferred to extraction, since, as I have shewn, the removal of the opaque membrane lets loose, and is almost certainly followed by the loss, through the incision in the cornea, of the vitreous humour.

Case.—Mons. de K——, ætat. 64, a military man, rather thin, having the eyes deep in the orbits, of a good constitution, active habits, and lively disposition, now residing No. 1, Great Marlborough Street, underwent the operation for cataract by extraction, in the right eye, in 1833. The incision was made upwards, through nearly the superior half of the cornea. Probably owing to some difficulty in fixing the eye, or to some unforeseen accident, the operation appears to have been long and laborious; it was followed by severe inflammation, which destroyed sight in the eye. Some months ago he related his case to me.

The operated eye seemed to have been the seat of iritis; the pupil was nearly closed, and nebulous; the iris thickened and discoloured; a rather broad cicatrix marked the place of incision in the cornea. Vision extinct in this eye. The left eye was the seat of capsulo-lenticular cataract, which, seen through the pupil, was of a blueish grey colour; iris bright and contractile. The patient distinguishes the form of well illumined large objects. After the dilatation of the pupil with belladonna, of which very little rendered it extremely large for forty-eight hours, the cataract appeared, as if composed of a central part hardly more than two lines in diameter, of a blueish amber colour, quite opaque, and

an external part less opaque, and stellated; as if the lens, reduced in size, were incased in a thickened, opaque, grey capsule. While the pupil remained dilated, the patient saw better than when it was of its natural diameter. Having ascertained the patient could not see to guide himself, I reflected on the propriety of operating on the left eye; and, considering that the operation I have devised is almost without risk, I advised the patient to submit to it. He consented. As his body was in a good state, and his bowels remarkably regular, I prepared him for it, merely, by a rather more than usually careful diet. I performed it, as it is described in a preceding number of your journal, on the morning of the 16th inst. after the bowels had acted, and after placing a blister on the right arm, in a darkened room, in the presence of three of my pupils. The sunk state of the eye compelled me to make the patient turn it upwards more than I usually find it necessary; and the great projection of the orbital ridge rendered the raising and fixing of the rather flaccid upper lid difficult. In one instant the needle appeared at the pupil, and a few rotatory motions cut to pieces the anterior layer of the capsule, the fragments of which followed the instrument as I withdrew it, the lens preceding them in their descent. The pupil was at once free. The shade was dropped over the eye, and every precaution taken to keep the patient quiet. (Gruel and barley water.)

Little or no pain was experienced during or after the operation. A sensation of pressure, gradually subsiding, was felt in the eye for four or five hours; and, for about the same time, there was a little weeping from it. The patient remarked, that the water coming from it was cool; and contrasted it with what he called the scalding hot liquid, which escaped from the other eye, after the operation by extraction. In the evening he was perfectly easy; there was no disagreeable sensation in the eye. Pulse 70, regular and soft; skin moist; tongue clean.

17th.—The patient has slept well, is free from pain, and in excellent spirits. I allowed him a cup of cocoa for breakfast, and a bread pudding for dinner. The blister has risen well.

18th.—The patient feels rather feeble, but has slept well; the skin is perspiring freely; the eye and head are quite easy. I carefully raised the lid, after wiping it gently with a rag soaked in warm water, to examine the eye, and found the conjunctiva quite free from injection. At the lower part of the pupil, there remained, I thought, a small fragment of opaque membrane. The patient said, "I see, but not quite clearly." During the day he felt momentary, slight, darting pains in the eye.

19th.—Same satisfactory state. When I raised the lid for a few seconds, the patient said, "Now I see your features." The bowels acted after a pill, composed of ext. colocynth, c. gr. iii.; gum galban. gr. ii.

20th.—The patient remarked, as I raised the lid, "I see you; I see all the motions of your lips."

21st and 22nd.—The pupil appeared quite clear. The sight improves. I allowed the patient the sight of a lady who had come to see him. "Do you see her well?" said I. "Very well indeed," was the reply, uttered with joyful emotion.

23rd. — Deeming it advisable still to keep up some counter-irritation, I had a second blister applied on the place occupied by the first, which had nearly healed.

The following days.—The sight continued good, and the patient well.

28th.—The eye appears quite bright and natural. I have kept up a little counter-irritation behind the left ear, by means of an emplast. calfaciens.

Fig. 1.

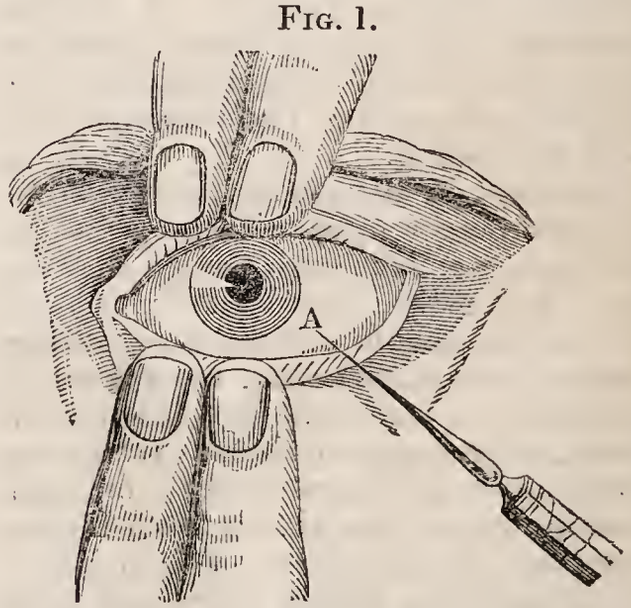

Fig. 1. shews the operation as performed on the left eye.

A. is the point of the sclerotica, at which the needle should be introduced.

Fig. 2.

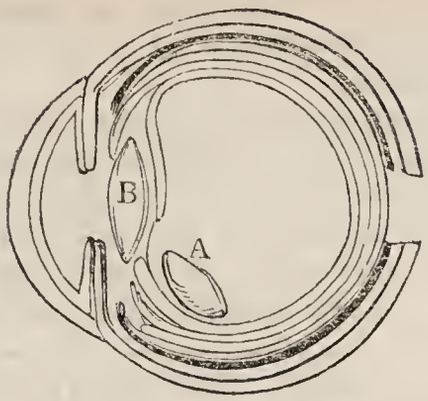

Fig. 2. shews the place to which the lens is removed by the operation.

A. The lens in the place it occupies after the operation.

B. The place occupied by the lens previously to the operation.

ON THE USE OF THE HEATED AIR-BATH,

IN CASES OF ASPHYXIA FROM DROWNING, &c.

To the Editor of the Medical Gazette.

Sir,

Should you deem the subjoined paper worthy of publicity, by inserting it you will oblige, sir,

Your most obedient servant,

G. A.

Senior House-Surgeon, Liverpool Northern Hospital.

August 30, 1838.

Amid the various agents in use for the recovery of persons apparently drowned, those employed in raising the temperature of the body's surface decidedly stand pre-eminent: with the view of effecting this object, various methods have been devised and described by different writers; but I am not aware of the heated air-bath having been either recommended or employed. The importance of this adjuvant may be estimated by a perusal of the following details:—

The heated air-bath has been used by me, in the Liverpool Northern Hospital, in nearly every case of asphyxia from drowning brought there within the last *nine* months; and, did time and opportunity serve, numerous cases might be adduced in support of its utility.

I was led to employ this agent by the following circumstance:—A case was brought into hospital during night, when insuperable obstacles presented themselves to my obtaining a sufficiency of warm water to provide a bath in the usual manner. An old air-bath (similar to those used in hospitals during the prevalence of cholera), which happened to be at hand, was instantly applied, and in a few minutes the temperature was raised to 90 Fahrenheit. The patient recovered.

This case impressed me with a due sense of the importance of the agent I had so accidentally stumbled upon. I thenceforward determined to use it on every occasion, and my endeavours have been crowned with success in many instances where I had hardly dared to look for it.

Before proceeding further I shall describe the apparatus, its appliance, and the advantages to be derived from its use, in preference to the warm bath, or other artificial methods of raising the temperature of the body. The instrument used is formed of two tubes of sheet-iron, each three feet long and four inches diameter, joined at right angles. The lower part, or pedestal, is wider, and made of heavier metal than the tubes, to give stability to the instrument. In this is placed a spirit-lamp, furnished with eight or ten separate wicks, each provided with a stopper, by the removal or appliance of which the temperature may be rendered subservient to the will of the operator.

Now with our air-bath ready, let us suppose a person asphyxiated brought in. The wet clothes are stripped off; the body dried and placed upon a table covered with a folded blanket; a pillow is then placed under the shoulders, and another under the sacrum; a couple of fracture cradles are applied over the extent of the body, and these covered in with blankets. The mouth of the tube conveying the heated air is then introduced near the feet of the patient, and in five minutes you may have an atmosphere of 100 Fahrenheit in contact with nearly eleven-twelfths of the whole surface.

Nor does the superiority of the heated air over the warm water bath end with its being more accessible, or of easier application. No one, I think, will deny the necessity of keeping up the temperature while performing artificial respiration, yet to inflate the lungs while the patient is immersed in a warm bath of the usual construction, is nearly impossible. In using the heated air bath the position of the patient is admirably

adapted for manipulating with the bellows, &c. While employing the air-bath, the very warmth and rarity of the atmosphere engendered tends toward the expanding the walls of the chest; whereas in the case of the warm bath, the gravity of the fluid pressing on all sides of the thorax must be no slight disadvantage to contend with, in our attempts to fan into life any feeble spark which may still exist.

The necessary evaporation from a surface removed from warm water is also dispensed with—in itself no unimportant matter; and by using the split cloth as an assistant in performing artificial respiration, the capacity of the chest may be diminished at pleasure, without disturbing the coverings of your apparatus.

Another and important advantage derivable from the use of the heated air bath is, that on account of its appliance being so rapidly performed, and the atmosphere thereby engendered brought so generally in contact with the surface, the violence of the reaction is most certainly diminished by the collapse being so soon cut short.

In the reaction from the the asphyxia of drowning, the aphorism of Hippocrates, "Ubi irritatio ibi fluxus," holds most true, as is abundantly observable from the diversity of symptoms with which it (the reaction) is accompanied; for instance, in a nervous irritable female this stage may be ushered in with violent hysteria or convulsions; in a person with delicate lungs, or where such a predisposition exists, inflammation of the organs themselves may be lighted up. No one, I think, will doubt that by inducing reaction as soon as possible, you the more quickly determine from the lungs and brain the inordinate quantity of blood there stagnant, and thus lessen the tendency to inflammation of particular organs or tissues, which is certainly the thing most to be dreaded.

With the air bath applied, you can, as I said before, regulate the temperature at discretion; but, after reaction is established, you may persist in its use as a stimulant in keeping the circulation to the surface; while, at the same time, you employ the lancet in controlling the volume or frequency of the pulse.

The heated air bath may be employed in other descriptions of cases than those before mentioned; as, for instance, where a person has fallen from a great height, and where the concussion has been communicated, as it were, to the whole frame.

I might go on enlarging on this subject, but fear my paper has already reached an inordinate length. Would any of your readers but give this apparatus a fair trial, they will doubtless arrive at a fair estimate of its merits.

THE LONDON UNIVERSITY AND MEDICAL EDUCATION.

To the Editor of the Medical Gazette.

Sir,

I am glad to find that the subject of medical education, which has been discussed with so much good sense in some of your late numbers, has called forth a legislator of the London University to defend this infant institution. The subject is but a part, though a great one, of a larger question, which must sooner or later be taken up and discussed by men of less confined views than those who have hitherto put themselves forward as guides, and who are not actuated by a hatred of existing institutions, because they think their self-assumed superiority has not free play, owing to the existing laws which regulate the profession of medicine in this country. Several reforms have been proposed, and it was supposed by many that the establishment of a University in this metropolis would solve all the difficulties of this larger question. I agree with you, sir, that it is desirable there should be an institution in this metropolis which should be empowered to confer degrees in medicine upon those who have studied where there are advantages which no other town in this country affords: but is the London University, as at present constituted, likely to remedy any of the evils complained of? Rather, is it not more probable that it will create an addition to the already too many causes of opposing interests? I confess that I never anticipated any other result, because it had its origin in a blind hatred, on the part of its instigators, of existing institutions; was constituted in a most inexcusable ignorance of existing laws; and was at length chartered with the design of securing the parliamentary support of the Dissenters. It is not, therefore, to be wondered that it should have exhibited the confined and limited spirit of

party. Need I do more than refer to the conduct of the Senate, in reference to the question, whether Ecclesiastical History and the Gospels should form a portion of the subjects on which candidates for the degree of B. A. should be examined, and the interference of the Secretary of State, on being prompted by certain Dissenters?

The appearance of a letter, signed "C. L.," of the 11th ult., and the spirit which appears to actuate the writer, who by his tone betrays his office of legislator in the University, has indneed me to consider well the regulations he defends. He appears to me, however, to mistake the proper end to be sought in framing a course of education for young men before they commence any *professional* studies, in his allusion to the system pursued at Oxford and Cambridge, and ignorant of the evils incidental to the existing regulations to which students are obliged to conform. He seems to think, that after young men have passed a preliminary examination on subjects which are to include "more than what has been usually required at Oxford or Cambridge," but not "more stringent than will be readily passed by young men of 17 or 18, who have been educated properly;" and after their minds, thus trained, have been well crammed with the knowledge of opinions to be gathered by an assiduous attendance, during four years, upon lectures on a dozen or fourteen subjects (while only the *principal part* of *two*, out of these four years, is to be devoted to *practical* instruction), the London University will acquire for its degree in the persons of physicians *thus made*, the repute of being "a highly respectable one." What is the system, after all, but a copy of the regulations of some of the Scotch Universities, in which the *professional* system is exalted, and the practical thought of only as subordinate. It is the same mistaken view of education which dictated the recognizing of King's and University College in London: the former has no hospital attached to it, and the latter the smallest in this town; while the schools attached to Bartholomew's, the Borough, St. George's, the London, and the Middlesex hospitals, *i. e.* hospitals where there are the best opportunities of gaining sound medical education, are not recognized. The consequence will be, that the generality of young men thus educated will be fitter to become indifferent medical critics and makers of books than physicians and surgeons. When I find the Senate of the London University promulgating regulations which will make it imperative upon those who may wish to be candidates for its degree, to study only where the medical school is connected with a large hospital, and where *the teachers are the physicians and surgeons of that hospital*, or *appointed by them*, then only shall I begin to think that the interests of the public, and not of party, guide its deliberations; then the main end of all medical education to rear up sound *practitioners*, not *speculative teachers*, will be recognised; and then Manchester, Liverpool, Birmingham, Leeds, Newcastle, Bristol, &c. may furnish candidates for the degree of the London University.

I would recommend to C. L. and his brother senators, the perusal of your articles on the necessity of lectures being practical, and Mr. Whewell's book on the Principles of English University Education, which probably suggested them. It is clear to me that C. L. has little or no intercourse with medical students, else he would have seen that your suggestions are more calculated to obviate the evils they are subjected to in their endeavours to educate themselves, than the curriculum he defends.

No one can have failed to observe how fruitful a source of *trading* in lecturing the opposite system to that which I would call the *practical* or Hospital system, leads*. Young physicians, who are panting for fame and private practice, not contented with writing before they have had means of gaining experience, an octavo, it may be, on diseases of the lungs or heart, for the instruction of their more experienced brethren, and, perhaps, their teachers; but finding that their merits do not receive that attention which, in their own opinion, they should, adopt this other mode of keeping themselves before the public, and thus endeavour to impress that public with the idea of their experience,

* May I call your attention to the following advertisement which appears on the wrapper of your number for this week:—"To Lecturers.—Gentlemen qualified to teach anatomy, materia medica, botany, comparative anatomy, medical jurisprudence, or the practice of physic, may hear of an opportunity of lecturing at a medical school in London, on application, with a statement of their qualifications, by letter (post paid) to," &c.

by joining some school as lecturer on the principles and practice of physic, or on a class of diseases. I will make no comment upon the want of correct moral feeling which such conduct betrays, whether in an author or a lecturer—they have their reward ; but, unhappily, a serious evil, so often inflicted on the unsuspecting student, who is tempted, perhaps, by the diminution of the lecturer's fees to become his pupil, and thus to learn the errors of his speculative master. Or if the fees are equally high, and either through private friendship or the great talents of one teacher, a young student is induced to enter as a pupil at a school, which is not an hospital school, he finds himself more or less involved in the quarrels in which that school had its origin. Is it not, then, time for those who are anxious for the honour of our profession, to do all in their power to remedy, by well-contrived regulations, the defects which at present exist, and prevent students being subjected to the evils I have pointed out?

I am not wedded to any Utopian scheme, like the uneasy reformers of the present day, who are quick in detecting the anomalies, but overlook the manifest advantages of our existing institutions. It will be readily conceded that our large hospitals afford the best advantages for medical education, and in connexion with them might be framed such a system of regulations as on the whole would best answer the end in view—the making good physicians and surgeons. You may well conceive that I equally disapprove of the newly-issued regulations of the College of Physicians and those of the Society of Apothecaries.

I have already said, that the question which you have discussed is part of a larger one, but having trespassed too much on the limits which should be assigned to a letter, I have no intention to discuss at present the whole question. Whenever the laws regulating the practice of medicine and the vending of drugs in this country shall be revised, it must be settled in what governing body should be vested the power to grant permission to practise the healing art, and with what body that of giving permission to vend drugs. To the former governing body I would suggest, that the object of the regulations to be complied with by those who seek for their license should be, to secure the diligent attendance of medical students at large hospitals during a given number of years—at least four—not to multiply the subjects of lectures ; for the details may be left for the regulation of those who ought to direct the studies of the pupils at these hospitals. General and morbid anatomy might then be learnt under the greatest advantages, so as to render it of practical usefulness ; physiology and pathology would be taught in the wards of these hospitals, and the pupils would be taught therapeutics at the same time; chemistry and pharmacy, the appearance and properties or drugs, and the best mode of dispensing them, would be better learnt in the laboratory and dispensing room of these hospitals. What lectures, and the order in which the different branches of study should be followed, might be left to the directors of these schools. I do not forget that midwifery is a necessary branch. I presume that the teachers in these schools would take care that their pupils should learn the management of women in childbed, and of their infants. It will be objected, that I lower the standard of medical education by leaving out subjects that are included in certain curricula to which I have referred. My object is rather to sketch the outline of a system which would give medical education a more practical turn, and afford the very best foundation for higher scientific attainments to those who wished to gain them.

Should these remarks have any value, and, consequently, gain admission into your journal, I may extend them in another letter.—I am, sir,

Your obedient servant,

M. D.

Sept. 1, 1838.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Microscopic Illustrations of Living Objects, their Natural History, &c. With Researches concerning the most eligible Methods of constructing Microscopes, and Instructions for using them. By C. R. GORING, M.D. A New Edition, emended and enlarged. By ANDREW PRITCHARD, M.R.I. &c. 8vo. London, 1838.

THE wonders revealed by the microscope are scarcely, if at all, inferior to

those displayed by the telescope: the latter carries us into distant worlds, the former opens a new world around us; every atom of animate and inanimate nature teeming with interest. For a long period, however, the telescope was the more perfect instrument of the two, owing to the achromatic principle having been so soon adapted to it, and with such complete success. Dr. Goring, however, recently succeeded in applying the achromatic corrections, as well as those for sphericity, to the microscope; and the result has been most satisfactory; leading to important results in the various branches of natural science, more especially in animal and vegetable physiology.

In the work before us, every kind of information connected with the subject is detailed in a manner simple and easily understood, but at the same time perfectly scientific and sufficiently minute. Plans and diagrams are given, by which the construction and principles of the various forms of microscope are demonstrated; and we strongly recommend it either to the man of science, who wishes to employ this instrument in enabling him to penetrate somewhat deeper into nature's secrets, or to the amateur, who seeks to gratify himself by the contemplation of some of those objects of surpassing beauty which this really wonderful instrument displays to him.

MEDICAL GAZETTE.

Saturday, September 22, 1838.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

RECENT EVIDENCE ON THE MEDICAL RELIEF OF THE POOR.

ONE of the most important questions asked by the Select Committee, was the one put to Dr. Kay, concerning the arrangements adopted relative to midwifery patients. It was doubly important, both from the weightiness of the subject itself and from the view of the future policy of the Triumvirate disclosed in the answer. Dr. Kay states, that in Norfolk and Suffolk the medical men attend such cases of midwifery as the Board of Guardians may require, but that the demand is seldom made. In one or two unions, he believes that the common midwives of the district have been paid for attendance, but he adds—and here the cloven foot of Malthusianism peeps out without a covering—"it did appear to me to be that kind of medical attendance which ought to be earliest got rid of."—(Reports, &c. p. 8.)

The *earliest* got rid of! And the other kinds are to follow a little later, we suppose—a couple of years or so, after the serfs have been taught to save up midwifery fees out of a shilling a day. Hints have been given, too, that diet-tables err on the side of plenty, and are so toothsome that they must tend to decoy the idler hinds from their work, and make them bury themselves in the *otium cum dignitate* of a Union Workhouse. May we not expect, therefore, to see Dr. Kay issue the prescription *Cont. Decoct. Avenæ dilutissimum, omissâ avenâ?* This diligent series of attempts to make the poor live upon less and less—to make those whose income is brought down by competition to a minimum insufficient to procure food and raiment, provide for medical advice out of their slender means—is not altogether without a parallel. There is a well-known story in the books, that, once upon a time, an economist did his best to teach his mare to live without food. From day to day he went on with his useful lesson. Like the Poor-law Commissioners, he taught her to live on coarser food; he cut off all the luxuries of hay and oats, and reduced her to meaner fodder; but, as he afterwards complained to his friends, just as he had brought her to live on one straw a-day, the poor thing died. A nation does not die, but its patience sometimes comes to an untimely end.

Dr. Kay says that the reason that obstetrical attendance ought to be the soonest got rid of, is that the parties have the necessity of providing it in view for several months—as if the certainty of an approaching evil was always a sufficient guard against it. In the bad old times of Charles II., one of the tortures practised by the King's troops upon the Cameronians in Scotland, was to tie them to a stake on the sea-shore, between high and low water-mark, so that when the tide rose they were drowned by the advancing waves. This is not unlike the situation of an indigent man who sees time coming on, and with it a liability for which he has no means of providing. He might, indeed, provide for it by renouncing the instinct which compels every man to obtain meat and drink for himself and his family—just as the Scottish Covenanters might have escaped had they chosen to renounce their creed.

At pp. 10-11, there are several curious questions and answers developing another feature of the famishing system. It seems that the boards are sometimes displeased with medical officers for ordering what their High Mightinesses suppose to be too much meat, porter, or wine, for the pauper patients; and Dr. Kay acknowledges, that if the guardians and the medical attendants cannot agree upon this point, new hands must be procured from London or from some distant district. One of the questions is singular enough. Mr. Barneby wants to know whether, if the medical man does not attend to the suggestions of the guardians on this subject, Dr. Kay would wish to "have an order passed, that the medical man should attend the Board of Guardians always after he had given orders of that kind?" (Page 11.)

If there is a necessity for such orders, as there unquestionably must be every day, this would be a troublesome part of his duty. Imagine explaining the utility of each scrag of mutton, each pot of porter, each counteractive of the diseases produced by starvation, to the very Board that has just reduced a widow's pension from 3s. 6d. to 2s. a week. How they would be startled if you told them that the medicines for the poor, according to Rousseau, are to be found in the kitchens and cellars of the rich; or that it cannot be doubted for a moment that the disorders of labouring men proceed in a great measure from want of food. Mr. Ceely, a most intelligent witness, when asked what was the condition of the working people where his practice extended, (in and near Aylesbury) answered:—

"Their circumstances are bad, and their physical condition is bad; their intellectual endowments are small; they are a poor sickly set of beings, which is partly attributable to the soil, and partly to their insufficient diet and their general depressed physical condition, from their low rate of wages." (P. 63.)

On one occasion Mr. Ceely attended a parish in which an epidemic prevailed, and it cost him 10*l.* out of his own pocket to furnish wine and beef-tea for the patients; the cases being so numerous and so distressing at the time, that he could not withhold his assistance. (P. 67.)

In this instance Mr. Ceely was anxious to get his patients well as soon as he could; but how many cases must there be, where the practitioner sees the necessity of giving wine and generous food, but is afraid of the Board of Guardians, and unable to act like the witness just quoted.

After Dr. Kay, the next witness examined was Mr. H. W. Rumsey, of Chesham, whose evidence is certainly the most instructive among all the witnesses examined before this Committee; and the spirited and manly tone in

which his information is given is worthy of all praise.

The Provincial Medical Association in the year 1835 appointed a committee of its members to consider the best means of affording relief to the sick poor, especially with reference to the working of the New Poor-law. Mr. Rumsey was the secretary of the committee, and was engaged for a year and a half in making the necessary inquiries. He found in many instances that medical men were unwilling to communicate the facts which had come to their knowledge; and this fear of giving offence to the authorities was found even among those who did not hold official appointments. The reason he gives is the natural one, that the doctors are afraid of the consequences of opposing their best customers; "the boards of guardians being composed of the principal and most respectable inhabitants in the country." (P. 25.) Alas! for the changes of language! *Respectable* once meant *worthy of respect,* but now too often merely signifies *rich.* On Thurtell's trial a witness having stated that his victim, Weare, was respectable, was asked, "what do you mean by respectable?" To which the answer was, "He kept a gig." The respectability of guardians, however, is probably many degrees above that of Mr. Weare, and may often rise to a close carriage, or perhaps two; in the latter case we would suggest that "respectable" should be eschewed as too shabby a word, and "noble" or "august" be substituted. The witness, however, when questioned as to the system of plural voting, said he thought it tended to produce the election of the most respectable guardians; on which the following questions and answers ensued:—

"Do you mean the most wealthy? I would not say the most wealthy only, but men of intelligence.

"Then in your union you have no complaint to make of the medical arrangements?—Yes, I have; I do not find that intelligent men being elected prevents abuses in the medical arrangements.

"You do not find that the most intelligent are the most benevolent?—I have many friends among the guardians, and I should be sorry to be pushed upon that point."—(P. 31.)

The benevolent Mr. Smith, of Southam, in a pamphlet on Alfred Societies, speaks of the progress of anti-social feeling "which has begun in a 'reign of terror' among the labouring classes;" but it appears too clearly from Mr. Rumsey's evidence, and from the report published by the Provincial committee, that this reign of terror is by no means confined to working men. Country surgeons are under its leaden sway, and dare not open their lips for fear of the "venerable" and the "august." Thus at p. 2—3 of the Provincial Report we find a note from a practitioner living under the reign of terror, and crying out, "The vice-chairman of the Union is a most excellent patient of mine, and thinks very highly of the new act, *and perhaps that statement might displease him;* if it did, the mischief might not end there; *he could influence others.*"

In the Rugby Union the assistant-commissioner reproved a practitioner present at the Board of Guardians for having had any thing to do with the proceedings of a medical meeting held at Warwick to petition parliament against the present system of medical relief. On hearing of this reproof, a practitioner in the neighbourhood became so terrified that he begged to be allowed to withdraw his name from the Warwick petition; and others objected to sign it for the same reason.—(*Ibid.* p. 65.)

In fact this *guardianophobia*, or fear of being supposed to advocate truth, and not the Poor-law, is so widely extended, that a particular mark has been adopted

in the pamphlet just quoted to designate those who are under its influence; an obelus points out that the statement was received from some one who has requested that his name may not transpire.

If fifty practitioners could be found to speak out with the masculine frankness and true English feeling of Mr. Rumsey, we should consider the good cause as won.

Some of the other chief points in his evidence before the Select Committee are:—

1. The extreme reduction of the number of medical officers in some unions, as in the Aylesbury, from 16 to 3; in the Banbury, from 14 to 3; in the Witney, from 8 to 2; and in a number of others, making a total of 109 reduced to 27.—(Reports, &c. p. 28.)

2. His exposure of the totally erroneous statement of the Poor-law Commissioners, that in forming the medical districts the best arrangement has been adopted in almost every case.—(Reports, &c. p. 29).

3. The reduction of medical salaries under the new system—(p. 31); the remuneration having been already inadequate under the old.—(P. 30.)

4. A list of unions where the lowest tender has been received in opposition to character, personal qualification, and residence.—(P. 36.)

5. He thinks that in some unions the remuneration is very fair, particularly in those of Norfolk and Suffolk,—(p. 36.) Yet how can this be? We have already shewn in our last article, that in those counties the care of a district of 3,000 souls is rewarded with about 40*l.* or 50*l.* per annum.

At. p. 41, however, he thinks that the remuneration should amount to about 6s. 6d. per case; not that the practitioner should be paid by the case, but that he should receive such a salary as would amount to about this sum for each patient. Apply this gauge to the Norfolk and Suffolk salaries, and it will appear that they are anything but fair or adequate, unless we suppose a district to supply only 120 or 150 cases per annum, which is quite out of the question. The evidence of the other witnesses we must pass over, at any rate for the present. They consisted of Sir Astley Cooper, Drs. Elliotson, Marshall Hall, R. D. Thomson, and Webster, and Messrs. Rowe, Farr, Evans, and Toogood. There is much, however, worth reading in their evidence, particularly in the statistical information of Mr. Farr, and the account of the adulteration of medicines by Dr. R. D. Thomson. It must be confessed, that to a truly reasonable person the chief inquiry of the Select Committee would seem to be a work of supererogation. We smile when we read that Sir Hudibras found out by algebra what o'clock it was, and,

> "Could tell by sines and tangents straight,
> If bread or butter wanted weight;"

and perhaps at some future period, when the heats of party have subsided, it will excite surprise to hear that it required so operose a process to discover that business, to be properly performed, must be properly paid for.

MEDICAL VERSES.

We learn from the *Gazette des Hôpitaux*, that a young French physician, pseudonymously calling himself Dr. Prosper Viro, has published a dissertation in verse on acute rheumatism of the joints. The versification is easy and agreeable, says our contemporary, who cites the following lines as a favourable specimen:—

> "Il est là (le rhumatisant) frémissant sous la main qui le touche,
> Et son pouls saccadé, ferme, rebondissant,
> Soulève votre doigt sous sa tige de sang."
>
> "Quivering he lies beneath the touching hand,
> While his firm, jerking, and rebounding pulse
> Lifts up your finger with its stalk of blood."

The author defends M. Bouillaud's method of practice with great warmth; and not only gives the causes, treatment, and diagnosis of the disease, but has suc-

ceeded in narrating in verse two entire cases of rheumatism. The reviewer in the *Gazette des Hôpitaux* does not wish to give an opinion on the medical controversy, but is rather inclined to adopt a *sage réserve*, and answer with the author's verse:—

" La Charité dit oui, mais l'Hôtel-Dieu dit non."

By the by, would it not be a capital speculation to draw up a few manuals for young gentlemen preparing for examination at certain terrific halls and colleges, either in English doggrel or Latin verse, as might best suit the previous mnemonic habits of the tyro? Could not the cerebral nerves, for instance, be hitched into longs and shorts, something in the following style?

Paria Nervorum.

Primum nasus habet; dat cuncta videre secundum;
Luminaque huc illuc tertium in orbe movet.
Agnoscit quartum trochlearis sæpe tyrannum;
Quintum amat innumeras ire, redire, vias;
Nam frontem et lachrymas ophthalmica portio, nasumque,
At maxillaris corrigit ora viri.
&c. &c.

GLASGOW EYE INFIRMARY.

Clinical Lectures by Dr. Mackenzie.

Ophthalmitis Phlegmonosa, or Ocular Phlegmon.

[Concluded from p. 992.]

Case III.—*Scarlatina.* — *Typhus Fever.* — *Phlegmasia Dolens.*—*Phlegmonous Ophthalmitis of both Eyes.*—*Death.*

Stewart Bell, a weaver, aged 23 years, was admitted into the Glasgow Fever Hospital, on the 14th of May, 1836, for scarlatina, and was dismissed cured in the course of a few days. He was re-admitted on the 1st of June. On his re-admission he complained of acute pain in the left thigh and leg, much increased by pressure on the inner part of the thigh. Both the leg and thigh were swollen, but of their natural colour. The left arm was rigid, but not swollen, and pressure or motion produced but slight uneasiness. He complained of pain in the joints. Pulse 112, pretty firm; tongue furred, moist; bowels loose. It was the impression of Dr. Cowan, whose patient he was, that he was affected with phlegmasia dolens, or phlebitis. He had had several rigors on the 24th May, followed by headache, heat of skin, and urgent thirst; and on the 25th the affection of the limbs commenced.

He was ordered ℥ss. of Castor-oil, with 25 drops of Laudanum; gr. iss. of Opium at bedtime; and 12 leeches to the thigh.

2d.—Considerably relieved by the leeching. Urine drawn off by the catheter.

Thrice a-day two grains of Calomel, with five grains of Dover's Powder.

5th. — Typhoid eruption. Delirium. Œdema of the eyelids to a great degree. Pulse 100; tongue brown and dry.

7th.—Delirium continues. Pulse 100; tongue much loaded; bowels slow.

Six grains of Calomel, with 12 of Jalap.

8th.—Pulse 120. Powders continued.

9th.—Rigidity of both arms. Delirium continues. Pulse 120; eyes much swollen and protruding.

Twelve leeches to head. Powders continued.

10th.—Sinking. Pulse 120; features sharp. During this day, deposition of purulent matter was discovered in both eyes. Died in the evening.

11th. — *Inspection:* Dura mater thickened, and in the course of the longitudinal sinus a small, rough, irregularly-shaped, ossified body, about the size of a split pea, was discovered. Brain softer than natural. Lungs engorged, and bronchi reddened. No disease in stomach or intestines. No trace of pus in the veins. The eye-balls, which had previously been much protruded, had sunk nearly to their ordinary place in the orbits.

Left eye.—The cellular and adipose tissue surrounding the eye-ball, was indurated, swelled, and infiltrated with serum. The cornea transparent. The sclerotic reddish, soft, and thickened, especially around the entrance of the optic nerve. The anterior chamber contained a reddish fluid, with some white flaky matter. The iris was thin and soft; it had contracted no adhesions. The choroid coat adhered more firmly than usual to the sclerotic; its inner surface presented some reddish spots, interspersed with white patches, apparently of lymph, which could not be detached from it, and seemed incorporated with its substance. The ciliary body was not materially changed. The pigment was in some places entirely deficient, and in others the remains of it could be easily separated from the surface: no trace of it was found on the posterior surface of the iris, and the slight shreds of it which remained on the ciliary processes were readily removed, so as to leave that structure of a uniform white colour.

The choroid coat was separated from the retina by a considerable quantity of reddish fluid, with whitish puriform flakes, from the entrance of the optic nerves forwards to the zonula of Zinn. It flowed out on penetrating the choroid.

The retina, thus detached from the choroid, was firmly united to the hyaloid

membrane, and anteriorly to the zonula Zinnii; so that these textures formed a whitish thickened mass, inclosing a small quantity of a reddish fluid, the remains of the vitreous humour. The membranous laminæ of the vitreous body had entirely disappeared towards its centre. The adhesion and thickening of the retina, zonula Zinnii, and hyaloid, seemed to be from effusion of coagulating lymph. It was most remarkable at the zonula Zinnii, and greater near the entrance of the optic nerve than at the equator of the eye.

The lens and both layers of its capsule were transparent.

Right eye.—The appearanees were exactly similar, with the following exceptions:—1st. There was a thin lamina of lymph lying in the lower part of the anterior chamber, parallel to the iris, and extending up to the pupillary margin. 2dly. A zone of the retina and hyaloid membrane in the equator of the eye was so slightly thickened, and so transparent, that the reddish fluid in the centre of the vitreous body was distinctly seen through it, when the light was transmitted through the lens. 3dly. A slight uniform layer of pigment remained on the posterior smooth zone of the ciliary body, and a very distinct layer on the posterior surface of the iris.

Here is the preparation of the eyes, the dissection of which was drawn up by Dr. Rainy; and this is a wax model of the left one, by Dr. Paterson.

Phlebitis, attendant on typhus fever, is no doubt a rare event; but I understand, from Dr. Cowan, that he has witnessed several cases of it. It generally shews itself about the period of convalescence, and not early in the disease, as it did in Bell. For example, one patient, a man of 25 years of age, was pronounced convalescent on the 15th of May, had rigors on the 24th, and died on the 29th. Pus was found in the left femoral vein. In two cases, besides that of Bell, Dr. Cowan saw affections of the eyes, probably the result of phlebitis. In the one case, the corneæ ulcerated on the day of the patient's death; in the other, there was purulent deposition and ulceration of the corneæ, on the 19th day of the fever.

CASE IV.—*Phlegmonous Ophthalmitis of both Eyes—Death.*

Daniel Maclellan (No. 7104), aged 35 years, admitted into the Glasgow Eye Infirmary, under the care of Dr. Rainy, on the 7th of June, 1836. The bulbs of both eyes are of an intense red, much swollen, and projecting far out of the sockets, but still covered with the upper eyelids. The whole cellular membrane is œdematous; the lower eyelids swollen and everted. The irides have assumed a greenish hue; the pupils are irregular, and are not affected with light. The right pupil is occupied by an opaque lens; the left presents a hazy whiteness. Has no perception of light nor shade with either eye. Thinks he has a very slight perception with left eye. This disease commenced about a fortnight ago, after an attack of severe pain in side of chest, for which he was bled profusely. Pain in the eyes has been frequently severe, and is still occasionally troublesome above the orbits. Pulse 116, soft; bowels rather bound. Has been treated with leeching, blisters, scarifications, purgatives, and calomel and opium. Eight leeches to each eye; eight grains of calomel and eight of aloes.

10th.—Swelling of left eye greatly diminished, and vision improved. Swelling of right continues.

Ten leeches to right eye.

12th.—Right eye still much inflamed. Leeches repeated to right eye.

A Calomel and Opium pill (2 gr. and 1 gr.) morning and evening.

13th.—The lids were carefully brought together by plaisters, and a compress and bandage applied over the eyes. The swelling of both eyes is much diminished to-day.

Solution of Nitrate of Silver, 4 gr. to 1 oz. Three Calomel and Opium pills daily.

15th.—Mouth a little sore.

Two pills daily.

16th.—Head to be shaved. Blisters behind ears.

Twelve drops of Vinum Colchici at bedtime.

18th.—Complains of pain in left eye.

Two leeches to left upper eyelid.

22d.—Pain in the left eye was relieved on the 20th. On the morning of the 21st, suddenly became insensible, and expired.

Inspection.—The pia mater was rather more vascular than usual. A considerable quantity of serum under the arachnoid, and at the base of the brain.

Right eye.—The orbital cellular tissue was infiltrated with serum; it was indurated, and contained a collection of purulent matter, which projected under the conjunctiva, and communicated, by an opening on the under surface of the sclerotica, with the interior of the eye. This is distinctly seen in the preparation here before you. The sclerotica was greatly thickened, and firmly adherent to the indurated cellular tissue around the entrance of the optic nerve. The choroid adhered firmly by its outer surface. The inner surface of the choroid presented traces of fibrinous effu-

sion, and was completely detached from the retina by a collection of purulent matter, which also extended into the cells of the vitreous body, and communicated with the external abscess through the opening in the sclerotica already mentioned. The retina and hyaloid membrane adhered firmly, and formed a whitish thickened mass, inclosing the infiltrated cells of the vitreous body, but presenting apertures of communication at various points. The lens was transparent; the iris was adherent to the capsule; some blood was effused in the anterior chamber, with some traces of lymph.

Left eye. — The cellular tissue around this eye was also indurated, infiltrated with serum, and closely adherent to the sclerotica, which was much thickened, especially at entrance of optic nerve. The choroid adhered firmly to the inner surface of the sclerotica. Very little fluid was found interposed between the choroid and retina. On turning back the choroid, the retina, hyaloid membrane, and vitreous body, were found united into a firm yellowish mass, having much the appearance of coagulable lymph, and with scarcely any traces of purulent matter. The ciliary body was of a dark red colour; the lens and its capsule transparent; a slight effusion of blood in anterior chamber.

Both lungs adhered at several points to the pleura costalis, but they were chiefly cellular adhesions. The left lung was collapsed to one-third of its ordinary size. A fibrinous concretion was found in the left ventricle of the heart, but not firmly adherent to its inner surface.

Here are the eyes, prepared by Dr. Rainy; and here is one of them accurately modelled by Dr. Paterson.

Maclellan was a strong muscular man. As is noted in the case, he had had pleurisy some weeks before he was seized with the affection of his eyes, and had been bled for it. Whether he had any inflammation of the vein which had been opened could not be learnt. The medical gentleman who first saw him, when he began to complain of his eyes, considered the symptoms as indicative of inflammation of the brain. The patient complained of severe pain in his head; he was bled for this, and had calomel and opium. Beer mentions (*Leitfaden*, I. 424) that he had frequently seen this disease mistaken for cephalic fever. Maclellan was also seen by Dr. William Brown, before being admitted here. Dr. B. saw him ten or fourteen days after the commencement of the ophthalmic disease. Both pupils were then widely dilated, the one more than the other, and immovcable. The retinæ were insensible to a lighted candle. The pain had subsided. There was a considerable eversion of the lower eye-lids. Dr. B. proposed puncturing between the lower eye-lid and the eye-ball, on the presumption that matter was effused there.

You are well acquainted with the fact, that phlebitis is apt to be followed by what have been termed *purulent deposits*, in different parts of the body, and that these have generally been ascribed to the circulation of pus. The notion that the matter found in the lungs, liver, brain, cellular membrane, and other parts, in cases of phlebitis, is actually that which has been carried through the circulation from the vein originally injured or inflamed, is now almost universally abandoned. The matter found in the organs secondarily inflamed is doubtless secreted in these organs themselves; and according to Professor Cruveilhier (Anatomie Pathologique, I. livraison xi. Dictionnaire de Médecine et de Chirurgie Pratiques, art. *Phlebite*), and Mr. Douglas (Inaugural Essay on Phlebitis; Glasgow, 1835), it is probably from inflammation of the veins of these parts that the secondary affections arise. Far from regarding any mere deposit of pus from the general circulation as the efficient cause of the secondary abscesses which occur in consequence of phlebitis, it is the opinion of both these inquirers that the circulating pus acts only by irritating the coats of the capillary veins in different organs of the body, and exciting them to inflame; and that these inflamed veins induce a circumscribed suppuration around themselves. This is not a conjectural opinion, but one founded on various and accurate observations.

To apply these views to the eye (for I must here differ a little from Mr. Douglas, who seems inclined to consider the disease of the eye, which supervenes to phlebitis, as a mere sympathetic affection, and not attended by phlebitis in its own tissues), we cannot believe that the same pus which has passed from the uterine veins, or from the veins of the extremities, in cases of phlebitis, is deposited between the choroid and retina, or in the aqueous chambers of the eye, but that the textures in the neighbourhood of which pus is found in secondary phlebitic ophthalmitis have themselves been excited to inflammation; and it is, according to our present knowledge of the subject, the most probable supposition, that the minutest veins of these textures (the vasa vorticosa of the choroid, for instance) are the parts in which the circulating pus is first arrested, and in which the irritation and secondary inflammation are first produced.

The effects of the inflammation of a vein may be distinguished into primary and secondary, local and remote. We must not confound these several effects. Take,

as an example, suppurative inflammation of the veins of the uterus after parturition. The *primary* effects are, increased thickness of the coats of the affected veins, false membranes on their internal surface, gradual coagulation of their contents, deposition of pus in their cavities, and occasionally destruction of their whole texture. The *secondary* effects are abscess in the liver, purulent collections in the chest, inflammation and ulceration of the synovial membranes, and phlegmonous ophthalmitis. The effects just now enumerated are also *remote*, while the swelled leg, or phlegmasia dolens, is an example of a *local* effect. It is a consequence merely of the obstructed state of the iliac and femoral veins, produced by the original inflammation extending into these vessels. It would be incorrect, then, to speak of phlegmasia dolens of the eye as some have done, meaning thereby the secondary and remote inflammation of that organ, arising from the circulation of purulent matter.

The constitutional symptoms which attend any secondary phlebitic disease, such as ophthalmitis, are generally of a typhoid description; rigors, prostration of strength, rapid feeble pulse, low wandering delirium, attacks of vomiting and purging, and a brown parched tongue. Hence the difficulty of the cure, and the little good effected by depletory measures.

AMESBURY'S SPLINTS.

To the Editor of the Medical Gazette.

Sir,

In answer to Mr. Keyser's note of the 27th ultimo, stating that there is no such splint as Mr. Amesbury's employed at the North London Hospital, I can only observe, that my engraver, Mr. Linton, whose word I have no reason to doubt, assured me that he copied it from one actually in use at that institution. I am fully aware that the splint in question is there called Mr. Liston's splint; but as it in nowise differs from Mr. Amesbury's in principle, I did not consider it necessary to take any special notice of it.—I am, sir,

Your obedient servant,

Henry Hancock.

59, Harley Street,
12th September, 1838.

CHINESE EYE INFIRMARY.

From a communication, recently received from Canton, the following gratifying facts have been gathered:—The Eye Infirmary has evidently been gaining favour with both natives and foreigners, ever since its establishment. The expenses, since its erection last season, have been not far from 1200 dollars, and the contributions by strangers have exceeded that sum by about 300 dollars. The entire number of pa ients, up to Sept. 8th, 1836, was 1,912. So great is the press of business that vast numbers are obliged to go away without being operated upon, owing to the impossibility of having every thing done by one single oculist—who is without an assistant.—*Philadelphia Med. Exam.*

BOOKS RECEIVED FOR REVIEW.

Anatomical Tables; containing concise Descriptions of the Muscles, Ligaments, Fasciæ, Blood-vessels, and Nerves. Intended for the Use of Students. By Thomas Nunneley, Lecturer on Anatomy in the Leeds School of Medicine. Highley.

Chemistry of Organic Bodies. By Thomas Thomson, M.D., Regius Professor of Chemistry in the University of Glasgow, &c. &c. &c. pp. 1076. Bailliere.

The Physiognomy of Mental Diseases. By Sir Alexander Morison, M.D. No 5, containing—Monomania with Paralysis.

Medical Portrait Gallery. Part 7, containing Dr. W. Hunter, Dr. Jenner, and Dr. Baron. Fisher.

A Manual of Diseases of the Eye; or, Treatise on Ophthalmology. By S. Littell, M.D., of Philadelphia. Revised and enlarged by Hugh Houlston, M.R.C.S. Churchill.

A General Outline of the Animal Kingdom. Part 1. By Thomas Rymer Jones, F.Z.S., Professor of Comparative Anatomy in King's College, London.

A Treatise on Neuralgia. By Richard Rowland, M.D., Member of the Royal College of Physicians, Physician to the City Dispensary. Highley.

Analytic Tables, adapted to the Purposes of General and Forensic Medicine, from the French of Professor Devergie. By Martin N. Lynch, M.D. M.R.C.S.E. Longman and Co.

WEEKLY ACCOUNT of BURIALS.

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